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> "*To the solid ground*
> *Of Nature trusts the mind that builds for aye.*"—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, MAY 1, 1873

THE WILD BIRDS PROTECTION ACT

"SAVE me from my foolish friends," ought to be a stave in the spring-song of each fowl of the air, from the Nightingale which warbleth in darkness to the Dotterel which basketh at noonday. Last year, as is well known, a bill for the protection of "Wild Fowl" was brought into Parliament at the instance of the "Close-time" Committee of the British Association,[*] and the various changes and chances which befell it before it became an Act were succinctly recounted in the Committee's report at the Brighton meeting, printed in NATURE, vol. vi. p. 363.

This bill, as at first prepared and introduced to the House of Commons, was framed entirely on the Sea-birds Preservation Act, which became law in 1869, and only differed from that successful measure where difference was needed, and the penalties and procedure it proposed were the same as those which have proved to be so thoroughly efficient in the former case. The minute care, the practical knowledge, and the consideration of various interests with which it was originally drawn, may be gathered from a few facts. Many of the birds it intended to protect are known in various parts of the country by various names, and accordingly all these names were introduced, for it was clear to the promoters of the bill, though not, as shown by the sequel, to the public at large, that a man summoned for killing (let us say) a Lapwing would never be convicted if he brought, as he easily might bring, credible witnesses who in good faith swore that it was a Peewit, and that they never heard it called anything else. At the same time, that the measure might not be needlessly severe, care was taken that of those species which bear different names in Scotland and England and do not breed in the latter, they should only appear under the name by which they are known in the former. A few species too, though coming strictly under the category of "Wild Fowl," were omitted because of their making themselves obnoxious to farmers. But the great feature of the bill was its being directed to a definite point—the preservation during the breeding season of those birds which, beyond all others, were and are subjected to cruel persecution at that time of year—thousands of Wild Ducks, Plovers, and Snipes, being constantly to be found in the poulterers' shops throughout the spring months, not only killed while they are breeding, but killed, it is not too much to say, because they are breeding, since during that season they put off much of their natural shyness and fall easy victims to the professional gunners. Furthermore, all who really know anything of birds know that it is just these kinds which are most rapidly diminishing in number—some of them, which in bygone days were most abundant, are now only seen as stray visitors. There is, for example, the Avocet, the disappearance of which can be plainly traced to its destruction by gunners,[*] and had we space we could cite many similar cases. Then too, nearly all these birds are of no small importance as an article of food, and their supply to our markets has produced a trade of considerable extent.

Now, on the other hand, there are a good many enthusiastic persons, of whom we desire to speak with all respect, who have long been under the belief that in this country the number of birds generally, and of small birds in particular, has been gradually diminishing, and these persons wished for a much wider extension of the principle of protection than seemed to the "Close-time" Committee necessary or expedient. Whether their zeal is according to knowledge may be judged from what we have further to relate, but it is very plain that they disregard the widespread belief in the mischief popularly supposed to be caused by many of even our most useful small birds, and the fact, which no observer of experience can deny, that under certain circumstances, certain birds do a very considerable amount of harm—witness Song-thrushes and Blackbirds in the strawberry-beds—as well as that it is only careful observation which will convince an unprejudiced man that the harm so done is outweighed by the general good. Further, too, these persons overlook the impossibility of making people change their opinions by Act of Parliament, and it could be only when they become better acquainted with the great truths of natural history, that the desired results would follow. An attempt to force public opinion in this country generally fails.

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[*] This Committee in 1871-72 consisted of Mr Barnes, one of the secretaries of the Association for the Protection of Sea-birds, Mr. Dresser (reporter), Mr. Harting, Prof. Newton, and Canon Tristram, and it may be doubted whether five gentlemen more thoroughly conversant with the subject could have been selected Mr Harland, the other secretary of the Sea-birds' Association, has since been added to their number.

[*] See Stevenson's "Birds of Norfolk," vol ii. p. 237 and following pages.

Now this being the state of things when the "Wild Fowl Protection Bill" was introduced by Mr. Johnston, the enthusiasts at once tried to make it meet their ends. The history of the bill being, as we have said, accessible to our readers, there is no need for us to enter upon details, and we content ourselves by reminding them that, in an almost deserted House, Mr. Auberon Herbert, on the motion for going into committee, succeeded in carrying, by a majority of 20 to 15, an "instruction" to extend the protection accorded under the bill to "Wild Fowl" to other wild birds, and thereupon the spirit of the Bill was entirely changed, and it was converted from the reasonable measure originally contemplated into one of indefinite and general scope. Persons of common sense at once saw that in its new shape it would be impracticable, not to say tyrannical, and notice was speedily given of its rejection. Its introducer, however, contrived to get it referred to a Select Committee, by whom it was still further modified, the objections naturally urged against its sweeping clauses being overcome by limiting its effects to certain birds named in a schedule, while the penalties were diminished. The schedule, it is true, contained the names of all those birds originally included in the Bill, but many others were added, though on what principle some were omitted and others introduced we cannot profess to say. No ornithologist whose opinion could carry the slightest weight appears to have been consulted, and it is needless to say that no ornithologist was among the twenty-three members forming the Select Committee.*

We need not dwell further on historic details. It is now evident that the efforts of the enthusiasts—well intended as they doubtless were—have produced a law which is on all sides admitted to be virtually inoperative, instead of the effective measure which the results of the Sea-birds Act warrant us in believing that the original Bill would have proved. Substantial fines, which would have been resonable enough where professional gunners and poulterers were concerned, would have been manifestly cruel in the case of schoolboys. Accordingly the penalties were, to use the forcible expression we have heard applied, "sweated away" to suit the minor offenders, and the Act is almost a dead letter. Mr. Herbert, on the 21st of June last, laid a cuckoo's egg in the carefully-built nest of the British Association Committee, and the produce is a useless monster—the wonder alike of the learned and the layman, and an awful warning as an example of amateur legislation. The forebodings of the "Close-Time" Committee have proved but too true. In its last Report we read—

"Your Committee cannot look with unmixed favour on this measure. It appears to them to attempt to do too much, and not to provide effectual means of doing it. In their former Reports they have hinted at, if not expressed, the difficulty or impossibility of passing any general measure, which, without being oppressive to any class of persons, should be adequate to the purpose. Further consideration has strengthened their opinion on this point. They fear the new Act, though far from a general measure, will be a very inefficient check to the destruction of those birds, which, from their yearly decreasing numbers, most require protection, its restraining power having been weakened for the sake of protecting a number of birds which do not require protection at all. Your Committee have never succeeded in obtaining any satisfactory evidence, much less any convincing proof, that the numbers of small birds are generally decreasing in this country; on the contrary they believe that from various causes many, if not most, species of small birds are actually on the increase. They are therefore of opinion that an Act of Parliament proposing to promote their preservation is a piece of mistaken legislation, and is mischievous in its effect, since it diverts public attention from those species which, through neglect, indifference, custom, cupidity or prejudice, are suffering a persecution that will, in a few years, ensure their complete extermination."

We believe that this opinion is entirely correct, but our space would not allow us to adduce evidence in support of it. Mr. Herbert has now confessed the inutility of his handy-work, and some time since gave notice of a motion for the appointment of a Committee of the House of Commons to examine witnesses on the question. Before this article appears in print, our readers will know whether he gets what he wants. If he succeeds we suspect that not much good will follow. The eloquence of the enthusiasts is likely to overpower the reason of the true naturalists—a race not prone to sentimentality or given to sensationalism.

We would observe that the destruction of "Wild Fowl" stands on a very different footing from the destruction of "Small Birds," and if either is to be stopped it must be by different means. To check the first we believe no measure can be devised so complete as that which was last year spoilt by Mr. Herbert, but, since his unhappy success has taught Leadenhall Market that an Act of Parliament may be set at nought with impunity, it is quite possible that a new Act to be effectual should absolutely prohibit, within certain days, the possession or sale of the birds to be protected, irrespective of whether they can be proved to have been received from abroad or not. The destruction of "small birds" is chiefly caused by professional bird-catchers, for the numbers killed by the gun is in most cases comparatively trifling. The outcry that would be raised by farmers and market-gardeners, were they hindered from shooting the birds they find rifling their crops, would quickly repeal any Act which Parliament might inconsiderately pass to that effect. But we certainly should have no objection to putting the bird-catchers under some restriction, and we believe it would be to their own advantage if they were restrained from plying their art during the breeding-season. We shall no doubt be condemned by many excellent persons, but we cannot look upon bird-catchers as a class that should not be suffered to exist. The vocation of a bird-catcher may or may not conduce to the practice of all the virtues, but there is no reason for regarding it as essentially and necessarily vicious. Good and bad exist in every trade, bird-catching among the rest. We conceive that Mr. Sweedlepipes had a right to make his living—nay, to be protected in doing so as long as he did not exercise his calling to the detriment of the community. Of course this view will not suit the spasmodic writers of letters to the *Times* and other newspapers with their passionate appeals on behalf of the harmless Hedge-Sparrow and the unappreciated Tomtit. Who is there that systematically persecutes either? Certainly not the bird-catcher even of the blackest dye, begrimed with the soot of Seven Dials or Spitalfields. Are there not just as many Hedge-Sparrows and Tomtits in this country as there is room or food for? Are there not now many more Skylarks and Chaffinches than there were before heaths were broken up and bogs drained, plantations made and "vermin" killed by the game-keepers? But our excellent enthusiasts cannot see this: with them are alike despicable and detestable the gardener who will not believe that the Bullfinch is actuated by the purest and most benevolent motives in nipping off his apple-buds, and the farmer who doubts whether the Sparrow's ravages in his ripening grain are counterbalanced by that saucy bird's services in the cabbage-garden. To them all birds are at all times bent on benefiting the human race. No statement in this direction is too gross for such people to swallow. The last we have met with is one of the most absurd. In the *Quarterly Review* for the present month (p. 402), we read that from some nameless moors the sportsman has been driven by the vipers, and the abundance of the vipers is owing to the extermination of "their natural enemy, the beautiful peregrine falcon"! Such a story is not worth refutation; its original teller has said "that which is not," and the man who gravely repeats it is an idiot or worse.[1]

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* The printed "Proceedings" of the Select Committee do not throw much light on the subject. The schedule was proposed by Mr. Samuelson. On a division the Owl was saved by 14 votes to 4, the Hedge-Sparrow and Whinchat by the casting vote of the chairman, the Thrush was lost by 9 to 6. All the birds added are included only under their book-names, which of course are, as every practical naturalist is aware, very different from those by which they are commonly known.

But now to conclude, we beg leave to offer the following suggestions:—

1st. That the "Wild Fowl Protection Bill" be passed as originally introduced, with the possible exception of the sentence whereby fowls proved to have been imported from any foreign country are exempted.

2nd. That a "Bill for the Regulation of Bird-catchers" be brought in—its chief feature being the absolute prohibition of bird-catching by means of traps, springes, or nets during the spring months—say from April 1 to July 1, and that at other times of the year such engines should not be used within (say) 50 yards of any highway.

3rd. That the "sport" of Swallow-shooting be absolutely and at all times prohibited; and finally we may add that if a Chancellor of the Exchequer should ever take a hint from North Germany and lay a tax on birds in cages, we in the name of our Nightingales shall thank him.

FAUNA DER KIELER BUCHT

Fauna der Kieler Bucht. Zweiter Band: *Prosobranchia und Lamellibranchia, nebst einem supplement zu den Opisthobranchia.* Mit 24 tafeln. Von H. A. Meyer und K. Möbius. Small folio, 139 pp. (Leipsic, 1872.)

WE are rejoiced to see the second volume of this excellent "*ouvrage de luxe.*" Like the first volume, the second bears evident marks of having been prepared with the greatest care. The illustrations are inimitable and life-like: we venture to say that no such figures of Mollusca and their shells have ever been published in any country.

The introduction to the present volume contains an account of the currents, saline ingredients, and temperature of the water in Kiel Bay, together with elaborate tables of the latter properties in comparison with those in some other parts of the North Atlantic and in North Japan, as well as a notice of the peculiarities, distribution, and frequency of occurrence of the Kiel Bay Mollusca, and relative abundance of the genera and species in proportion to that of the Mollusca in Great Britain, Christianiafiord, and the Sound.

The body of the work embraces the subclass Prosobranchia (comprising the orders Cyclobranchiata, Pectinibranchiata, and Siphonobranchiata) of the class Gastropoda, a supplement to the first volume in respect of the other sub-class Opisthobranchiata (orders Pleurobranchiata and Pellibranchiata), and the Lamellibranchia (order Lamellibranchiata of the class Conchifera), with short diagnoses in Latin, and full descriptions in German of all the species given in the work. The admirable figures amply illustrate every character of the living animal and its shell, some being of the natural size, and others magnified 300 times.

We are not told whether any Brachiopod, marine Pulmonobranch, or Cephalopod inhabits Kiel Bay; but assuming the list to be complete, we find 23 species of Conchifera, and 40 of Gastropoda, being altogether 63 species. There are 562 species of Mollusca in the British seas. This great difference may arise from the brackish nature of the water in Kiel Bay; and to the same cause may be attributable the small size of all the Mollusca, except *Mytilus edulis*, which is usually stunted on the open sea coast.

The authors have satisfactorily shown that the genus *Triforis* (erroneously changed by Deshayes to *Triphoris*) is distinct from *Cerithium*, although belonging to the same family, between which and *Cerithiopsidæ* it appears to be intermediate. The principal difference consists in the animal of *Triforis* having a retractile proboscis; and Lovén's description of *T. perversa* was doubtful on that point. Other writers on the Mollusca have done nothing to help us in the classification of this difficult group. The shells are distinguishable by the shape of the mouth, which is very peculiar in *Triforis*; and the sculpture of the apex differs from that of *Cerithium*—an important character which might have been advantageously represented in the plate before us.

We hope the authors will not take amiss a few slight criticisms. Their *Rissoa inconspicua* is not Alder's species, but *R. albella* of Lovén. *R. octona* of Linné is probably a variety of *Hydrobia ulvæ*, judging from his description and the habitat "in Sveciæ subpaludosis." The species described and figured by Meyer and Mobius as *R. octona* has two more (viz. ten) whorls; it is not horn-colour, but variegated; the mouth is oval, and not "fere orbiculata;" and Linné does not mention the ribs which characterise the Kiel Bay species. The figures of *Rissoa striata* do not show the foot-appendage or caudal cirrus, although it is described in the work. *Amphisphyra* should be *Utriculus*.

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[1] It is painful, however, that such folly should be countenanced by reviews which in other respects are deservedly of high repute. But in no department of criticism is there such a want of competent writers as in Zoology. We are not exaggerating when we say that nine out of ten reviews of zoological works are written by men who have no sound knowledge of the elements of the science.

We wish the authors could have given us some information as to the *modus operandi* of the *Teredo* in excavating its cylindrical tube, instead of merely quoting Kater's opinion that the shell is the boring organ. One thing is certain, and indeed has been admitted by Kater, that the foot of *Teredo* is in front, occupying the bottom of the tube, while the shell at the same time occupies that part of the tube which lies immediately above the foot, and is closely pressed against the sides of the tube. To suppose that the position of the foot and shell could be reversed by the animal, so as to make the shell lie at the bottom of the tube and the foot on one side during the process of excavation, is quite inconsistent with our knowledge of the *Teredo* and of the habits of other boring and burrowing Mollusca. *Solen*, *Cardium*, *Natica*, *Actæon*, and many other kinds burrow in sand by means of their strong muscular foot; *Pholas dactylus* occasionally does the same; and the limpet uses its foot only for excavating the hard rock in which it is sometimes more or less deeply imbedded. The gradual enlargement throughout of the tube of *Teredo*, especially at the opening (where the siphons are placed), cannot possibly be caused by the shell, which invariably lies at the other end; and the prickles which cover the surface of the shell, and enable it to act as a fulcrum or *point d'appui*, could not be renewed if they were continually employed in rubbing away the wood. There can scarcely be a question that the foot is the sole instrument of perforation in *Teredo*, as it is in *Solen*, *Pholas*, and *Patella*.

J. GWYN JEFFREYS

OUR BOOK SHELF

The Student's Manual of Comparative Anatomy and Guide to Dissection. Part I. (Mammalia). By G. H. Morrell, M.A. (Longman and Co.)

THIS work is in two parts, which are of such different characters that they must be considered separately. The first is intended to include a short and complete summary of the main facts of the anatomy of Mammalia. This is a large undertaking, and one which a resident in Oxford has not full opportunities of completing; for the advantages in any place other than London, are not sufficient to enable any single student, however enthusiastic, to get familiar with many of the subjects discussed. There is a want of vividness and point in many of the statements, several of which are too inclusive. Referring to the lobulation of the kidneys, the seals and whales are mentioned as presenting it, but why are the ox, otter, and rhinoceros omitted? The peculiarity of the stomach of the chevrotain is not referred to, and all we can possibly infer as to that of the peccary or hippopotamus is that it is constricted into two or three portions, which is undoubtedly not enough. Half a page only is devoted to the peculiarities of the liver throughout the class, and that of man is called simple, while that of the Ruminants is included among the multifid. The spleen of the marsupials is stated erroneously to be bent or bilobed.

But the great and inexcusable imperfection of the work is the omission of the description of the generative system, which no amount of argument could persuade us will prove of the slightest good in any way. It only engenders a mystery and curiosity in the mind of the younger students, as to peculiarities of structure, which if they were treated in the ordinary routine, would, as they undoubtedly are among medical students, be looked upon in nothing but a common-place manner.

The second portion of the work, the guide to the dissections of the brain, heart, &c., of the sheep are excellent, and will be found of great value; they have long been wanted by teachers. A carefully compiled synopsis of the cerebral convolutions in man and the higher apes, from the work of M. Gratiolet, terminates the book.

Académie Royale de Belgique. Centième Anniversaire de Fondation. Two vols. (Brussels: F. Hayez, 1872.)

THESE two stout volumes, intended as a memoria of the celebration of the hundredth anniversary of the Belgian Academy, treat of a great variety of interesting and valuable matters. The Belgian Academy of Science, Literature, and Art was founded by Maria Theresa on December 16, 1772, but as December is not a very suitable month for a great public gathering of men from all parts of Europe, the Academy held its centenary fête on May 28 and 29, 1872, and it did it very royally, in presence on both days of His Majesty the King of the Belgians, who gave the opening address, and entertained members and friends on the second day in his palace at Brussels. There took part in the celebration distinguished deputies from all the countries of Europe and from America, and altogether it seems to have been a great success. In these volumes will be found a detailed account of all that was said and done, verbatim reports of all the speeches made, and of all the interesting papers read. The Academy began to make preparations for the centenary celebration in 1869 by the appointment of a commission. This commission appointed members of the various classes of science, literature and art to prepare papers giving accounts of the work done in these classes from the commencement, and others to do the same for the various literary, antiquarian, artistic and scientific subjects with which the Academy deals. From this it may be surmised that these two volumes contain matter of very great value indeed. The first paper is by M. A. Quetelet, giving a sketch (170 pages) of the history of the first century of the Academy. But the second volume will be the more interesting of the two to scientific men; we can only indicate its contents:—Astronomy in the Royal Academy of Belgium from 1772 to 1872, by M. E. Mailly; Report on the Mathematical works of the Academy during the same period, by M. J. M. de Tilly; Report on works in the Physical Sciences, Meteorology, and Physical Geography, by M. J. Duprez; Report on works in Chemistry, by M. L. G. de Koninck; Report on works in Zoology, by P. J. van Beneden; Report on works in Botany and Vegetable Physiology, by M. E. Morren; Report on works in Geology and Mineralogy, by M. G. Dewalque.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Biela's Comets

THE present note is designed to show that several comets move in nearly the same orbit with that of Biela; that they probably entered the solar system *as a group*; and that, after making their first perihelion passage in close proximity to each other, they were, when receding from the sun, thrown into their present orbits by the disturbing influence of Jupiter.

1. *Was the comet of 1772 identical with that of Biela?*—The mean of the seven consecutive periods between January 2, 1806, and September 23, 1852, is 2437·7 days. Counting five periods of the same mean length from February 17, 1772, brings us to July 2, 1805—six months before the perihelion passage of 1806. In other words, the mean period between 1772 and 1806 was greater by about thirty-seven days than that between 1806 and 1852. The perturbations during the half century succeeding the apparition of 1772 have not been computed. It seems very unlikely, however, that the difference of periods

could thus be accounted for. We conclude, therefore, that the comet of 1772 was not that of Biela.

2. The first comet of 1818 is regarded by Dr. Weiss as a probable member of the Biela group.* This body, discovered by Pons, was visible only four days. Its elements, as computed by Pogson, have a striking resemblance to those of Biela's comet, the longitudes of the ascending nodes differing by only 1°. There can be little doubt that it was connected, in its origin, with the comet of Biela.

3. The companion of Biela, observed in 1846 and 1852, is another comet of the same cluster. The fact that several cometary masses move in orbits almost identical, may afford a plausible explanation of the division of Biela's comet. Was one member of the group overtaken by another as they were approaching perihelion in 1845, and was their separation after imperfect collision the phenomenon observed at that epoch?

4. The comet detected by Pogson, at Madras, on December 2 and 3, 1872, may have been another member of the same family. Its perihelion passage occurred nearly three months after the time computed for that of Biela. Prof. Newton has remarked † that so great a lengthening of the period could not probably be explained by planetary perturbation.

M. Hoek has shown ‡ that certain comets have been associated in groups before entering the solar domain. When the members of such cometary systems are widely separated, they may pass round the sun in very different orbits. The comets, however, which constitute the Biela cluster must have entered our system at small distances from each other, since their orbits are nearly coincident. These orbits, between longitude 255° and 265°, pass within no great distance of that of Jupiter. The group had perhaps made its first perihelion passage in a parabolic orbit. Receding from the sun, it fell under the controlling influence of Jupiter; the comets had various positions in relation to the planet, and hence the orbits resulting from the attraction of the latter were slightly different.

We might regard the comet of 1772, the companion of Biela, and Pogson's comet of 1872, as probably identical, but for the small increase of distance between the two Biela-comets in the interval from 1846 to 1852. The period would be about 2456 days.

That the comets of this cluster have been moving in their present orbits but a comparatively short time is rendered probable by the fact that no two of the members hitherto detected have become widely separated, and that, notwithstanding the frequency of the return to perihelion, the meteoric *débris* is much less diffused than in the case of other known streams.

Were all the members of this cluster originally united in a single comet, or did they *enter the solar system* as a group? To this question, perhaps, no satisfactory answer can yet be given. It seems probable, however, that the united masses would have formed a somewhat conspicuous object, too brilliant to have entirely escaped observation. DANIEL KIRKWOOD

Bloomington, Indiana, April 15

Earthquake in Dumfries

WHILE sitting in my lonely house in a retired but beautiful glen of Dumfriesshire, I was aroused on the evening of Wednesday 16th current, at ten minutes to ten o'clock, by one of the most singular noises ever I had listened to. The tone of it was somewhat like thunder, but it did not rise and fall in pitch. It lasted, perhaps, for twenty seconds, and was accompanied by a slight tremor. At first I thought it was a two-horsed carriage coming, and at a lumbering pace, and then, with some hesitation, I took it for thunder, but next day I found that it was generally recognised as an earthquake. The shaking was very perceptible in some localities. It extended through the parishes of Closeburn, Morton, Penpont, Glencairn, and Tynron, over a length, I am safe to say, of ten miles. Dr. Grierson of Thornhill Museum felt it as a rude shock. In Tynron village there was some alarm, as one family thought it was the wall of the churchyard that had fallen. On December 24, last year, a similar shock was felt in some parts of Upper Nithsdale. Although I have resided for many years in Dumfriesshire, these are the only occasions on which there was any surmise of an earthquake. The local papers have said almost nothing about it, but I am sure this will interest some of your readers.

Tynron School, April 23 J. SHAW

East India Museum

ALLOW me to make yet another suggestion (in addition to those of P.L.S. and Prof. Newton), with regard to the disposal of the natural history collections at the India House. It seems to me to be one of the greatest popular delusions, that specimens of natural history necessarily require lofty halls and spacious galleries for their preservation and exhibition in a useful manner. I hold, on the contrary, that, with few exceptions, they far better serve educational and scientific purposes when arranged in ordinary apartments. All the scientific work in the British Museum is done in small rooms; and the palatial galleries with their crowded myriads of specimens and miles of glass cases, however instructive they may be (or might be made) to the public, are a positive hindrance to scientific work. I am very much mistaken if all the India House natural history collections might not be suitably placed in two or three ordinary sitting rooms, and so arranged in cabinets and boxes as to be far more convenient for reference and study than they have ever been. The rent of a moderate-sized house in an airy situation, say 250*l*. with an equal sum for the salary of an efficient Curator, and a small grant for cabinets and the necessary books of reference, is all the expense required to make this interesting collection completely accessible to all who wish to consult it. Every one interested in Indian natural history would then visit it. It would again receive gifts of collections from travellers, Indian Officers, and other persons interested in the natural history of the East; and its increase in value from this source alone might go far towards furnishing a tangible equivalent for the expense incurred, while it would certainly render the collection a better representation of the Indian fauna than it is at present, and more worthy of a place, at some future time, in the proposed grand Indian Museum.

Such a modest establishment would also, I believe, do much good by showing at how small an expense a really useful scientific museum may be kept up, and would thus encourage the formation of local museums in cases where 20,000*l*. or 30,000*l*. cannot be raised for a building. It would not, of course, be a show museum for the uneducated public to wander and gaze in; —the British Museum serves that purpose. But it would prove greatly superior to any such mere exhibition, as a means of furnishing definite information on Indian zoology, and enabling any intelligent inquirer to obtain some idea of the many wonderful and beautiful forms of life which characterise, what is at once the smallest and the richest in proportion to its extent, of the great zoological regions of the globe.

ALFRED R. WALLACE

IT will be greatly to be regretted if even your suggestions are adopted as a remedy for the present neglect, and the claims of scientific men and of the public at large for a Government museum be abandoned. It is very desirable for Indian interests that the Museum shall be, as before, connected with the Indian department.

It is quite true accommodation in the sky-parlours, with casual access by a lift, is given for the industrial collections so well conducted by Dr. Forbes Watson, and which collections, as chairman of the Indian Committee of the Society of Arts, I feel bound to contend for as of great value to England and to India.

There is no solid ground for letting the Government go. They acquired in the like way the property of the Levant Company, and attempted to shirk the rights and obligations, but were compelled to maintain the public buildings, churches, hospitals and burial-grounds at Constantinople, Smyrna, &c. It must be owned they constantly attempt to evade the obligations.

They are now engaged in paying off the stock of the old East India Company, of which they have acquired the territory, houses, property, prerogatives, &c., and they must simultaneously accept every obligation, pecuniary and moral.

This was a museum for the service of England and the service of India, and there is no reason why it should not be kept up. There is, it is true, a growing licence in this day for representing us as usurpers and oppressors of India, whereas the peace, prosperity, and progress of India have been created by us, and were we to withdraw, would be destroyed by the sanguinary conflicts of the various races of conquered and conquerors constituting the populations.

We ought to stand on our right to share in the prosperity of India as a prerogative belonging to us. Besides, for the benefit of India, the collections are kept up by Englishmen, for there is not the requisite knowledge among natives in India; the work must be done in this safer climate, and the specimens can be better preserved here than in the museums of the hot plains, or those which may be formed in the damp regions of the hills.

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* Astr. Nach., No 1710.
† American Journal of Science, April 1873.
‡ Monthly Notices of the R.A.S., vol. xxv., p. 243.

HYDE CLARKE

St. George's Square, S.W.

Instinct

Moving in a Circle

IN your last week's number a letter appeared with the initials N. Y., in which it was stated that it is believed in North America that a lost man always strays in a circle towards the left. I may mention that whilst walking in a woody and hilly part of the New Forest, I found, to my great astonishment, that I had described a complete circle, and it was towards the left. My father also tells me that he has been informed (although under what circumstances he does not recollect) that the same idea obtains in Australia. It has been suggested that the reason of this fact (if fact it is) is, that the right side of the body is stronger than the left; in confirmation of the truth of this explanation, it is worthy of notice that Dr. Wm. Ogle (in a paper on Dextral Pre-eminence, Medico-Chirurgical Transactions, vol. liv.) finds that men are right-legged as well as right-handed, although the rule has not so universal in application. One of the points adduced by him in evidence is that bootmakers generally find the right foot larger than the left.

If any of your readers who have strayed in a similar manner, would take the trouble to write to you merely stating whether they wandered to the right or the left, it is possible that a sufficient body of facts might be collected either to confirm or disprove this curious belief.

GEORGE DARWIN

Down, Beckenham, April 29

Perception in Dogs

PERHAPS you will think that the following story of a Mentone dog, Pietrino, is worth adding to the similar stories which have appeared in your columns:—

The Archduchess Marie Régnier passed the winter of 1871–2 at the Hotel Victoria in Mentone. While there she became much attached to a spaniel belonging to M. Milandri, the landlord, and on her return to Vienna in the spring she took the dog there. Not long after, the dog reappeared at the hotel in Mentone, having returned on foot a distance of nearly one thousand miles over a country totally unknown, excepting having once traversed it by rail. The fatigue caused the poor fellow to die a few days afterwards, and Pietrino is honoured with a grave and a monument in the hotel gardens.

I send you a French paper containing the same facts.

JAMES B. ANDREWS

Villa d'Adhemar, Mentone, April 17

PERHAPS the following anecdote on the instinct of dogs, which has lately come to my knowledge, may prove of interest to some of your readers.

A family residing in Yorkshire possessed two dogs, one a mastiff, and the other a small dog. The owner, visiting Hastings, took the little dog with him, and at the house where he stayed there was a larger animal, who, disregarding the laws of hospitality, woefully maltreated his youthful visitor. The little dog, upon this, disappeared, and in a few days returned, bringing with him the mastiff from Yorkshire, which set upon the Hastings dog and thrashed him to within an inch of his life. Having performed this piece of retributive justice he returned to his home in the north, while the little dog stayed to rejoice over his fallen antagonist.

A. PERCY SMITH

Rugby, April 18

Prehistoric Art

MR. SEARLE V. WOOD'S inquiry (NATURE, vol. vii. p. 443) whether any existing race of savages is capable of depicting animals with the spirit and fidelity of the supposed contemporary representations of the mammoth is a most pertinent one, but must be answered in the affirmative. In the Atlas to Gustav Fritsch's great work on the Aborigines of South Africa, just published at Berlin, will be found reproductions of delineations of animals, executed in caves by the Bushmen, which are certainly equal to the carvings and tracings of the prehistoric period. The originals are usually painted, but sometimes carved or scratched in sandstone or some other soft material. Five different colours are employed; the objects represented are usually the animals indigenous to the country, but the human figure is occasionally introduced, and since the arrival of the European colonists, horses and even ships have been added. It is most remarkable to find the Bushmen in this respect so far in advance of the comparatively civilised negro, who has never of his own impulse produced anything approaching to the merit of these designs. Perhaps some of your contributors will be able to state whether any corresponding difference exists in the cerebral organisation of the respective races.

R. G.

London, April 19

April Meteors

IN continuation of my report sent you yesterday in reference to the April meteors of this year, I desire to add the following. The evening of April 21 being clear, a watch was sustained from 9^h to 12^h, during which time 14 shooting-stars were seen. These, with the 20 observed on the two previous evenings, make the total number seen 34 in 7¼ hours of observation. The details of the meteors noticed on April 21 are as under:—

Ref. No.	Date.	Time		Beginning. R A	Beginning. D	Ending. R. A	Ending. D
21	April 21	9 8	1½ mag. *	266°	54°+	236°	32°+
22	,,	9 10	2nd mag *	299	38½+	309	38 +
23	,,	9 29	3rd mag *	310	59 +	319	58 +
24	,,	9 41	3rd mag *	289	61 +	270	68 +
25	,,	9 57	2nd mag *	263	50 +	238	47 +
26	,,	10 22	3rd mag *	273	51 +	273	61 +
27	,,	10 30	4th mag *	325	68 +	328	60 +
28	,,	10 32	4th mag *	264½	61 +	255	55 +
29	,,	10 50	4th mag *	319	69½+	339	66 +
30	,,	11 7	α Lyræ	295½	45 +	309	49 +
31	,,	11 16	3rd mag *	278	49 +	270	59½+
32	,,	11.32	4th mag *	275	14 +	283	12 +
33	,,	11 40	3rd mag. *	284	59 +	270	47 +
34	,,	11.45	4th mag. *	334	47 +	341	41 +

Nos. 22, 25, 26, 30, and 31 were from the radiant near α Lyræ. On April 19 and 20 the largest proportion of meteors were Lyraids, but on April 21 they were in a minority. Nos 21, 23, 24, 33, and 34 were conformable to a radiant at ο Draconis, R.A. 283°, D. 59°+, and it is worthy of note that on the two preceding nights there were no indications of this radiant point. To sum up my recent observations, it would seem that from the various meteoric tracks noted, the April shooting-stars of this year had three well-marked centres of radiation, viz., (1) near α Lyræ, (2) near Arcturus, and (3) at ο Draconis (R.A. 283°, D. 59+). There were also evidences of at least two other radiant points that, owing to the paucity of meteors, could only be approximately ascertained, viz., (1) near ζ Draconis, and (2) near α Cygni. The brightest meteor seen on April 21 was a Lyraid; time, $11^h 7^m$. Its path was accurately fixed. The meteor first appeared at 1° N. of δ Cygni, and travelling to N., disappeared in a small triangle of stars 5° N. of α Cygni. Several of the meteors emitted sparks in traversing their courses, but the majority were small objects of very brief duration.

The foregoing particulars (taken in conjunction with my previous letter) may be useful in determinating the radiant po[illegible]f the April meteors, especially with regard to those diverging from Lyra, which, I believe, are considered identical with Comet I. 1861. I fixed this point at R.A. 274°, D. 37+, which is nearly of accord with the result of Karlinski (1867), R.A. 278°·2 D. 34°5+, and of Prof. A. Herschel (1864), R.A. 277°·5, D. 34°6+.

Bristol, April 22

WILLIAM F. DENNING

A proposed new Barometer

IN the number of the *Philosophical Magazine* for May 1871 is an article by Prof. Heller, of Ofen, rendered (carelessly enough) from Poggendorff's *Annalen*, describing a balance fitted with nearly equal weights of very different volumes, which he proposes as a barometer. He says that the principle on which it is founded "has not hitherto been used in barometric measurements." This is not quite correct; a balance, absolutely identical in principle, is described by Boyle in vol. i. p. 231, of the Philosophical Transactions, under the title of "A new Statical Baroscope." It would seem that the practical difficulty of keeping it in accurate adjustment has been and still will be a bar to its use in the way the two inventors have proposed; otherwise, it might perhaps be advantageously employed in mountain surveys; it would, at any rate, be free from many of the objections to the aneroid.

Considered, however, as an exact barometer, I would maintain that the principle is altogether erroneous, depending as it does on the assumption that the pressure of the atmosphere is purely a function of its specific gravity or density. This is not true, for pressure may vary within wide limits, whilst the density remains unchanged. Experimentally this might be shown by putting, say, an aneroid and a balance, such as I have been speaking of, in a large glass vessel, which can be made air-tight when closed. Under normal conditions the two will at first register the same pressure; but if the temperature is sufficiently increased or diminished, the increase or diminution of elastic force will manifest itself by the aneroid; but as the density remains unaltered, the balance will show no change. Does such an experiment at all correspond with any natural observations? I think so, in, of course, a limited degree. If the lower part of a column of air is heated, its expansive force will push the adjacent air outwards and upwards; but as it does so, it has to overcome a certain amount of inertia; to do this requires time, during which, as the volume of the heated air does not increase in proportion to the temperature, the elastic force does. This ought to be shown by the barometer; I think it often is, but the barometer is a sluggish instrument at best, and its indications are undoubtedly wanting in quickness, and therefore in exactness. Still its principle is correct; so is the principle of the aneroid, or of Bourdon's barometer (on which there is an interesting paper in the *Quarterly Journal* of the Meteorological Society for April 1872), though practical difficulties stand in the way of their use becoming general. But the tangent balance is not capable of measuring atmospheric tension, except when that tension depends on density alone; and this is frequently not the case—perhaps never.

J. K. LAUGHTON

April 23

Acquired Habits in Plants

AT p. 446 of NATURE, J. G. records a "dog violet" which he thinks has assumed an unusual form. As there are several plants called "dog violet," and as one of them does in favourable situations attain a very considerable height, it would be interesting to know what was the species observed by the river Aled. The *Viola canina* (*V. riviniana* Reich.), in one of its forms which is probably a distinct species, has flowering shoots which sometimes attain a foot in length, and if supported by the surrounding vegetation do sometimes stand nearly upright. If this was the plant observed, J. G. only found a more than usually strong form.

C. C. BABINGTON

The Zodiacal Light

MR. BACKHOUSE asks if the observations given in vol. iii. p. 203, afford any proof that the Zodiacal Light is not a lens-shaped disc of light enveloping the Sun; if this theory were correct, and the sun enveloped in a continuous mass of light-reflecting matter, whenever the light is seen in the evening after sunset, it ought to be also seen in the morning before sunrise, of the same brilliancy at the same angular distances from the sun, especially when those distances are small, for then the effect of an elliptical form in the section of the envelope by the plane of the ecliptic would be almost entirely eliminated.

The results of observation given in most of our hand-books of astronomy are therefore directly at variance with this theory, and I did not consider it necessary to allude to it before.

Jamaica, April 6 MAXWELL HALL

ON VENOMOUS CATERPILLARS *

POISON and venom are often used as convertible terms. I do not understand them to be so. Poison properly means something which injures the system by introduction through the stomach. Venom, something which injures by introduction into the vascular system through lesion of the tissues. Most poisons are also venoms; whatever injures, if introduced into the stomach, will most probably also injure if introduced directly into the blood. But the converse is not true: most venoms are not poisons, that is, it is not by digestion and assimilation that they work, but by entering the vascular system from without. It is said that you may swallow the venom of the rattlesnake with impunity; and I imagine you may, if it does get absorbed through the mucous membrane; but Dr. Fayrer's experience, lately published, of the effects of the semi-swallowing, which occurs in extracting the venom from a poisoned wound by sucking, would rather seem to show that such extremely virulent venom would penetrate the mucous membrane, and act as if actually introduced by a wound, his throat having become dangerously ulcerated from sucking the poison from the wound of a man bitten by a cobra. There is yet another way than swallowing or being wounded, by which venom may injure, and that is through the nervous system, by application to the skin. This is the way in which the nettle must sting. In that case there is not the smallest lesion in the skin, and if a nettle were artistically made to touch the open surface of a gaping wound, it would not sting at all; neither is it by mechanical irritation that the pain is caused. The nettle has a venom gland, as well as the rattlesnake, and it is the application of this venom to the delicate termination of the nerves in the skin which produced the pain felt.

The subject to which I invite the consideration of the Society this evening is whether any insects possess similar power of injury to that of the nettle. In ordinary cases the venom of insects is applied by a puncture in the skin, into which the venom is introduced by an apparatus provided for the purpose. But for a long time it has been said that certain caterpillars sting like the nettle, although the authorities have for the most part been too vague to allow us to be very sure as to the fact; and supposing the fact to be true, it has been argued that the pain or annoyance was merely the result of mechanical irritation of a similar nature to that which medical men sometimes meet with in hairdressers, or rather hair-cutters, where minute portions of the cut hair of their customers work their way into the skin below the shirt-sleeve and give rise to a painful and irritating sore on the wrist. Two passages which I shall take leave to quote, will bring the question, as it at present stands, pretty fairly before the meeting. The first is from a paper by myself on the geographical relations of the chief Coleopterous Faunas, which was published in the Linnæan Society's Journal for 1870 (p. 55):—

"A very remarkable African affinity in the Lepidoptera has been mentioned to me by Dr. Welwitsch. It is plain that an affinity to any genus endowed with peculiar properties is rendered doubly certain if the supposed allied species possesses the same properties. There is a Lepidopterous insect in Australia, the larva of which possesses remarkable poisonous powers. It has been named *Doratophora vulnerans*. Such insects also occur in South Africa. Livingstone speaks of a caterpillar called *Rigura* as producing fearful agony if a sore is touched with its entrails. Mr. Baynes, in his "Explorations in South-west Africa," speaks of another, or perhaps the same, which he calls the *Kaa*, and which is used as a poison for their arrows by the Bushmen; and Dr. Welwitsch had a personal experience of the severe swelling and pain in every part of his body which he touched with his hand after collecting specimens of a caterpillar against which he had been warned as poisonous. He had in consequence of the warning carefully avoided touching them, shoving them into a phial with a straw; but whether he had inadvertently touched them or fingered the leaves on which they had been feeding (which he collected for examination), he and his servant were both laid up helpless for two or three days. His specimens of the caterpillar were lost; but among his Lepidoptera Dr. Fendler of Vienna, who has undertaken a description of them, finds no less than four species of *Doratophora*, and these, doubtless, are the perfect insects of species of the caterpillar, from one of which he suffered."

The second passage which I wish to quote is from a paper by Mr. Roland Trimen, Notes on the above paper, and also published in the Linnean Society's journal. It is as follows:—

"At p. 55 Mr. Murray notes what he considers 'a very remarkable African affinity' in the Lepidoptera of Australia, in reference to the case of the larva of *Doratophora vulnerans* Lewin. The instances which he cites as analogous, however, are very different in character, for he quotes the mention by Livingstone 'of a caterpillar called *Rigura*, producing fearful agony if a sore is touched *with its entrails'*; and the statement made by Baynes and other travellers, that a caterpillar is used by the Bushmen to poison their arrows. It is evident that, if a caterpillar be used at all for poisoning arrows (concerning which report my inquiries have hitherto been attended by no satisfactory result) it must be the intestines or juices of the animal which are so employed. But the case of *Doratifera vulnerans* is the common one of (what appears to be mechanical) irritation, by means of clusters of spines, a defence possessed by many caterpillars, not only in Australia and South Africa, but throughout the globe, and of which the larva of the European *Cnethocampa processionea* presents a familiar example. Duncan (Nat. Libr. Ent. vol. vii. Exotic Moths, pp. 181-2. pl. xxii. f. 5) represents the larva of *D. vulnerans* as possessing four fascicles of rufous spines, exsertile at will on both the anterior and posterior portions of the body, and quotes Lewin to the effect that the wound inflicted by the fascicles is very painful. According to Mr. Murray's account it would appear that the African larvæ, from the handling of which Dr. Welwitsch experienced such suffering, were near allies (if not actually species of *Doratifera*); and the conclusion is obvious that it was by fascicles of spines that the pain was occasioned—not an uncommon case in the warmer parts of the world, and one by no means indicative of any special relation between the Lepidopterous faunas of South Africa and Australia."

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* A paper read at the opening of the Kensington Entomological Society

Mr. Trimen is obviously right as to the absence of analogy between the venomous properties of the caterpillars spoken of by Livingstone and Baynes, and those met with by Dr. Welwitsch, and it was a slip on my part to collocate them together; but I am not satisfied that he is equally right in referring the pain caused by the species of *Doratophora* to mechanical irritation. He gives no facts in support of his assumption to that effect, and the facts communicated to me by Dr. Welwitsch regarding the insect from which he suffered seem to me wholly inconsistent with that supposition. It may be supposed from his and my silence that we acquiesced in Mr. Trimen's views. But it is not so. When Mr. Trimen's paper appeared Dr. Welwitsch spoke to me upon the point, and I urged him to communicate to the scientific world fuller details of the incident than I had given, and I understood that he intended to do so in any account of the insects collected by him. I therefore did not feel warranted in speaking, which I now regret, for as with much else that he had on hand to do, his life has been too short for him to do it himself. Now that he has passed away from us I should not like an erroneous impression to exist as to the facts; and although I have little to add to what I formerly stated as communicated by him to me, I should wish to repeat it more precisely, and to say that Dr. Welwitsch himself was firmly convinced that it was not a case of mechanical irritation but of a special virus of unusual potency.

In the first place, then, Dr. Welwitsch had heard of this noxious caterpillar before he met with it—the natives knew it well and dreaded it. In the next place when he did meet with it his native attendant warned him of it—and they took every precaution against touching it; they plucked leaves on which the caterpillars were feeding and guided them from the leaf into the wide-mouthed bottle or vessel he had to carry such specimens home in. They also took specimens of the plant on which they were feeding. I suggested to him that the sting might have been in the plant, but this he was positive was not the case. The virulence of the venom was such that by the time they reached home in an hour or so after, every tender part of their body which they had touched with their fingers had become swollen and inflamed; their eyes were closed up, their lips and cheeks swollen as if they had been assisting (as principals) at a prize fight, and the consequent fever was so great that they were laid up, unable to move for two or three days; and when they did get up he found that their attendants had bundled out of the house both the caterpillars and the plants on which they fed. Now it seems to me that mechanical irritation is a wholly inadequate cause for such extreme inflammatory action. Mechanical irritation may go a certain length, but there are bounds beyond which we must look for some other explanation.

But first we want more facts and more examples. I exhibit two caterpillars, apparently different species, which I have received from Old Calabar, given to me with a notandum as reckoned injurious if not venomous, but my information as to them is too vague to allow me to cite them as positive examples of venomous caterpillars. And I also show one from Brazil which I have received from my friend, Mr. Fry, which he informs me bears a very bad character in Brazil. Both of these, indeed, all to which this property has been ascribed, are hairy caterpillars; but then it is only hairy caterpillars that seem to have the necessary apparatus for stinging—all stinging plants, so far as I know, are hairy. If the caterpillars have a special venom, then, as in the nettle, there should be a gland at the base of each hair, which should be hollow, and the spines in most, if not all, our caterpillars are hollow. I know of no physiological reason against their being so made. In the skin of the newt there are pores which exude an acrid irritating fluid. If a hollow hair were placed over the pore with proper muscles, we should then have a parallel to the supposed case.

But, as I said before, we want information as to the existence and amount of this venomous property, and the chief object of this paper to-night is, after eliciting the views of the meeting, to suggest to those who may have the opportunity, the desirableness of making observations on the point.

A. MURRAY

ON SPACE OF FOUR DIMENSIONS

WE may define *space* as that which indicates and measures the extension of the Universe. We may determine the form and position of any material object by assuming three infinite planes, fixed in infinite space, and at right angles to each other. Space then is the room occupied by matter, or included between distant masses of matter; and, as such, we know of it only as possessing three dimensions:—length, breadth, thickness.

Descartes (*Principia* pars. 2, "*Quid sit spatium, sive locus internus*") remarks, "For, in truth, the same extension in length, breadth, and depth, which constitutes space, constituted body; and the difference between them consists only in this: that in body we consider extension as particular, and conceive it to change with the body; whereas in space we attribute to extension a generic unity (*genericam unitatem*), thus after taking from a certain space the body which occupied it, we do not suppose that we have at the same time removed the extension of the space, because it appears to us that the same extension remains there so long as it is of the same magnitude and figure, and preserves the same situation in respect to certain bodies around it, by means of which we determine the space."

Gauss used to say that one of the happinesses of his future life would be the amplification of his conceptions of space; the realisation of that which he had once known as space of three dimensions, as space of four dimensions. For just as we can conceive of beings "like infinitely attenuated book-worms in an infinitely thin sheet of paper," which can realise space of only *two* dimensions, so also we may conceive of beings capable of realising space of *four* dimensions. Prof. Sylvester, Dr. Salmon, Prof. Clifford, and others, have indicated in some of their profoundest mathematical demonstrations that they possess "an inner assurance of the reality of transcendental space." We desire now to bring forward, with great apology to the mathematicians for our temerity, some ideas, which we believe may enable even the least mathematical amongst us, to realise,—faintly, indeed, and very dimly—the possibility of existence of space, other than that which we now occupy. This we propose to do, (α) by attempting to realise a condition of life in space of two dimensions, and (β) by adding the element of diverse motions, to our already known space.

Our knowledge of the Universe involves the conception of *space*, *time*, and *number*. These are intuitive notions: we cannot strictly define them; in the abstract our notion of them is merely relative; apart from material existence we cannot realise them. Extension is an essential property of matter, and our conception of space is linked with our conception of extension. Robert Hooke, in a series of lectures *De Potentia Restitutiva*, written nearly two hundred years ago and too little known, defines a sensible body as "a determinate space, or extension, defended from being penetrated by another, by a power from within." Now this power may be most readily conceived to be a vibratory motion of the particles across a position of rest. Let us imagine an infinitely thin plane vibrating between two fixed points with such velocity that no other matter can penetrate into the space limiting the vibration, then a solid bounded in one direction by the two fixed points would be the result. For example, let an infinitely thin sheet of iron a metre square vibrate with extreme velocity in a span of one metre, and a cubic metre of iron would be the result. The rapid vibration of the plate would defend the range of vibration from being penetrated, and impenetrable material substance would result. An infinitely thin line vibrating between two fixed points would furnish a plane. An infinitely thin plane vibrating between two fixed points would furnish a solid. Thus by the addition of motion we can convert a determinate space, approximately of one dimension, into space of two dimensions; and by the addition of motion we can convert space of two dimensions into space of three dimensions. Can we conceive of any motion which given to space of three dimensions shall generate space of *four* dimensions? We do not know of such motion, but we can surely conceive the possibility of its existence. Space of four dimensions is transcendental space: it is beyond the limit of our experience, but not beyond the limit of our imagination.

Let us now endeavour to realise the condition of a being living in space of two dimensions. If man possessed the eyes and the power of flight of an eagle, superadded to his ordinary intellectual qualities, he would, no doubt, have very enlarged views of space. As it is, man is distinguished from the brute animals by his erect bearing, and the range of space which his vision enables him to scan. Our eyes are easily movable in various directions, so also is our head; by a slight movement of the head and eyes, we may take in either space bounded by the horizon, or by a surface a foot square. If we throw our head back we enlarge our view of space; if we bend our head forward we narrow our view of space. Now, imagine that a man thus endowed, and with our own notions of space of three dimensions, begins to stoop forward and to grow so: his eyes survey less space; he stoops more forward; his body forms angles of 80°, 70°, 60°, 50° in succession, with a horizontal plane. Then he is obliged to go on all-fours, his limbs shorten and are gradually absorbed into the mass of his body; he crawls, he creeps; at length his limbs disappear altogether, and he trails himself along and glides like a serpent, moving in a horizontal plane. During these successive shrinkings in the direction of his thickness his head has become fixed, his eyes motionless, in the plane in which he moves, and his vision has hence become more and more limited. Now his body begins to diminish in thickness; he becomes thinner, and thinner, and thinner, and when he has become very thin indeed, let his thickness be expressed as the numerator of a fraction, while the denominator is an infinitely great number—say, if you will, as many figures as, written on paper, would reach ten billion miles, with ten figures to an inch. Now he is a mere plane, an infinitely thin surface; he occupies space approximately of two dimensions; his eyes are on a line. Try to imagine what the ideas of space of such a being would be; compared with our own ideas of space, compared with his own ideas before and during his process of flattening. He would now contemplate only a plane surface; he would see length and breadth without thickness. Compare also his ideas of space at each and every position between verticality and horizontality as his ken gets less and less, and at last the whole world is shut out from him.

Again, to come nearer home, and back again to the world of real existences, let us compare our own ideas of space after concentrating our vision for awhile on a book a foot square, with our ideas of space acquired while we ascend a lofty mountain, or lie upon our back on the deck of a vessel in mid-ocean. Compare the views of space possessed by a prisoner immured for forty years in a dungeon eight feet square, of La Sachette in the *Trou aux Rats*, of a being bed-ridden for half a century, with those of a hunter in the prairies of the West, a sailor of the Atlantic, even of a dweller in a flat tame country. The conceptions of space possessed by these different people will vary enormously. Contract the limits of space of possible contemplation; remove the possibility of contemplating space of great dimensions, and the *faculty* of such contemplation will itself die out; and thus, by a gradual process of diminution, we may arrive at our ideal being, living in space of two dimensions. Finally, let us imagine the being of two dimensions—length and breadth—to become narrower and narrower, and when he has become extremely narrow let us divide his breadth by an infinitely large number, and he becomes approximately of one dimension; he has now only length; he lives in a line; his one motionless eye is a point.

So much for space of less dimensions than our own. Let us now try to conceive an extension of our ordinary space; and let us attempt this by the superaddition of motion to known space. And let us clearly realise the fact that one and the same thing may easily possess various motions at the same time. For instance, when I walk across the room, talking the while; my vocal chords possess *five* distinct motions: (α) their own proper motion of vibration; *plus* (β) the motion of translation caused by walking forward; *plus* (γ) the motion of rotation of the earth about its axis; *plus* (δ) the motion of revolution of the earth about the sun; *plus* (ϵ) the motion of translation of the whole solar system through space. Let us suppose now that our bodies instead of being at apparent rest, were to vibrate in arcs with an amplitude of 10,000 miles, and with an infinite velocity; and let the plane of the direction of vibration itself vibrate between limits 10,000 miles apart; and let the whole vibrating system move with infinite velocity in a circle 1,000,000 miles diameter; and let the circle rotate upon its diameter; and let the sphere of revolution thus formed revolve in an infinitely great ellipse; and let the ellipse rotate upon one of its axes; and—— but hold! we have surely arrived at a somewhat enlarged view of our own relations to space. Conceptions of this nature sufficiently pursued may, perchance, lead us to the very threshold of transcendental space; and, once on the threshold, we may look wonderingly beyond.

G. F. RODWELL

ON THE SPECTROSCOPE AND ITS APPLICATIONS

VIII.

I TOLD you I had something more to say about the spectrum of blood, and this is not only an instance of the way in which the spectrum helps us in several important questions that, at first sight, do not seem at all connected with each other, but it shows the enormous power of research that is open to us. The colouring matter of blood, for instance, is found, like that of indigo, to exist in two perfectly different states, which give two perfectly different spectra. The colouring matter of blood is indeed capable of existing in two states of oxidation, which are distinguishable by a difference in colour, and also in their action on the spectrum. They may be made to pass one into the other by suitable oxidising and reducing agents; they have been named by Professor Stokes, their discoverer, red and purple cruorine. Previous to the introduction of spectrum analysis, red and purple cruorine were perfectly unknown. Further, if by means of a spectrum microscope, such as I have already described, a blood-stain is examined, Mr. Sorby asserts that the thousandth part of a grain of blood,—that is to say, a blood-spot so small that it only contains $\frac{1}{1000}$ of a grain, is perfectly easy of detection by means of this new method, and he has shown that its presence may be easily proved in stains that have been kept for a long time, and recognised even after a period of fifty years.

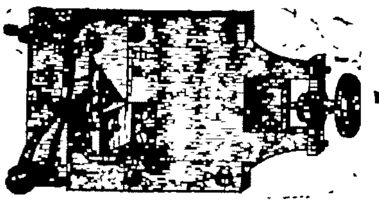

FIG 46

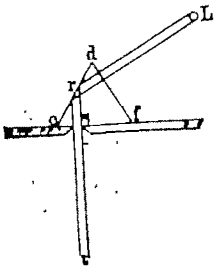

FIG 47

Fig. 46.—Steinheil's slit, showing reflecting prism. Fig 47.—Path of light through reflecting prism and into the slit.

He has also shown how it may be detected under the most unfavourable conditions, provided that a trace of hæmatin has escaped decomposition or removal; he has, in fact, successfully applied this method in several important criminal cases.

Another very interesting fact is, that when blood contains very small quantities of carbonic oxide gas in solution, it exhibits a very curious series of absorption bands. This fact is of considerable value in toxicological research, for in cases of poisoning by the so-called charcoal fumes, where, as is well known, the poisonous action is due to the formation of carbonic oxide, it can be readily detected by the peculiar bands which the blood under these circumstances exhibits.

Mr. Sorby has also applied the spectrum microscope to the study of blow-pipe beads, and has shown that in some cases as small a quantity as $\frac{1}{10000}$th of a grain of some substances can be thus recognised, even when mixed with other coloured bodies, which would interfere with the usual reactions dependent on colour alone.

In the case of radiation, as you know, we are able to determine the existence of new elements altogether. This is produced to a certain extent, as in the above case, in the absorption spectrum. Let me give you another practical application of this principle. Dr. Thudichum, as a result of researches made for the Medical Department of the Privy Council, has communicated to the Royal Society a paper in which he narrates the result of his inquiries on the yellow organic substances contained in animals and plants; and at the present moment it is impossible to say what important practical results may be expected as we come to know more about these substances, especially in the matter of dyes, which I am sure is a thing that will commend itself to you.

Again, Mr. Sorby, in a communication to the Microscopical Society, brings the matter still nearer home. He shows us that, in the case of wines, he can, by means of the absorption bands, determine the very year even of vintage, and this, you will see at once, is a matter of very great importance. Let me read you an extract from one of Mr. Sorby's reports. He says:—"The difference for each year is at first so considerable that wines of different vintages could easily be distinguished; but after about six years, the difference is so small that it would be difficult or impossible to determine the age to within a single year. After twenty years, a difference of even ten years does not show any striking contrast, and the age could not, therefore, be determined to nearer than ten years by this process. However, up to six years I think it quite possible to determine the age to within a single year. I took specimens of various ports from the casks, of different ages up to six or seven years, and labelled them in such a manner that I did not know the age of any, but could ascertain it afterwards by reference. I then made the experiments with great care, and found that, by proper attention to the details described above, I could correctly determine the year of vintage of each particular specimen." (*Chemical News*, December 17, 1869, p. 295.)

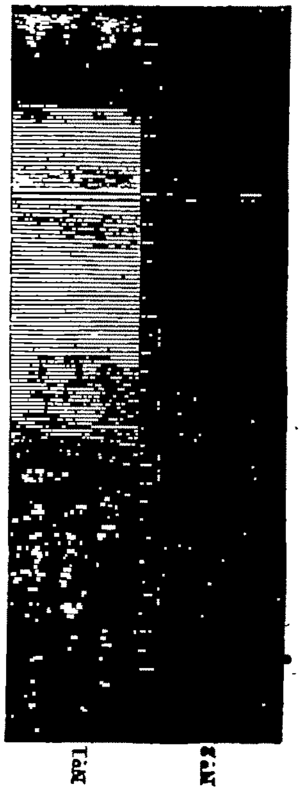

FIG. 48.—Coincidence between the bright line given out by sodium vapour and the dark line produced by the absorption of sodium vapour.

We have, in fact, a definite method of analysis of animal and vegetable colouring matter, and also of the colouring matter of decayed wood. Nor is this all, for, in another communication—for these things are now beginning to crowd upon us, and they will continue to do so much more by-and-by—Dr. Phipson asserts that this new method is perfectly competent to indicate any artificial coloration of wine. Mr. Sorby, on the other hand, has given his attention to beer; so that you see, if I have been taking you occasionally to the stars, I sometimes have the opportunity of travelling a great deal nearer home.

Mr. Sorby has also made some extremely delicate and interesting researches on the colouring matters existing in leaves. He has been able to identify numerous colouring principles, which he has arranged in five distinct groups: these groups rejoice in the names of chlorophyll, xantophyll, erythrophyll, chrysophyll, and phaiophyll, the absorption spectra of which are perfectly distinct and well marked. It is found generally that leaves contain colours belonging to several groups, and frequently more than one of the same group. Mr. Sorby also finds that the change of colour which takes place in autumn consists chiefly in the disappearance of the chlorophyll, which renders the remaining colours visible, and these most frequently are of a yellowish tint. Some leaves, however, turn red in the autumn: this appears to be due to a falling off of the vital power of the plant, for by artificially diminishing the vital power, the intensity of this red colour is increased.

One great value of this method of research is that it enables us to recognise special colouring-matters, even when mixed with several others, and to determine the particular conditions in which they occur in plants or animals—whether in a solid state or in solution—and whether those dissolved out by reagents exist as such in the living organisms, or are the products of decompositions.

So that you see, on the whole, at the present moment, I think we may be full of hope that the new process may gradually lead to many more practical applications; but really we cannot say much about them at present, because the introduction of spectrum analysis is so recent. We are, however, already furnished with another instance of the close connection there always must be between any great advance in physical inquiry and the application of the skill of our opticians to aid us in the inquiry. We have the Sorby-Browning spectrum microscope, and then a large number of people can study the beautiful phenomena which this new method of research has opened up to us, where formerly it was almost impossible to imagine that science, or even the practical affairs of earth, should in any way benefit.

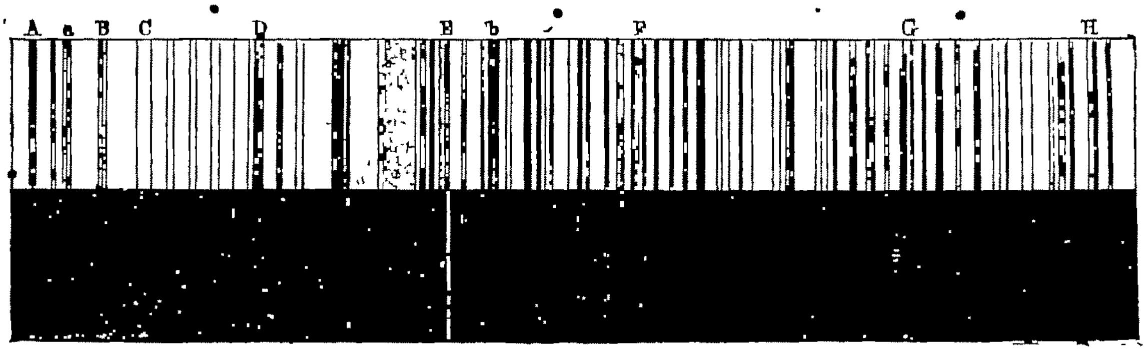

FIG. 49.—Correspondence of some of the lines given out by iron vapour (below), and of some of the Fraunhofer lines in the solar spectrum.

Having thus dealt very briefly with some of the more practical applications of the subject, I must now take you a somewhat distant journey to the sun and to the stars; and I must, in the first instance, attempt to connect the two perfectly distinct classes of phenomena which I have brought to your notice,—the phenomena, namely, of radiation, and the phenomena of absorption; and this connection between radiation and absorption is an instance of the slow growth of science. I remarked to you in the former lecture, that Fraunhofer, at the beginning of this century, had a very shrewd suspicion of the perfect coincidence of place in the spectrum between certain dark lines which he saw in the spectrum of the sun, which I promised to explain to you on this occasion, and the bright lines in the spectrum of sodium. You know how very simple the spectrum of sodium is: you will, perhaps, think it very strange indeed that such a simple thing was not explained very long ago. But Fraunhofer at the first suspected, and after him many of our greatest minds suspected, that there was some hidden, wondrously strange, connection between the double yellow line which you will remember is characteristic of sodium, and a certain double line which exists among the strange black lines of the solar spectrum, which I begged you to banish from your minds on the last occasion, when we were merely dealing with radiation. But now I must ask you to bear with me while I attempt to make clear to you all the strange facts concerning these black lines. I have been favoured by Dr. Gladstone with an extract from Dr. Brewster's notebook, dated St. Andrews, October 28, 1841. In it Brewster says:—"I have this evening discovered the remarkable fact that, in the combustion of nitre upon charcoal, there are definite bright rays corresponding to the double lines of A and B, and the group of lines *a* in the space A B. The coincidence of two yellow rays with the two deficient ones at D, with the existence of definite bright rays in the nitre flame, not only at D but at A, *a* and B, is so extraordinary, that it indicates some regular connection between the two classes of phenomena." The double lines A and B refer to some of these dark Fraunhofer lines in the solar spectrum, which for convenience of reference were at first called after the letters of the alphabet; we now find that their number is so enormous that it is absolutely impossible to attempt to grapple with them in any such method, but these names are still retained.

The explanation of the coincidence between the two bright lines of burning sodium vapour and the two dark lines D in the solar spectrum was first given by Prof. Stokes about 1852.

It is this. The light emitted by an incandescent vapour is due to the vibrations of its molecules, as a sound note emitted by a piano wire is due to the vibration of the wire. You have only to go into a room where there is a piano, and sing a note, to find that the wire which corresponds to your note will respond to your voice. Now, in the same way, when light is passing through a vapour, the molecules of which vibrate at any particular rate, they will be urged into their own special rate of vibrations by the vibrations of the light which correspond to that particular rate which is passing through them. Hence the light will, so to speak, be sifted, and the force it has exercised in impelling the particles in the interrupting vapour to vibrate will tell upon it; and in this way those particular vibrations which have had the work to do will be enfeebled.

It is clear that the parts of the spectrum thus reduced in brilliancy will depend upon the vapour through which the light has passed. If sodium vapour be traversed, then the light corresponding to the bright lines of sodium will be enfeebled.

This great law, to which the researches of Stokes and Stewart and Angström have led, and which has been established by the experiments of Foucault, Kirchhoff, and Bunsen, may be summed up as follows :—*Gases and vapours, when relatively cool, absorb those rays which they themselves emit when incandescent*; the absorption is continuous or selective as the radiation is continuous or selective.

J. NORMAN LOCKYER

(*To be continued.*)

NOTES

THE Emperor of Brazil has conferred upon Dr. Warren De La Rue the distinction of Knight of the Imperial Order of the Rose.

THE subject of Professor Tait's Rede Lecture, to be delivered on the 23rd inst., will be "Thermo-Electricity."

A PARAGRAPH has recently appeared in several scientific papers quoted from the *Zeitschrift fur Parasitenkunde*, stating that Prof. Hallier of Jena has described a new potato-disease, which made its appearance last autumn in the neighbourhood of that town, the disease being indicated by the presence of a purple web and the appearance of a number of black spots on the skin, referable apparently to the perithecia of a pyrenomycetous fungus. We learn from the Rev. M. J. Berkeley that this so-called new disease is nothing but the well-known "copper-web" which is in some years very destructive to asparagus, mint, and other crops, and has been known in some instances to attack the potato. The description in the *Zeitschrift* is identical with this familiar parasite. Figures will be found in Tulasne's "Fungi Hypogæi," under Rhizoctonia, showing that the so-called perithecia are spurious. Mr. Broome has detected the form of fructification known as conidia.

LADY LYELL, wife of Sir Charles Lyell, Bart, F.R.S., died last Thursday, in her 65th year. Her ladyship was the eldest daughter of the late Mr. Leonard Horner, F.R.S.

DURING the Easter term the following lectures in natural sciences will be given at Cambridge :—On Heat (1) Advanced (for the Natural Sciences Tripos), by Mr. Trotter, Trinity College, in Lecture-room No. 11, on Mondays, Wednesdays, and Fridays at 10, commencing Wednesday, April 30 (2) Elementary (for Special Examination and 1st Part of Natural Sciences Tripos), on Tuesdays, Thursdays, and Saturdays at 11, commencing Tuesday, April 29. On Chemistry, by Mr. Main, St. John's College, on Tuesdays, Thursdays, and Saturdays at 12, in St. John's College Laboratory, commencing Thursday, April 24. Instruction in Practical Chemistry will also be given. On Palæontology—the Mollusca, &c., by Mr. Bonney, St. John's College, on Tuesdays and Thursdays, at 9, commencing Thursday, April 24. On Geology—(for the Natural Sciences Tripos, Stratigraphical Geology), by Mr. Bonney, St. John's College, on Mondays, Wednesdays, and Fridays, at 10, commencing Wednesday, April 23. Elementary Geology (for the First part of the Tripos and the special examination), on Tuesdays and Thursdays, at 11, commencing Thursday, April 24; there will be excursions, of which notice will be given from time to time. On Botany (for the Natural Sciences Tripos), by Mr. Hicks, Sidney College, on Tuesdays, Thursdays, and Saturdays, at 11, in Lecture-room No. 1, beginning on Tuesday, April 29; the lectures during this term will be chiefly on Cryptogamic Botany and on Classification. Biology: the Trinity Prælector will give a course of Practical Lectures on Elementary Biology, on Mondays, Tuesdays, and Wednesdays, at 11 A.M., commencing Wednesday, April 30. This course is intended as an introduction to the study of both anatomy and physiology. A short lecture of about half-an-hour will be given at each meeting, followed by practical work for about 1½ or 2 hours.

THE annual *soirée* of the Royal Society last Saturday at Burlington House was a great success. The number of visitors was exceedingly large, and the objects exhibited were numerous and varied. In the Mathematical Room, Mr. Latimer Clark showed his remarkable experiment of the influence of light on the conductivity of selenium, recently described in NATURE.

THE office of "Lord Rector" of a Scotch University is generally regarded as merely honorary, a testimony of the estimation in which the students hold the gentleman whom they elect. As a rule the Lord Rector acquiesces in this opinion, and seldom does more in return for the supposed honour conferred than mark the commencement or close of his three years' tenure of office by making a speech to the students. As might be surmised, Prof. Huxley, who was recently elected to the Lord Rectorship of Aberdeen University, which counts Prof. Bain among its staff of teachers, does not regard the office as merely honorary: he intends to take advantage of the position conferrred upon him by doing some actual work for the good of the University. Naturally one of the first grievances he has attacked is the medical curriculum, which at Aberdeen, as at most other medical schools, is hampered by the "traditions of the elders" as to the supposed advantages of the dead languages to a medical student. Shortly after Prof. Huxley's election, he received a numerously signed petition from the medical students requesting him to use his influence to obtain the omission of Greek as a compulsory subject in the preliminary examination. Prof. Huxley has given notice that he will bring forward at the next meeting of the University Court a resolution to reform the medical curriculum at Aberdeen, as he considers it at present rather overweighted with classics, and believes that some new arrangement would probably be exceedingly advantageous, especially in the matters of natural history and botany.

WE hear from Mr. Lloyd that living specimens of the Lancelet (*Amphioxus lanceolatus*) have been very recently received at the Crystal Palace Aquarium, from Naples, and are now alive. We hope that Dr. Dohrn will be successful in sending other living specimens of this most interesting fish to other Aquaria in this country, so that its affinities and development may be more thoroughly worked out and generally understood.

MR. THOMAS WILLIAM BRIDGE was on Friday elected to a Natural Science Scholarship at Trinity College, Cambridge. Mr. Bridge has for some two years worked under Mr. J. W. Clark, the Superintendent of the University Museums of Zoology and Comparative Anatomy, and about a month since was appointed, by the Professor of Zoology, to the newly-founded post of Demonstrator in Comparative Anatomy in the University.

DR. DIVERS, of the Middlesex Hospital, has been appointed to the Professorship of Chemistry in the new Engineering College at Jeddo.

PROF. AGASSIZ has not been behindhand in employing the advantages placed at his disposal by Mr. Anderson's munificent bequest. A programme is already published of a summer course of Natural History at Penekese Island, designed chiefly for teachers, and for students preparing to become teachers. Among those that Prof. Agassiz is able to include on his staff we find the names of Profs. Shaler, Wilder, Packard, and Putnam, and every attempt is being made to obtain a sufficient endowment, through the liberality of others, to offer the course free of charge to deserving students. The Superintendent of the United States Coast Survey and the United States Commissioner of Fisheries have also promised all the assistance in their power to this excellent undertaking.

DR. CHARLES C. ABBOTT has discovered in the river drift at Trenton, New Jersey, in gravel at great depth, and beneath undisturbed layers of fine sand, three chipped implements, of unquestionably human manufacture, lying close to each other. One has a knife-like form, 9 in. long, made of a reddish-brown stone, compact, laminated, and susceptible of a high polish. The other two bear a considerable resemblance to common European forms: one is of opaque yellowish quartz, 5¼ in. long, and 1¼ in. in greatest width; the other is a flake of sand-stone rock, 6¼ in. long, 3¼ in. wide. From the occurrence of such specimens so near each other, Dr. Abbott thinks that we must admit that the antiquity of American man is greater than the advent of the so-called "Indian."

THE Royal Geographical Society have awarded the following medals for the present year:—In Physical Geography: Gold medal to W. G. Hudson, age 18, of Liverpool College; bronze medal to W. A. Forbes, age 17, of Winchester College. In Political Geography: Gold medal to S. E. Spring Rice, age 16, of Eton College; bronze medal to A. T. Nutt, age —, of University College School.

AT the meeting of the Royal Geographical Society on Monday, Sir Henry Rawlinson said despatches with reference to the East Coast Livingstone Expedition had been received from Sir Bartle Frere, dated March 27. The English portion of the expedition had been recently materially augmented, for, instead of consisting as previously of Lieut. Cameron and Dr. Dillan, it had received the valuable services of Lieut. Murphy, an officer of Engineers, who had obtained permission from the Indian Government to join it. Mr. Moffatt, a nephew of Dr. Livingstone, had also joined the expedition, and there was every reason to expect that his assistance would be of the greatest use in time of need. Bergamoyo had been already reached, and by the latest accounts the march into the interior had been commenced. From the first camp, at a distance of twenty miles from Bergamoyo, communications had been received from Dr. Dillan, in which he intimated his expectation of being speedily joined by Lieut. Cameron, Lieut. Murphy, and Dr. Moffatt. They would, notwithstanding the fact that the rainy season was not yet over, at once proceed on their journey.

PROF. THISELTON DYER announces a course of six lectures on the "Aspects of Vegetation" at the Royal Horticultural Society's Gardens; and Mr. Thomas Moore a course of six demonstrations on "Medical Botany" in the Chelsea Botanic Garden.

A TWICE-MONTHLY scientific periodical, in Turkish, is to be brought out in Constantinople called the *Dolab*, the *Repository*.

ON Jan. 31 there was a slight shock of earthquake at Rangoon in English Burmah. On Feb. 12 an earthquake was felt at Peshawur and Lahore in India. Slight earthquake shocks were felt on March 14, at 8 P.M., at Yanina (Janina) in Albania, Turkey.

THERE is a report from Doncaster to the effect that shortly after two o'clock on Tuesday afternoon the town was visited by a smart shock of earthquake, which shook several houses to their foundations. In our correspondence this week will be found an account of an earthquake which occurred recently in the south of Scotland.

THE French Association for the Advancement of Science, commences its second annual session at Lyons on August 21. We believe that there is every hope of a most numerous and interesting meeting.

THE *New York Journal of Applied Chemistry* for February contains a very excellent article on "The Promotion of Scientific Research," by Prof. C. A. Joy, in which he animadverts severely on those so-called "practical men" who test the value of all scientific investigation by the "What is the use?" standard. "Original research," the writer says, "is the nervous fluid that furnishes strength to the muscle. The brawny arm is but dead meat unless the body is fed with nourishing food. Theodore Parker, in one of his discourses, alludes to the figure of a Chinaman in a shop window turning vigorously a crank; upon investigation he found that it was the crank that turned the man, and not the man the crank. It is the same with practical applications. The practical man applies the principle, and with great pomp and arrogance claims to turn the crank; it is not true—a power higher than his is behind it all; the original investigation, the discovery of the principle upon which the movement rests, is really the engine that drives the man and makes him do its bidding." Prof. Joy in speaking of the recent article in NATURE, in which Sir Benjamin Brodie calls attention to the enormous expenditure of money of the University of Oxford in the way of subsidies to students and annuities to fellows, without any adequate results, counsels the Americans to forbear copying the English University system. He proposes the following plan of promoting scientific research:—Let there be incorporated a society for the promotion of scientific research, to consist of a small number of strictly scientific trustees, who shall hold the property and appropriate the income to such objects as they deem worthy of aid. It would not be, strictly speaking, a society, but a foundation for the purposes specified. The head-quarters of the corporation should be in New York City. If the wealthy citizens of New York, who owe all they possess to the progress of science, would give money into the hands of such a board of trustees, they would be doing a most important work. Wherever and whenever any person was known to be engaged in the prosecution of some scientific research, the trustees could make him an allowance for conducting the inquiry, or to enable him to publish his results. Such assistance would often secure important discoveries. There are numerous professors scattered over the country whose salary is so small that they are obliged to add to it by outside work, or whose services at the college are so pressing that they have no leisure for anything like voluntary labour. A little assistance and encouragement to such persons would go a great way. Any college would be flattered by having their officers thus singled out by the best judges of the country as worthy of a subsidy from a society founded to encourage research. This course is preferable to giving a fund to a college for educational purposes, or to found a professorship, as the means for education are very great in this country, and there is far less need of mere educational facilities than there is of men engaged in purely scientific study. It has often happened that money has been raised to found a professorship for a particularly able man; after his death a person of inferior ability takes his place, and thus the object of the donor is defeated. It is therefore better to put the money into the hands of trustees selected for the purpose, and let them pay the income to those who are known to be worthy to receive it. The demands upon the fortunes of our wealthy men are constant and numerous, and they naturally give to such objects as are within their comprehension. If they could be made to understand that the source of our prosperity is science, and that the springs of discovery whence flow all the improvements of the day must be kept perennial, they would freely give of their substance, and we should soon see the watch-fires of original research kindled over the whole country.

THE New York Nautical School-ship *Mercury* has spent the past winter in deep-sea research, as in a previous season, and, as before, has utilised the opportunities presented in the interest of science. Captain Giraud surveyed a large portion of the so-called "volcanic region" of the Atlantic Ocean, finding the water very deep in that vicinity. Specimens brought up from the bottom appeared to be of undoubted volcanic origin. The Casella-Miller deep-sea thermometer was used on one occasion at a depth of 2,040 fathoms, two miles north of the equator, in longitude 22° 16′ west, and indicated a temperature of 35° F., at 1,000 fathoms 38°, and at the surface 81°, the air being 80°. During the voyage from the Canary Islands to Rio the temperature at uniform depths was found to vary only about two degrees.

THE Iron-Steel Institute conclude their meeting at Willis's Rooms to-day.

PRIZES for papers on the "Elvan Courses" of Cornwall, are offered by Mr. J. A. Phillips, F.C.S., to the present and former pupils of the Miners' Association of Cornwall and Devon. The papers and illustrative specimens are to be deposited with Mr. J. H. Collins, F.G.S., Hon. Assistant Secretary of the Miners' Association, Polytechnic Hall, Falmouth, on or before Sept. 1, 1873. The author of the best paper will be entitled to a prize (in books, selected by himself) of the value of 5*l.* A second prize, also in books, of the value of 3*l.*, will be given to the author of the paper next in order of merit.

WE have received the first number of a new American journal, started last month, *The Sanitarian*, edited by Dr. A. N. Bell, of New York. It aims at presenting the results of the various inquiries which have been, and which hereafter may be made, for the preservation of health and the expectations of human life, so as to make them most advantageous to the public and to the medical profession. Among the most important articles is one by the editor, on "The New York Quarantine Establishment," which is illustrated with two maps. This is preceded by one on "Infant Mortality, with Suggestions for improving the condition of Foundlings;" and followed by another on "The necessity of Re-Vaccination." We strongly recommend this excellently conducted journal to those interested in sanitary science.

AMONG the rarer and more interesting remains found in the mounds of the west of America, are plates of mica cut into different shapes, and evidently preserved as objects of great rarity and value; and, in the absence of this mineral in the Mississippi Valley, the question has frequently arisen whence the material could have been derived. A recent communication from Prof. W. C. Kerr, the State Geologist of North Carolina, tends to throw some light on this subject, and to open an interesting chapter in regard to the American prehistoric man. The work of collecting mica is at present carried on upon the largest scale in the high and rugged region between the Black Mountain, the Roanoke, and the head waters of the Nolachuchy, principally in Mitchell County, North Carolina. The region in question has long been known for the existence of numerous open works and tunnels, which, at first sight, were supposed to have been made in the search for silver or some other valuable metal. Prof. Kerr, in his capacity of State Geologist, was led to investigate this question, and very soon found, in every instance, that the excavations referred to were much older than the earliest discovery of the country by the Spaniards, and that in all cases they were found in ledges of coarse granite, which contained nothing but large patches of mica. Prof. Kerr has been satisfied for some time that in these mines we have the work of the contemporaries of the mound-builders, and the localities whence they derived the mica. What use they made of it we cannot say; but it is suggested that it may have served the purpose of mirrors, or possibly have been used as windows, as well as for ornament. The number and size of these mines is remarkable, some of the open cuts being more than 100 ft. in diameter, and 20 ft. or 30 ft. in depth, even after the caving in and filling up of centuries of weathering. The tunnels often extend inwards several yards, but are said to be too small for a man of ordinary size to work in. These show distinct marks of the tool in the granitic wall, as if made by a chisel-shaped instrument about an inch broad. Numerous plates of mica are found in these tunnels and excavations, some of them trimmed to particular shapes. These facts open up a new chapter in the history of the American aborigines, illustrating the character of the commerce carried on at a very remote period, and showing the magnitude of the operations, and the extended period of time over which they must have been prosecuted, to enable a people furnished with nothing better than wooden and stone tools to produce excavations of so great magnitude.

Sirius, a journal of popular astronomy published at Leipzig and Vienna, contains, in its fourth number for this year, a lecture by Prof. Oppolzer, on "The Importance of Astronomy in connection with Ancient History," the continuation of an article on "Copernicus and his Anniversary," one of a series of articles on the "Topography of the Heavens," the present treating of the constellation Gemini, besides a few notes.

THE additions to the Zoological Society's Gardens during the last week include a Ring-necked Parakeet (*Palæornis torquata*) from India, presented by Mr. W. E. Johnson; a long-eared Owl (*Otus vulgaris*) from Europe, presented by Dr. Bree; a Wood Owl (*Syrnium aluco*), presented by Mr. H. W. L. Browne; a Chinese Harrier (*Circus spilonotus*); a grey Eagle Owl (*Bubo cinereus*) and a Bosman's Potto (*Perodicticus potto*) from W. Africa; a horned Tragopan (*Ceriornis satyra*) from the Himalayas; a Black-tailed Hawfinch (*Coccothraustes melanurus*) from Japan; two crested Buntings (*Melophus melanicterus*); two red-eared Bulbuls (*Pycnonotus jocosus*), and a red-vented Bulbul (*P. hæmorrhous*) from India; a red-headed Bunting (*Emberiza rutila*), and a yellow-browed Bunting (*E. chrysophrys*) from Japan; a black Tanager (*Tachyphonus melaleucus*) from S. America, purchased; two Emus (*Dromæus novæ-hollandiæ*) from Australia, deposited; a great Kangaroo (*Macropus giganteus*), and a Derbian Wallaby (*Halmaturus derbianus*), born in the gardens.

ON THE HYPOTHESES WHICH LIE AT THE BASES OF GEOMETRY*

Plan of the Investigation

IT is known that geometry assumes, as things given, both the notion of space and the first principles of constructions in space. She gives definitions of them which are merely nominal, while the true determinations appear in the form of axioms. The relation of these assumptions remains consequently in darkness; we neither perceive whether and how far their connection is necessary, nor, *a priori*, whether it is possible.

From Euclid to Legendre (to name the most famous of modern reforming geometers) this darkness was cleared up neither by mathematicians nor by such philosophers as concerned themselves with it. The reason of this is doubtless that the general notion of multiply extended magnitudes (in which space-magnitudes are included) remained entirely unworked. I have in the first place, therefore, set myself the task of constructing the notion of a multiply extended magnitude out of general notions of magnitude. It will follow from this that a multiply extended magnitude is capable of different measure-relations, and consequently that space is only a particular case of a triply extended magnitude. But hence flows as a necessary consequence that the propositions of geometry cannot be derived from general notions of magnitude, but that the properties which distinguish space from other conceivable triply extended magnitudes are only to be deduced from experience. Thus arises the problem, to discover the simplest matters of fact from which the measure-relations of space may be determined; a problem which from the nature of the case is not completely determinate, since there may be several systems of matters of fact which suffice to determine the measure-relations of space—the most important system for our present purpose being that which Euclid has laid down as a foundation. These matters of fact are—like all matters of fact—not necessary, but only of empirical certainty; they are hypotheses. We may therefore investigate their probability, which within the limits of observation is of course very great, and inquire about the justice of their extension beyond the limits of observation, on the side both of the infinitely great and of the infinitely small.

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* By Bernhard Riemann. (Translated by Prof. W. K. Clifford, from vol. xiii. of the Gottinger Abhandlungen.)

I.—*Notion of an* n*-ply extended magnitude*

In proceeding to attempt the solution of the first of these problems, the development of the notion of a multiply extended magnitude, I think I may the more claim indulgent criticism in that I am not practised in such undertakings of a philosophical nature where the difficulty lies more in the notions themselves than in the construction; and that besides some very short hints on the matter given by Privy Councillor Gauss in his second memoir on Biquadratic Residues, in the "Gottingen Gelehrte Anzeige," and in his Jubilee-book, and some philosophical researches of Herbart, I could make use of no previous labours.

§ 1.—Magnitude-notions are only possible where there is an antecedent general notion which admits of different specialisations. According as there exists among these specialisations a continuous path from one to another or not, they form a *continuous* or *discrete* manifoldness; the individual specialisations are called in the first case points, in the second case elements, of the manifoldness. Notions whose specialisations form a *discrete* manifoldness are so common that at least in the cultivated languages any things being given it is always possible to find a notion in which they are included. (Hence mathematicians might unhesitatingly found the theory of discrete magnitudes upon the postulate that certain given things are to be regarded as equivalent.) On the other hand, so few and far between are the occasions for forming notions whose specialisations make up a *continuous* manifoldness, that the only simple notions whose specialisations form a multiply extended manifoldness are the positions of perceived objects and colours. More frequent occasions for the creation and development of these notions occur first in the higher mathematic.

Definite portions of a manifoldness, distinguished by a mark or by a boundary, are called Quanta. Their comparison with regard to quantity is accomplished in the case of discrete magnitudes by counting, in the case of continuous magnitudes by measuring. Measure consists in the superposition of the magnitudes to be compared; it therefore requires a means of using one magnitude as the standard for another. In the absence of this two magnitudes can only be compared when one is a part of the other; in which case also we can only determine the more or less and not the how much. The researches which can in this case be instituted about them form a general division of the science of magnitude in which magnitudes are regarded not as existing independently of position and not as expressible in terms of a unit, but as regions in a manifoldness. Such researches have become a necessity for many parts of mathematics, *e.g.*, for the treatment of many-valued analytical functions; and the want of them is no doubt a chief cause why the celebrated theorem of Abel and the achievements of Lagrange, Pfaff, Jacobi for the general theory of differential equations, have so long remained unfruitful. Out of this general part of the science of extended magnitude in which nothing is assumed but what is contained in the notion of it, it will suffice for the present purpose to bring into prominence two points; the first of which relates to the construction of the notion of a multiply extended manifoldness, the second relates to the reduction of determinations of place in a given manifoldness to determinations of quantity, and will make clear the true character of an n-fold extent.

§ 2.—If in the case of a notion whose specialisations form a continuous manifoldness, one passes from a certain specialisation in a definite way to another, the specialisations passed over form a simply extended manifoldness, whose true character is that in it a continuous progress from a point is possible only on two sides, forwards or backwards. If one now supposes that this manifoldness in its turn passes over into another entirely different, and again in a definite way, namely so that each point passes over into a definite point of the other, then all the specialisations so obtained form a doubly extended manifoldness. In a similar manner one obtains a triply extended manifoldness, if one imagines a doubly extended one passing over in a definite way to another entirely different; and it is easy to see how this construction may be continued. If one regards the variable object instead of the determinable notion of it, this construction may be described as a composition of a variability of $n+1$ dimensions out of a variability of n dimensions and a variability of one dimension.

§ 3.—I shall now show how conversely one may resolve a variability whose region is given into a variability of one dimension and a variability of fewer dimensions. To this end let us suppose a variable piece of a manifoldness of one dimension—reckoned from a fixed origin, that the values of it may be comparable with one another—which has for every point of the given manifoldness a definite value, varying continuously with the point; or, in other words, let us take a continuous function of position within the given manifoldness, which, moreover, is not constant throughout any part of that manifoldness. Every system of points where the function has a constant value, forms then a continuous manifoldness of fewer dimensions than the given one. These manifoldnesses pass over continuously into one another as the function changes; we may therefore assume that out of one of them the others proceed, and speaking generally this may occur in such a way that each point passes over into a definite point of the other; the cases of exception (the study of which is important) may here be left unconsidered. Hereby the determination of position in the given manifoldness is reduced to a determination of quantity and to a determination of position in a manifoldness of less dimensions. It is now easy to show that this manifoldness has $n-1$ dimensions when the given manifoldness is n-ply extended. By repeating then this operation n times, the determination of position in an n-ply extended manifoldness is reduced to n determinations of quantity, and therefore the determination of position in a given manifoldness is reduced to a finite number of determinations of quantity *when this is possible*. There are manifoldnesses in which the determination of position requires not a finite number, but either an endless series or a continuous manifoldness of determinations of quantity. Such manifoldnesses are, for example, the possible determinations of a function for a given region, the possible shapes of a solid figure, &c.

II.—*Measure-relations of which a manifoldness of* n *dimensions is capable on the assumption that lines have a length independent of position, and consequently that every line may be measured by every other.*

Having constructed the notion of a manifoldness of n dimensions, and found that its true character consists in the property that the determination of position in it may be reduced to n determinations of magnitude, we come to the second of the problems proposed above, viz., the study of the measure-relations of which such a manifoldness is capable, and of the conditions which suffice to determine them. These measure-relations can only be studied in abstract notions of quantity, and their dependence on one another can only be represented by formulæ. On certain assumptions, however, they are decomposable into relations which, taken separately, are capable of geometric representation; and thus it becomes possible to express geometrically the calculated results. In this way, to come to solid ground, we cannot, it is true, avoid abstract considerations in our formulæ, but at least the results of calculation may subsequently be presented in a geometric form. The foundations of these two parts of the question are established in the celebrated memoir of Gauss—"Disquisitiones generales circa superficies curvas."

§ 1.—Measure-determinations require that quantity should be independent of position, which may happen in various ways. The hypothesis which first presents itself, and which I shall here develop, is that according to which the length of lines is independent of their position, and consequently every line is measurable by means of every other. Position-fixing being reduced to quantity fixings, and the position of a point in the n-dimensioned manifoldness being consequently expressed by means of n variables x_1, x_2, x_3, . . . x_n, the determination of a line comes to the giving of these quantities as functions of one variable. The problem consists then in establishing a mathematical expression for the length of a line, and to this end we must consider the quantities x as expressible in terms of certain units. I shall treat this problem only under certain restrictions, and I shall confine myself in the first place to lines in which the ratios of the increments dx of the respective variables vary continuously. We may then conceive these lines broken up into elements, within which the ratios of the quantities dx may be regarded as constant; and the problem is then reduced to establishing for each point a general expression for the linear element ds starting from that point, an expression which will thus contain the quantities x and the quantities dx. I shall suppose, secondly, that the length of the linear element, to the first order, is unaltered when all the points of this element undergo the same infinitesimal displacement, which implies at the same time that if all the quantities dx are increased in the same ratio, the linear element will vary also in the same ratio. On these suppositions, the linear element may be any homogeneous function of the first degree of the quantities dx, which is unchanged when we change the signs of all the dx, and in which the arbitrary constants are continuous functions of the quantities x. To find the simplest cases, I shall seek first an expression for manifoldnesses of $n-1$ dimensions which are everywhere equidistant from the origin of the linear element; that is, I shall seek a continuous function of position whose values distinguish them from one another. In going outwards from the origin, this must either increase in all directions or decrease in all directions; I assume that it increases in all directions, and therefore has a minimum at that point. If, then, the first and second differential coefficients of this function are finite, its first differential must vanish, and the second differential cannot become negative; I assume that it is always positive. This differential expression, then, of the second order remains constant when ds remains constant, and increases in the duplicate ratio when the dx, and therefore also ds, increase in the same ratio; it must therefore be ds^2 multiplied by a constant, and consequently ds is the square root of an always positive integral homogeneous function of the second order of the quantities dx, in which the coefficients are continuous functions of the quantities x. For Space, when the position of points is expressed by rectilinear co-ordinates, $ds=\sqrt{\Sigma(dx)^2}$; Space is therefore included in this simplest case. The next case in simplicity includes those manifoldnesses in which the line-element may be expressed as the fourth root of a quartic differential expression. The investigation of this more general kind would require no really different principles, but would take considerable time and throw little new light on the theory of space, especially as the results cannot be geometrically expressed; I restrict myself, therefore, to those manifoldnesses in which the line-element is expressed as the square root of a quadric differential expression. Such an expression we can transform into another similar one if we substitute for the n independent variables functions of n new independent variables. In this way, however, we cannot transform any expression into any other; since the expression contains $n\frac{n+1}{2}$ coefficients which are arbitrary functions of the independent variables; now by the introduction of new variables we can only satisfy n conditions, and therefore make no more than n of the coefficients equal to given quantities. The remaining $n\frac{n-1}{2}$ are then entirely determined by the nature of the continuum to be represented, and consequently $n\frac{n-1}{2}$ functions of positions are required for the determination of its measure-relations. Manifoldnesses in which, as in the Plane and in Space, the line-element may be reduced to the form $\sqrt{\Sigma dx^2}$, are therefore only a particular case of the manifoldnesses to be here investigated; they require a special name, and therefore these manifoldnesses in which the square of the line-element may be expressed as the sum of the squares of complete differentials I will call *flat*. In order now to review the true varieties of all the continua which may be represented in the assumed form, it is necessary to get rid of difficulties arising from the mode of representation, which is accomplished by choosing the variables in accordance with a certain principle.

§. 2.—For this purpose let us imagine that from any given point the system of shortest lines going out from it is constructed; the position of an arbitrary point may then be determined by the initial direction of the geodesic in which it lies, and by its distance measured along that line from the origin. It can therefore be expressed in terms of the ratios dx_0 of the quantities dx in this geodesic, and of the length s of this line. Let us introduce now instead of the dx_0 linear functions dx of them, such that the initial value of the square of the line-element shall equal the sum of the squares of these expressions, so that the independent variables are now the length s and the ratios of the quantities dx. Lastly, take instead of the dx quantities $x_1 x_2 x_3 \ldots x_n$ proportional to them, but such that the sum of their squares $= s^2$. When we introduce these quantities, the square of the line-element is Σdx^2 for infinitesimal values of the x, but the term of next order in it is equal to a homogeneous function of the second order of the $n\frac{n-1}{2}$ quantities $(x_1\,dx_2-x_2\,dx_1)$, $(x_1\,dx_3-x_3\,dx_1)\ldots$ an infinitesimal, therefore, of the fourth order; so that we obtain a finite quantity on dividing this by the square of the infinitesimal triangle, whose vertices are $(0,0,0,\ldots)$, $(x_1\,x_2\,x_3\,\ldots)$, $(dx_1,\,dx_2,\,dx_3,\ldots)$. This quantity retains the same value so long as the x and the dx are included in the same binary linear form, or so long as the two geodesics from 0 to x and from 0 to dx remain in the same surface-element; it depends therefore only on place and direction. It is obviously zero when the manifold represented is flat, *i.e.* when the squared line-element is reducible to Σdx^2, and may therefore be regarded as the measure of the deviation of the manifoldness from flatness at the given point in the given surface-direction. Multiplied by $-\frac{3}{4}$ it becomes equal to the quantity which Privy-councillor Gauss has called the total curvature of a surface. For the determination of the measure-relations of a manifoldness capable of representation in the assumed form we found that $n\frac{n-1}{2}$ place-functions were necessary; if, therefore, the curvature at each point in $n\frac{n-1}{2}$ surface-directions is given, the measure-relations of the continuum may be determined from them—provided there be no identical relations among these values, which in fact, to speak generally, is not the case. In this way the measure-relations of a manifoldness in which the line-element is the square root of a quadric differential may be expressed in a manner wholly independent of the choice of independent variables. A method entirely similar may for this purpose be applied also to the manifoldness in which the line element has a less simple expression, *e.g.*, the fourth root of a quartic differential. In this case the line-element, generally speaking, is no longer reducible to the form of the square root of a sum of squares, and therefore the deviation from flatness in the squared line-element is an infinitesimal of the second order, while in those manifoldnesses it was of the fourth order. This property of the last-named continua may thus be called flatness of the smallest parts. The most important property of these continua for our present purpose, for whose sake alone they are here investigated, is that the relations of the twofold ones may be geometrically represented by surfaces, and of the morefold ones may be reduced to those of the surfaces included in them; which now requires a short further discussion.

§ 3.—In the idea of surfaces, together with the intrinsic measure-relations in which only the length of lines on the surfaces is considered, there is always mixed up the position of points lying out of the surface. We may, however, abstract from external relations if we consider such deformations as leave unaltered the length of lines—*i.e.* if we regard the surface as bent in any way without stretching, and treat all surfaces so related to each other as equivalent. Thus, for example, any cylindric or conical surface counts as equivalent to a plane, since it may be made out of one by mere bending, in which the intrinsic measure-relations remain, and all theorems about a plane—therefore the whole of planimetry—retain their validity. On the other hand they count as essentially different from the sphere, which cannot be changed into a plane without stretching. According to our previous investigation the intrinsic measure-relations of a twofold extent in which the line-element may be expressed as the square root of a quadric differential, which is the case with surfaces, are characterised by the total curvature. Now this quantity in the case of surfaces is capable of a visible interpretation, viz., it is the product of the two curvatures of the surface, or multiplied by the area of a small geodesic triangle, it is equal to the spherical excess of the same. The first definition assumes the proposition that the product of the two radii of curvature is unaltered by mere bending; the second, that in the same place the area of a small triangle is proportional to its spherical excess. To give an intelligible meaning to the curvature of an n-fold extent at a given point and in a given surface-direction through it, we must start from the fact that a geodesic proceeding from a point is entirely determined when its initial direction is given. According to this we obtain a determinate surface if we prolong all the geodesics proceeding from the given point and lying initially in the given surface-direction; this surface has at the given point a definite curvature, which is also the curvature of the n-fold continuum at the given point in the given surface-direction.

§ 4.—Before we make the application to space, some considerations about flat manifoldnesses in general are necessary; *i.e.* about those in which the square of the line-element is expressible as a sum of squares of complete differentials.

In a flat n-fold extent the total curvature is zero at all points in every direction; it is sufficient, however (according to the preceding investigation), for the determination of measure-relations, to know that at each point the curvature is zero in $n\frac{n-1}{2}$ independent surface directions. Manifoldnesses whose curvature is constantly zero may be treated as a special case of those whose curvature is constant. The common character of these continua whose curvature is constant may be also expressed thus, that figures may be moved in them without stretching. For clearly figures could not be arbitrarily shifted and turned round in them if the curvature at each point were not the same in all directions. On the other hand, however, the measure-relations of the manifoldness are entirely determined by the curvature; they are therefore exactly the same in all directions at one point as at another, and consequently the same constructions can be made from it: whence it follows that in aggregates with constant curvature figures may have any arbitrary position given them. The measure-relations of these manifoldnesses depend only on the value of the curvature, and in relation to the analytic expression it may be remarked that if this value is denoted by α, the expression for the line-element may be written

$$\frac{1}{1+\frac{\alpha}{4}\Sigma x^2}\sqrt{\Sigma dx^2}$$

§ 5.—The theory of *surfaces* of constant curvature will serve for a geometric illustration. It is easy to see that surfaces whose curvature is positive may always be rolled on a sphere whose radius is unity divided by the square root of the curvature; but to review the entire manifoldness of these surfaces, let one of them have the form of a sphere and the rest the form of surfaces of revolution touching it at the equator. The surfaces with greater curvature than this sphere will then touch the sphere internally, and take a form like the outer portion (from the axis) of the surface of a ring; they may be rolled upon zones of spheres having less radii, but will go round more than once. The surfaces with less positive curvature are obtained from spheres of larger radii, by cutting out the lune bounded by two great half-circles and bringing the section-lines together. The surface with curvature zero will be a cylinder standing on the equator; the surfaces with negative curvature will touch the cylinder externally and be formed like the inner portion (towards the axis) of the surface of a ring. If we regard these surfaces as *locus in quo* for surface-regions moving in them, as Space is *locus in quo* for bodies, the surface regions can be moved in all these surfaces without stretching. The surfaces with positive curvature can always be so formed that surface regions may also be moved arbitrarily about upon them without *bending*, namely (they may be formed) into sphere-surfaces; but not those with negative curvature. Besides this independence of surface regions from position there is in surfaces of zero curvature also an independence of *direction* from position, which in the former surfaces does not exist.

(*To be continued.*)

SCIENTIFIC SERIALS

Zeitschrift fur Ethnologie, No. 6.—The present number gives a compendium of useful suggestions, which might advantageously be acted on in other countries besides Germany, addressed by the Anthropological Society of Berlin to all persons engaged in exploring, or other expeditions to distant regions. In those directions for observing and collecting whatever is most adapted to extend and rectify our actual knowledge, information is given in regard to the various races with whom travellers may come in contact, and the special geographical, linguistic, social and other conditions, which more particularly require further elucidation.—Prof. A. Bastian gives us in this number with his habitual completeness an exposition of the worship of the heavenly bodies among different nations, and the extent to which local conditions of climate and ethnological differences have influenced the character of the adoration offered to the sun and the moon and the stars. According to him a *true* worship of the sun except in the polar regions—is only to be found on elevated plateaux, where the return of the orb of day was welcomed with gratitude after the colder night, while in low-lying tropical lands the aborigines looked with dread at the glowing ball of fire which each summer seemed to threaten their world with annihilation. We can strongly commend this paper as a most comprehensive, although not specially novel exposition of Aryan and other mythological systems.—The German engineer, Herr H Keplin, has drawn attention to the mussel-hills (*Casqueiros* or *sambaquis*) of Brazil in the district of the Rio do San Francisco do Sol. The position of these deposits appears to refute the idea of their being mere Kjokkenmodings, while the great respect shown by the natives for the dead, and their care to provide them proper sepulture, would seem to afford further evidence that these elevations, which often rise to a height of 50 feet, cannot be due to the hand of man. In reference to the above, it may interest our own archæologists to know that Herr Walter Kauffman draws attention in the same number to his discovery in the neighbourhood of Hull, at a spot known as Castle Hill, near Holderness, of a burial place belonging, as he conjectures, to the transition period between the Stone and Bronze ages. Herr Kauffman found on the western side of the hill, where the ground had been cut for building purposes, a fragment of some loam vessel, a compact mass of oyster shells, some flint flakes, and a human rib. After carefully removing the earth, Herr K. discovered at from 4 to 4½ feet below the surface the vertebræ of another skeleton, and finally collected nearly all the bones of two skeletons, completely enclosed in a mass of oyster shells.—Dr. A. B. Meyer, of Manilla, in the course of a short visit in the Philippines, found skulls which presented that peculiar appearance of sharpening or filing of the teeth, described by the old traveller, Thévenot, and the accuracy of which has often been called in question. The Negrito skulls from the Philippines, examined by Dr. Meyer, also exhibited the artificial flattening of the heads noticed by Thévenot.—Herr Virchow drew attention last summer to the fact that occasional deviations present themselves from the normal cranial configuration of a race, which ought to teach us extreme caution in regarding any single specimen as a typical form. He was led to make this remark by his observation in the Anatomical Museum of Copenhagen of the skull of Kay Lykke, a man of the noblest Danish descent, who had flourished two hundred years ago, and been celebrated in his day for his personal beauty, his effeminacy, and the sensual bias of his disposition. Yet the skull of this once elegant, accomplished, and self-indulgent courtier of the 17th century, belonging to an otherwise brachycephalic race, is more strikingly dolichocephalic and depressed than the Neanderthal head, and might readily be supposed to have belonged to an Australian savage. The cranial capacity which is given by Professor Panum, of Copenhagen, as 1,250 cubic centim., is, moreover, below the amount that is conjecturally assumed for the Neanderthal skull.

The supplement to the vol. of the "Zeits. f. Ethnologie," for 1872, is exclusively occupied with the Linguistic Notes of Dr. G. Schweinfurth, drawn up as the result of his travel in Central Africa, and gives numerous vocabularies and specimens of the languages of the different tribes who occupy the district of the Bahr-el-Ghasal, among whom Dr. Schweinfurth lived more than two years.

Nuovo Giornale Botanico Italiano, vol. iv. Nos. 1—4, Jan.—Dec., 1872. The volume for 1872 of this journal, edited by one of the most accomplished of Italian botanists, Prof. Caruel, contains evidence of considerable scientific activity in the Peninsula. A large space of these four numbers is devoted to cryptogamic botany; we have papers on the mosses of Abyssinia, by De Venturi, and of Ceylon and Borneo, by Hampe; on the fungi of Parma, by Passerini; on Diatoms, by Ardissone, and on a new classification of cryptogams, proposed by Prof. Cohn. Besides several papers on systematic, descriptive, and geographical botany, one of the most interesting on physiological and histological subjects is by Saccardo, on the amyloid corpuscles contained within the fovilla of pollen, illustrated by a plate. Prof. Caruel contributes a very valuable biographical notice of the Italian botanist, Andrea Cesalpino, born at Arezzo in 1519, and a summary of the contents of his great work, "De Plantis," published at Florence in 1583, which his biographer states to contain the essential features of the classification propounded by A. L. Jussieu two centuries later.

Annalen der Chemie und Pharmacie, February, 1873. The number commences with a paper on a new derivative of sulphocarbamic acid, by H. Hlasiwetz and J. Kachler. The new body is obtained by the action of carbonic disulphide on camphor in the presence of ammonia. Measurements of its crystals are given. The numbers obtained by an analysis agree well with the formula C_3, H_{10}, N_4, S_4; this is regarded as an ammonia salt; a copper compound C_3, H_8, N_3, S_3, Cu, has been obtained, but the acid cannot be isolated from it, as SH_2 refuses to precipitate the copper. Several other compounds of the body are described.—The next paper is a short note by M. Berthelot on the formation of Acetylen by the silent electric discharge. Messrs. R. Boettger and Theodor Petersen contribute a paper on the Nitro-compounds of Anthrachinon. The following bodies are described: α Mononitroanthrachinon, α Monamidoanthrachinon, and α Diazoanthrachinon Nitrate; the behaviour of these α bodies with concentrated sulphuric acid is then described.—On the Vanadates of Thallium, by Thomas Carnelly. The author describes the method of preparation and properties of the salts in question; this paper has already appeared in the April number of the Chemical Society's journal, as also has the next, on Ethyl-amyl, by Harry Grimshaw, and Schorlemmer's paper on the Heptanes from Petroleum.—Crystallographic Notices, I. by C. Klein, is a long paper on the measurement, &c. of crystals; a contribution to our knowledge of Neurin, by Julius Mauthner; "Remarks on my Water Air-pump," by N. Jagn; and a paper on Excretin from Human Excrement, by F. Hinterberger. The author has established the formula C_{20}, $H_{36}O$ for this body, and has obtained a Brominated derivative C_{20}, H_{34}, Br_2 O.

Bulletin de la Société de Géographie.—The first article in the March number is by the Abbé Durand, formerly a missionary in Brazil, on the Solimoes, the name given to the Amazon from its junction with the Rio Negro upwards, this being the name of the most powerful tribe on its banks. The Abbé gives an account of his journey up the river as far as Peru. His article contains many valuable facts as to towns, and people, and products of the district through which he passed. The next article is the last of Capt. Derrégagaix's papers on the South of the Province of Oran; the present one treating of the Geology and Meteorology of the district. This is followed by a translation of part of Col. Yule's essay on the geography of the Oxus prefixed to Wood's "Journey to the Source of the Oxus"—M. N. de Khanikoff contributes a paper on our knowledge of the Khanate of Khiva.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 24.—On the Durability and Preservation of Iron Ships, and on Riveted Joints, by Sir William Fairbairn, Bart., F.R.S.

On the employment of Meteorological Statistics in determining the best course for a Ship whose sailing qualities are known, by Francis Galton, F.R.S.

Zoological Society, April 29.—Anniversary Meeting.—Viscount Walden, F.R.S., president, in the chair.—After some preliminary business the report of the Council was read by the Secretary, Mr. P. L. Sclater, F.R.S. It stated that the number of ordinary members of the Society on January 1 last, was 3,050, of Foreign members, 25, and of Corresponding members, 197. The total income of the Society in 1872 was 26,728*l*., being 2,017*l*. more than that of 1871, and exceeding the income of any previous year, except that of the year 1862, when the International Exhibition was held. The total expenditure of 1872 had been 26,900*l*., and a balance of 1,956*l*. had been carried forward for the benefit of the current year. The assets of the Society on December 31, 1872, were calculated at 10,532*l*., while the liabilities were reckoned at 5,490*l*. The Reserve-fund consisted of a sum of 8,000*l*. Reduced 3 per Cents. The Scientific publications of the Society for 1872 had consisted of the usual volume of "Proceedings," four parts of "Transactions," a Revised List of the Vertebrated Animals, now or lately living in the Society's Gardens, and a General Index to the ten years of the Society's "Proceedings," from 1861 to 1870. The most important work undertaken in the Society's Gardens in 1872 had been the bridge over the Regent's Park Canal, intended to connect the Society's new grounds on the north bank, with the present Gardens. This had been completed in October last at a total cost of 1,333*l*. The new Lodge and Entrance-gates in Primrose-hill Road had likewise been finished, and the new entrance opened to the public for the first time on Easter Monday. The total number of visitors to the Society's Gardens in 1872 had been 648,088, being 52,171 more than the corresponding number in 1871. The greatest number of admissions in any one day in 1872 had been 44,608, which took place on May 20 (Whit Monday). The number of animals in the Menagerie on Dec. 31, 1872, was 2,010. Many of the accessions during the year had consisted of specimens of rare or little known animals, of which full particulars are given. The Report concluded with a long list of donors, and their several donations to the Menagerie. The Meeting then proceeded to elect the new Members of Council and the Officers for the ensuing year, and a ballot having been taken it was found that Viscount Walden, F.R.S., had been elected President, Mr. Robert Drummond, Treasurer, and Mr. P. L. Sclater, F.R.S., Secretary of the Society. The new Members of Council elected were Francis Galton, F.R.S., John P. Gassiott, Jun., St. George Mivart, F.R.S., George Russell, and Richard H. S. Vyvyan.

Geological Society, April 9.—His Grace the Duke of Argyll, K.T., F.R.S., president, in the chair. The following communications were read:—"Lakes of the north-eastern Alps, and their bearing on the Glacier-erosion Theory," by the Rev. T. G. Bonney, F.G.S. The purpose of this paper was to test, by the lakes of the Salzkammergut and neighbourhood, the theory of the erosion of lake-basins by glaciers, which has been advanced by Prof. Ramsay. The author premised (1) that an extensive glacier could not exist without a considerable area to support it; (2) that under no circumstances could a glacier excavate a cliff of considerable height (say 1,000 ft.) approximately vertical; (3) that owing to the proximity of the regions, a theory of excavation which applied to the Western and Central Alps ought to be applicable also to the Eastern Alps. He then proceeded to examine a number of lakes in detail. The Konigsee lies in a remarkably deep, steep-sided valley, terminated by a cirque, with cliffs full a thousand feet high, and has no large supply area behind. The Hallstadtersee is similarly situated, has a cirque at the head, and two lateral valleys nearly at right angles to the lake, up which arms of it have formerly extended. These are not likely to have furnished glaciers which could have excavated the lake; and above the cirque there is no large supply area. The Gasauthal consists of lake-basins separated by valleys of river-erosion. The Fuschelsee and Wolfgangersee, on the south side of the Schafberg, are separated by a narrow sharp ridge of hills, incapable of nourishing glaciers large enough to grind them out; there are no signs of glaciers from other directions having eroded them. The Mondsee and Attersee (once one lake) on the north lie under the steep cliffs of the Schafberg, which could not have nourished a large glacier; and the ridge of the Schafberg is too sharp to admit of the supposition that a great glacier, coming from the south, has passed over it to excavate the lake; yet the Attersee, in a position least favourable to glacial action, is the largest and deepest lake in the Salzkammergut. The head of the valley in which these lakes lie is really among low hills, in the direction of the Austro-Bavarian plain. The Traunsee was shown to give no evidence in favour of a theory of glacial erosion. Since then these lakes either had at their heads preglacial cirques (the very existence of which was incompatible with much erosive power on the part of a glacier), or were beneath sharp and not greatly elevated ridges of rock, the author concluded that they had not been excavated primarily by glaciers. He considered a far more probable explanation to be, that the greater lake-basins were parts of ordinary valleys, excavated by rain and rivers, the beds of which had undergone disturbances after the valley had assumed approximately its present contour. He showed that the lakes were in most cases maintained at their present level by drift; and that, while in a region so subject to slight disturbances as the Alps, positive evidence for his theory would be almost impossible to obtain, no lake offered any against it, and one, the Konigsee, was very favourable to it.—"On the Effects of Glacier-erosion in Alpine Valleys," by Signor B. Gastaldi. The author described the occurrence in the valley of the Lanzo and other Alpine valleys, at heights between 2,000 and 3,000 metres (6,700 and 10,000 feet), of large cirques, in two of which, in the valley Sauze de Césanne, the bottom was occupied in the autumn by glaciers reduced to their smallest dimensions. The author noticed the various rocks in which these cirques were cut, and expressed his opinion that they are the beds formerly occupied by glaciers, the power of which to excavate even comparatively hard rocks, such as felspathic, amphibolite, and chlorite-schists, he considered to be proved. The author then referred to the mouths of the Alpine valleys opening upon the plain, which he described as being generally very narrow in proportion to their length, width, and orographical importance; and he pointed out that in the case of the valley of the Stura, at any rate, the outlet of the valley has been cut out by the river. This peculiarity he accounts for by the fact that whilst the calcareous and felspathic rocks are easily disintegrated by atmospheric action, certain other rocks, such as the amphibolites, diorites, syenites, amphibolite-schists, euphotides, serpentines, &c., resist atmospheric denudation; and he indicated the peculiar distribution of these rocks in the region under consideration, by reason of which portions of them occupied the points which are now the mouths of the valleys.

Anthropological Institute, April 22.—Prof. Busk, F.R.S., president, in the chair.—The following papers were read:—The Religious Beliefs of the Ojibois or Santeux Indians resident in Manitoba and at Lake Winnepeg, by A. P. Reid, M.D.—The predominating Danish aspect of the local nomenclature of Cleveland, by Rev. J. C. Atkinson.—Rock Inscriptions in Brazil, by John Whitfield.—Remarks about the consecration of the Serpent as an Emblem but not an Object of Worship among the Intelligent Druids, by James Hutchings.

Entomological Society, April 7.—Prof. Westwood, president, in the chair.—Mr. Champion exhibited specimens of *Tribolium confusum* and *Ptinus testaceus*, which he had observed in British collections mistaken for *T. testaceum* and *P fur.*—Mr. Verrall exhibited several new species of *Diptera* belonging to the families *Asilidæ* and *Syrphidæ*, taken in Britain —Mr. McLachlan stated that he had been informed by Lord Walsingham that he had observed Dragon flies in California and Texas preyed upon by other large insects which seized them whilst flying through the air. The latter were, no doubt, some species of *Asilus*; but it was the first time he had heard of Dragon flies being preyed upon by other insects, as they had, hitherto, been supposed to be free from such attacks.—Mr. F. Smith made some remarks on a species of *Pentatoma* sent from Calcutta by Mr. Rothney, which was of the same colour as the bark of the tree on which it was observed in great numbers —Major Parry communicated a paper on the characters of seven nondescript Lucanoid Coleoptera, with remarks on the genera *Lissotes*, *Nigidius*, and *Figulus*.—Mr. Frederick Bates communicated "Descriptions of new Genera and species of *Tenebrionidæ* from Australia, New Caledonia, and Norfolk Island." —Mr Muller read some interesting remarks on the habits of the *Cynipidæ*, communicated to him in a letter from Mr. W. F. Bassett, of Waterbury, U.S.—Part I. of the Transactions for 1873 was on the table.

Meteorological Society, April 16.—Dr. Tripe, president, in the chair —A discussion took place on the following questions which had been submitted to the consideration of the Meteorological Conference held at Leipzig in August last:—No 2. Barometers for Stations of the second order. No. 4. Maximum and Minimum Thermometers. No. 5. Instruments for determining Solar Radiation. No 18. Uniformity in Hours of Observation. No. 20. Division of the Year for the Calculation of Mean Results. On question No. 2, several spoke in favour of aneroids, and several that they were not to be trusted; the opinion of the meeting was that for hard rough work where the aneroid is exposed to low and high pressure it is not suited for taking correct observations, and that the Kew barometer is much to be preferred. On question No. 4 the testimony of the meeting was in favour of Phillips' and Negretti's maximum thermometer. On question 5, reference was made to a paper by Rev. F. W. Stow, M.A., on "Solar Radiation," which is printed in the Journal of the Society for April 1873. Time would not allow of questions 18 and 20 being fully discussed, so they will be brought up again at the meeting on May 21.

MANCHESTER

Literary and Philosophical Society, April 15.—R. Angus Smith, F.R.S., vice-president, in the chair.—Mr. Francis Nicholson exhibited two fine eggs of the golden eagle (*Falco chrysaëtos*) taken the previous week from a nest in the north of Scotland. Fortunately some of the large landed proprietors both in Scotland and Ireland are now preserving this noble bird from persecution during the breeding time.—A letter was read from Mr. William Boyd Dawkins, F.R.S., who, as Secretary of the Committee of the British Association for carrying on the exploration of the Victoria Cave, felt obliged to notice the "Notes on Victoria Cave," by Mr. W. Brockbank, published in the Proceedings, March 10, 1873. Mr. Dawkins submitted that until the work of the Committee, to which the cave has been handed over by the kindness of the owner, be finished, and the observations to which Mr. Brockbank has had no access, be recorded, his notes must of necessity be imperfect and liable to error. Mr. Dawkins then calls attention to two matters of fact, in which he shows Mr. Brockbank's statement to be entirely unfounded.—"On some Improvements in Electro-Magnetic Induction Machines," by Mr. Henry Wilde.

PHILADELPHIA

Academy of Natural Sciences, October 15.—Prof. Leidy directed attention to the collection of fossils, from the vicinity of Fort Bridger, Wyoming, presented by Dr. J. Van A. Carter, Dr. Joseph K Corson, U.S.A., and himself. Some of the fossils were referred to a huge pachyderm with the name of *Uintatherium robustum*. [This subject has already been several times referred to in NATURE. See Mr. A. H. Garrod's letter last week]. Prof Leidy further called attention to a multitude of chipped stones, which he had collected about ten miles north-east of Fort Bridger. Many of the fragments are broken in such a manner that it is difficult to be convinced that they are not of artificial origin. The materials of the splintered stones consist of jaspers, quartzites, some of the softer rocks of the tertiary strata, and less frequently of black flint identical in appearance with that of the English chalk.

December 3, 1872.—The president, Dr. Ruschenberger, in the chair.—Joseph Wilcox made remarks about some glacial scorings lately observed by him in St. Lawrence County, N. Y.

December 10, 1872.—The president, Dr. Ruschenberger, in the chair.—Jos. Wilcox made the following remarks:—Having lately visited many mineral localities in Canada, I desire to place them on record, as many of them are not mentioned either in the "Geological Report of Canada," or in Dana's "Mineralogy." At the falls of Ottawa River at Grand Calumet Island, black mica (phlogopite), pyroxene, hornblende, serpentine, tremolite. The following localities are all in the Province of Ontario. At Arnprior, Calcite (dog tooth spar); near Packenham, Hornblende; in Bathurst, pyroxene, scapolite, sphene, apatite peristerite; two miles south-west of Perth, bronze mica (phlogopite), having beautiful hexagonal marks on the cleavage planes; near Otty Lake, in North Elmsley, Apatite, pyroxene, black mica (biotite), zircon, red spinel—chondrolite; in Burgess, apatite, black mica (biotite); near Bob Lake, twenty miles north-west of Perth, the best crystals of apatite are found; near the St. Lawrence River, six miles south-west of Brockville, large octahedral crystals of iron pyrites, some of them four inches in diameter. All of these minerals are well crystallized, except the peristerite and chondrolite.—Prof. Leidy directed attention to some fossils recently received from Dr. J. Van A. Carter, of Fort Bridger, Wyoming. They were—*Palæosyops junior*, *Uintacyon edax*, *Uintacyon vorax*, and *Chamæleo pristinus*.—Remarks on silver ore from Colorado, by George A. Konig.

December 17, 1872.—Dr. J. L. LeConte in the chair.—Prof. Cope made some remarks on the Geology of Wyoming, especially with reference to the age of the coal series of Bitter Creek. He said that discovery of the Dinosaur *Agathaumas sylvestris* had settled the question of age, concerning which there had been much difference of opinion, in favour of the view that they constitute an upper member of the Cretaceous series. It appeared to the speaker, that the explorations directed by Dr. Hayden during the past season had contributed largely to our knowledge, proving the existence of an interruption between the cretaceous and tertiary formations: less it is true than that which exists elsewhere, and similar to that insisted on by Clarence King's survey in the region of Bear River and the Wahsatch country.—Prof. Cope defined a genus of Saurodont Fishes from the Niobrara Cretaceous of Kansas, under the name of *Erisichthe*. He stated that it agreed with *Portheus* and *Ichthyodectes* in the absence of nutritious dental foramina on the inner face of the dentary bone, and especially with *Portheus* in the irregular sizes of the teeth.

January 7.—Dr. Ruschenberger, president, in the chair.—E. Goldsmith described what he considers a new mineral which he names *Trautwineite*, after its first observer, Mr. J. C. Trautwine. The mineral has a green colour; the hardness is between 1 and 2, and it is micro-crystalline. The regular forms, which he saw, were short hexagonal pyramids, the infinite pyramid (prism), and triangular slender prisms, which may be one-sixth sections of the hexagonal prism. Under ordinary circumstances the mineral is dull, but when observed under power it appears vitreous. The streak is light green. The qualitative chemical examination indicated the oxides of chromium, iron, and magnesium.—Prof. Cope remarked, that, through the kindness of Prof. B. F. Mudge, he had an opportunity of examining additional specimens of the turtle from the cretaceous of Kansas, described by him in the Proceedings of the Academy, 1872, p. 129. The phalanges indicated a large flipper of the type of marine turtles. They are more flattened than in the *Propleuridæ* so far as the latter are known, and are proportionally larger. The genus and species were named *Toxochelys latiremis*.

Paris

Academy of Sciences, April 21.—M. de Quatrefages president, in the chair.—The following papers were read.—A final answer to M. Secchi, by M. Faye. M. Faye called attention to the fact that Father Secchi has accused him of insinuating that his drawings of the spots are not authentic, which insinuation also applies to the drawings of Carrington and Father Tacchini. This he showed was not the case, his statement that photographs, and not drawings, were required, being perfectly obvious as regards its signification. He then proceeded to answer Secchi's statements as to eruptions projecting the erupted matter towards a common centre, and asked how it was that these masses cooled during a passage which lasted often but a day or two, or even a few hours, could produce spots which lasted for months. He then answered several other objections, and called attention to Respighi's observations of the chromosphere, the earliest, as they are the best yet executed, as fully bearing out his theory.—On the condensation of Carbonic Oxide and Hydrogen, and of Nitrogen and Hydrogen, by the silent electric discharge, by MM. P. and A. Thenard. The authors had noticed that the protocarbide of hydrogen and carbonic anhydride, which, under the silent discharge condensed to a liquid, were doubled in volume and converted into carbonic oxide and hydrogen by the spark, they therefore sought to recombine the two latter gases by the discharge; in this they succeeded, and the action was more rapid than with the first. They also succeeded in producing ammonia from three volumes of hydrogen and one of nitrogen when treated in the same way; the action was most rapid when an acid was present to absorb the NH_3 as fast as it was formed.—On the physical and political history of Chili, by M. Gay, was a sketch of a work by the author in Spanish consisting of thirty volumes.—On the qualities necessary to the springs required for the supply of water to Paris by M. Belgrand.—M. Leymerie was then elected correspondent of the Mineralogical section in place of the late M. Haidinger, and M. Didion correspondent of the Mechanical section in place of the late Canon Moseley.—On a spectral illuminator, by M. F. P. Le Roux, described a new method of obtaining monochromatic illumination.—On the action of electricity on flames by M. Neyreneuf.—On the application of the curves *des débits* to the study of the laws of rivers and to the effects produced by a multiple system of reservoirs by M. de Graeff.—Observations on *Phylloxera vastatrix*, by M. Maxime Cornu.—A decree from the President of the Republic was received authorising the Academy to receive a legacy of 40,000 francs, left to it by the late Marshal Vaillant.—On the interference fringes observed in the case of Sirius and several other stars when large telescopes are employed; a consequence of the relative angular diameter of the stars in question, by M. Stephan. The author hopes, by means of certain observations, to obtain an approximate measurement of the diameter of Sirius.—On the comparison of electrical machines, by M. Mascart.—Remarks on the resistance of galvanometers, by M. J. Reynaud.—On the condensed discharge of the induction spark, by M. Th. du Moncel.—Researches on the chloride bromide and iodide of trichloracetyl, by M. H. Gal.—On the action of sodic sulphide on glycerin, by M. F. Schlagdenhauffen.—On a volumetric method of estimating oxygen in hydric peroxide and other liquids, by M. F. Hamel; this is an application of the disengagement of oxygen from the above body, by means of potassic permanganate. The gas liberated and the permanganate used form the data necessary for the preparation of standard permanganate solution, where the oxygen liberated per c.c. of reagent used is known. On the properties and composition of a cellular tissue which extends throughout the organism of the vertebrata, by M. A. Muntz.—Discovery of a new human skeleton of the paleolithic period in the caverns of Baoussé Roussé, by M. E. Rivière.—On the influence of various coloured rays on the spectrum of chlorophyll, by M. J. Chautard.—A note on the habits of "Lombrics," by M. E. Robert.

DIARY

THURSDAY, May 1.

Royal Society, at 8.30.—On the Effect of Pressure on the Character of the Spectra of Gases: C. H. Stearn and G. H. Lee.—On the Condensation of a Mixture of Air and Steam upon Cold Surfaces: Prof. Osborne Reynolds.—Further Observations on the Temperature at which Bacteria Vibriones and their supposed Germs are killed when exposed to Heat, &c.—Dr. Bastian.

Society of Antiquaries, at 8.30.—Flint Implements from Japan: W. L. Lawrence.—On Religious Guilds, and particularly the Privileged Guild at Walsoken, Norfolk: J. G. Nichols.

Linnean Society, at 8.—On Cinchonas: J. E. Howard.

Chemical Society, at 8.—On Zirconia: J. B. Hannay.—On a new class of Explosives: Dr. Sprengel.

Royal Institution, at 2.—Annual Meeting

FRIDAY, May 2.

Geologists' Association, at 8.—On the Valley of the Vézère (Dordogne), its Limestones, Caves, and Pre-historic Remains: T. Rupert Jones.

Royal Institution, at 9.—Alcohols from Flints: Prof. Reynolds.

Archæological Institution, at 4.

Horticultural Society, at 3.—Lecture.

SATURDAY, May 3.

Royal Institution, at 3.—Ozone: Prof. Odling.

SUNDAY, May 4.

Sunday Lecture Society, at 4.—The Relations between Science and some Modern Poetry: Prof. Clifford.

MONDAY, May 5.

Royal Institution, at 2.—General Monthly Meeting.

Geologists' Association.—Excursion to Aylesbury, from Euston Square at 10.15 A.M.

Entomological Society, at 7.

Asiatic Society, at 3.

London Institution, at 4.—Elementary Botany: Prof. Bentley.

TUESDAY, May 6.

Anthropological Institute, at 8.—Eastern Coolie Labour: W. T. Distant.—The Westerly Drifting of Nomades from the Fifth to the Nineteenth Century. Part X. The Alans or Lesghs: H. H. Howorth.

Society of Biblical Archæology, at 8.30.—On the Signification and Etymology of the Hebrew Noun תִּרְשָׁתָא Tirshatha: R. Cull.—On the Chronology of the Olympiads in Connection with the Golden Age of Greece: W. R. A. Boyle.—On the Sites of Ophir and Taprobane, from Greek and Hindu Authorities: A. M. Cameron.—On the Character of the Preposition in the Egyptian Language: P. Le Page Renouf.—Translation of an Egyptian Hymn to Ammon: C. W. Goodwin.

Zoological Society, at 8.30.—On some new Species of *Araneidea*: O. P. Cambridge.—On African Buffaloes: Sir Victor Brooke.

Royal Institution, at 3.—Music of the Drama: Mr. Dannreuther.

WEDNESDAY, May 7.

Society of Arts, at 8.—Improvements in the Manufacture of Gun Cotton: S. J. Mackie.

Horticultural Society.—Exhibition of Roses, Azaleas, &c.

Microscopical Society, at 8.—On the Development of the Sturgeon's Facial Arches: W. K. Parker.

London Institution, at 7.—Conversazione and Lecture by Prof. Clifford.

THURSDAY, May 8.

Royal Institution, at 3.—Light: Prof. Tyndall.

Mathematical Society, at 8.—On an application of the Theory of Unicursal Curves, Plan of a Curve-tracing Apparatus: M. Hermite.—On Bicursal Curves: Prof. Cayley.

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THURSDAY, MAY 8, 1873

A VOICE FROM CAMBRIDGE

IT is known to all the world that science is all but dead in England. By science, of course, we mean that searching after new knowledge which is its own reward, a thing about as different as a thing can be from that other kind of science, which is now not only fashionable, but splendidly lucrative—that "science" which Mr. Gladstone and Mr. Lowe always appeal to with so much pride at the annual dinner of the Civil Engineers—and that other "science" prepared for Jury consumption and the like.

It is also known that science is perhaps deadest of all at our Universities. Let any one compare Cambridge, for instance, with any German university; nay, with even some provincial offshoots of the University in France. In the one case he will find a wealth of things that are not scientific, and not a laboratory to work in; in the other he will find science taking its proper place in the university teaching, and, in three cases out of four, men working in various properly appointed laboratories, which men are known by their works all over the world.

This, then, is the present position of Cambridge after a long self-administration of the enormous funds which have been so long accumulating there for the advancement of learning. Cambridge no longer holds the place which is hers by right in the van of English science, her workers are few, and to those few she is careful to afford no opportunity of work, such as it is the pride of scholastic bodies in other countries to provide for the men who bring the only lasting honour to a university.

We have in what has gone before instanced Cambridge specially, as we have to refer to a step which has been recently taken there; but if the state of things is to be condemned at Cambridge, it must be admitted that it is only too recently that an attempt has been made to correct, in one direction, a similar state of things at Oxford.

What then do the Universities do? They perform the functions, for too many of their students, of first-grade schools merely, and that in a manner about which opinions are divided; and superadded to these is an enormous examining engine, on the most approved Chinese model, always at work, and then there are fellowships.

Now the readers of NATURE do not need to be informed that at the present moment there are two Royal Commissions inquiring into matters connected with the Universities, and that not long ago, at a meeting at the Freemasons' Tavern, the actual absence of mature study and research at the Universities, the lack of opportunities and buildings for scientific puposes, the apotheosis of the examining system, and the wanton waste of funds in fellowships, were unhesitatingly condemned by some of the most distinguished men in the country, many of them residents in the Universities.

Within the last week a memorial has been presented to the Prime Minister by persons engaged in University education at Cambridge, which on one of the points above referred to contains a most important expression of opinion; but we had better give the memorial *in extenso*:—

[Memorial.]

"We, the undersigned, being resident Fellows of Colleges and other resident members of the University of Cambridge engaged in educational work or holding offices in the University or the Colleges, thinking it of the greatest importance that the Universities should retain the position which they occupy as the centres of the highest education, are of opinion that the following reforms would increase the educational efficiency of the University, and at the same time promote the advancement of science and learning.

"1. No Fellowship should be tenable for life, except only when the original tenure is extended in consideration of services rendered to education, learning, or science, actively and directly, in connection with the University or the Colleges.

"2. A permanent professional career should be as far as possible secured to resident educators and students, whether married or not.

"3. Provision should be made for the association of the Colleges, or of some of them, for educational purposes, so as to secure more efficient teaching, and to allow to the teachers more leisure for private study.

"5. The pecuniary and other relations existing between the University and Colleges should be revised, and, if necessary, a representative Board of University Finance should be organised.

"We are of opinion that a scheme may be framed which shall deal with these questions in such a manner as to promote simultaneously the interests of education and of learning, and that any scheme by which those interests should be dissociated would be injurious to both."

This memorial reflects great credit upon the two out of seventeen heads of Colleges, and the majority of Professors, Tutors, Assistant-Tutors, and Scholars who have signed it. The only wonder is that some action to remedy a state of things which has been considered a scandal by many, both in and out of the University, who have had the best opportunity of studying it, should not have been taken before. But we think the memorial fails in one point, and we believe that Mr. Gladstone has hit the blot, for his carefully worded reply reads to us most ominous. "The time has scarcely arrived for bringing into a working shape proposals for extending and invigorating the action of the Universities and Colleges in connection with the more effective application of their great endowments." We see in the memorial too much reference to teaching, and too little to the advancement of learning.

Surely if the funds accumulated at our great Universities are to be merely applied to teaching purposes, the Government has the best possible argument for instantly requiring a very large proportion of the "great endowments" to be handed over, in order to endow other teaching bodies at present crippled for want of funds, and to create other teaching centres where now no teaching exists.

Might not the memorialists have taken a higher line, in which they would have been supported by all the culture of the country? Might they not have pointed out that the universities were once the seats of learning, and that the fact that they are now merely seats of teaching has arisen from a misapplication of the "great endowments" to

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which Mr. Gladstone refers? Why should not the men of Cambridge say boldly that they wish their University to become again in the present what it was in the past? No government would dare to cripple such a noble work. As representing the then range of knowledge, and as seats of research centuries ago, our universities were unequalled; at present in both these respects they are ridiculous.

COUES' AMERICAN BIRDS

Key to North American Birds. By Elliott Coues, M.D. (Salem, U.S.)

THIS by no means small volume is intended to give a concise account of every species of living and fossil bird at present known from the continent north of the Mexican and United States boundary. The reputation of the author, who is so well known by his works on the sea-birds, and for the anatomy of the loon, cannot but be increased by this production, which illustrates on every page the extent of his general information, and the soundness of his judgment. The subject is treated in a manner rather different from that usually adopted by systematic ornithologists; less stress is laid on specific peculiarities, and more on the elucidation of the characteristics of the genera, families, and orders. There is a freshness and boldness in the manner in which the facts are handled, which will be extremely acceptable to those who look upon ornithology as a branch of natural history rather than an all-absorbing study of itself. We know of no work of the size which gives such a fair and reliable description of the reasons that have led to the limitation of the ranges of the larger divisions which now obtain, and their inefficiency is in many cases rendered but too evident. The introduction, occupying nearly seventy pages, incorporates much of the work of the illustrious Nitzsch, which is daily becoming more fully appreciated, though neglected so long. We are surprised to find that the labours of Mr. Macgillivray have not been here done equal justice to, for there cannot be a doubt that the peculiarities of the viscera are of as great importance in the classification of birds, and yet they are scarcely mentioned; in one instance we find it incorrectly stated that the cæca of the *Cathartidæ* are very small, the term must be here understood in its extreme sense, as they are absent altogether.

The descriptions of the genera are clear and concise; many of the peculiarities of the beak and primaries especially, are made more evident by the liberal introduction of excellent line drawings, as in the account of the genus *Vireo,* which is discussed much in detail; and in most cases a picture of the whole bird, or the head, is given. A key is appended for discovering the genera with facility, constructed on the same principle as those employed by botanists. The paucity of the avian fauna in the region discussed, in comparison to that of the Southern Continent, is made most manifest, and the few stragglers which have thence made their way north, serve well as illustrations of the classes which, were it not for them, would not find a place in a work on North American Birds.

FLAMMARION'S ATMOSPHERE

The Atmosphere. Translated from the French of Camille Flammarion, edited by James Glaisher, F.R.S., &c. (London: Sampson Low and Co., 1873.)

IN some respects the volume before us may be considered as the sequel to its equally sumptuous companion "The Forces of Nature." For the ordinary reader must have some acquaintance with physics intelligently to follow the disentanglement of the various forms of energy—the mingled play of which give rise to the phenomena of meteorology. Nevertheless, M. Flammarion writes so lucidly and pleasantly, that a totally unscientific person can read this work with enjoyment and instruction. On the other hand it contains much that will be of interest to the man of science, as well as to the mere *dilettante.*

The scope of the work is stated in the editor's preface. It treats of the form, dimensions, and movements of the earth, and of the influence exerted on meteorology by the physical conformation of our globe; of the figure, height, colour, weight, and chemical components of the atmosphere; of the meteorological phenomena induced by the action of light, and the optical appearances which objects present as seen through different atmospheric strata; of the phenomena connected with heat, wind, clouds, rain, electricity; and also of the laws of climate. These subjects are illustrated by ten admirable chromo-lithographs, and upwards of eighty woodcuts, but many of these latter we observe have already done duty in other French treatises. The coloured illustrations are quite works of art; especially noteworthy are the representations of a sunset, of sunrise as seen from the Righi, and of a solar and a lunar rainbow. Science has more often given than received aid from art, but the pages of this book show how much service art can render to science. The printing is remarkably well executed.

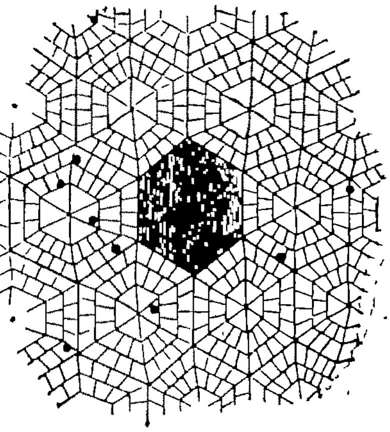

FIG. 1.—Section of a hailstone enlarged.

The translation has been done by Mr. E. B. Pitman, and the task has been well discharged. The value of the original work is considerably increased by the careful revision it has received from Mr. Glaisher, and the additions by him of many useful foot-notes. The tendency of M. Flammarion, like other popular French writers, to run into grandiloquent language, has been in general suppressed; though still a few cases remain that might well have been pruned.

One of the important features in this book is the frequent graphic delineation of meteorological data. Take for example the representation of the decreasing rainfall in passing from tropical to polar regions.

In a similar manner is shown the increase of rain, according to altitude, but in this there is evidently a mistake in one of the figures. Following this woodcut is the representation of the comparative depths of rainfall at noticeable spots. Towering over the whole is the rainfall at the mountain station of Cherra-Poejen in India, where upwards of 50 *feet* of rain annually descend during the seven months of the rainy season.

The engravings of different forms of hailstones are interesting. Here are some that fell on different occasions At the four corners are represented hailstones that fel at Auxerre, on July 29, 1871. The small drawings are o the more usual form of hailstones. The two stones in the centre are taken from drawings exhibited to the Academy of Sciences at St. Petersburgh, in September 1863. These stones were ellipsoidal in shape; their surface when examined through a lens "had the aspect of six-fronted pyramids, and a section of the interior revealed the existence of a hexagonal network of meshes," which is here represented on an enlarged scale. The fact of the crystalline structure of ice palpably occurring in hailstones, is a most interesting observation. Mere pressure of adjacent hailstones, like the pressure of soap-bubbles in a dish, would hardly produce such definite and regular hexagons.

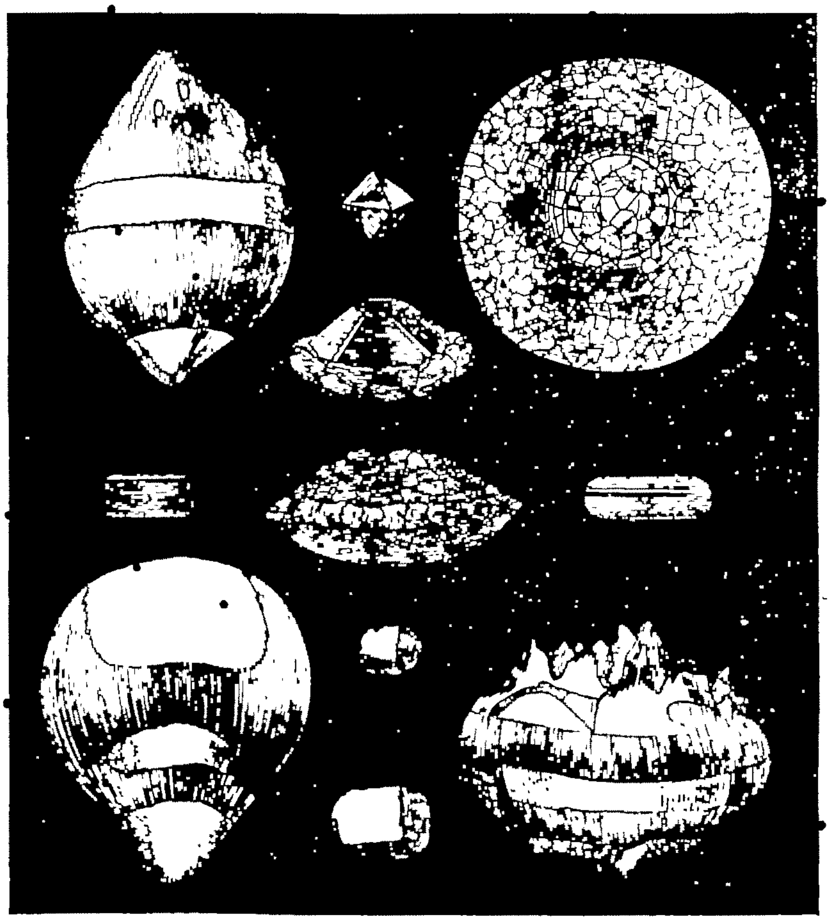

Fig. 1.—Different forms of hail.

As indicative of the labour Mr. Glaisher has bestowed on this work, we notice that all measurements are given in English equivalents, centigrade degrees are converted to Fahrenheit, Paris observations are replaced by data from Greenwich, and appropriate condensation and excision has reduced by one-half the unwieldy size of the original work.

Notwithstanding this evident care, several blemishes have escaped editorial revision. For example, the *ascent* of sound is given as the explanation of the ease with which sounds are heard in a balloon.

On p. 195 it is stated that "The sun's rays, after having traversed either the air, a pane of glass, or *any* transparent body, lose the faculty of retreating through the same transparent body to return towards celestial space." No reference is here made to diathermic bodies, such as rock-salt, concerning which this statement is wholly incorrect; and even as regards the most athermic substances, such as alum or water, a considerable percentage of the sun's rays (its luminous portion, for example) would be re-transmitted. To explain electrical phenomena, M. Flammarion remarks, "It is admitted, first, that electricity is a subtle fluid capable of being amassed, condensed, and rarefied, &c.," and on p. 493, "The Saint Elmo fires are a slow manifestation of electricity, a quiet outflow, like that of the hydrogen in a gas-burner." At the present day we hardly expected to find so material a conception of electricity put forth, unguarded by a restriction of the fluid theory being merely a convenient hypothesis whereby electrical effects can be represented to the mind. And what evidence has M. Flammarion for his unqualified assertion on p. 427, that "the globe is one vast reservoir for this subtle fluid [electricity], *which exists in all the worlds appertaining to our system, and of which the radiating focus is in the sun itself. . . . Its palpitations sustain the life of the universe!*"

We have noticed a few other passages that have escaped the editor's attention in the present edition. The author speaks of a mist in the Grotto del Cane as "composed of carbonic acid gas, which is *coloured by a small quantity of aqueous vapour*." This is difficult to understand, the vapour being as invisible as the gas itself. We did not know it was necessary to use a "preparation of 'Joseph's paper,'" steeped in a solution of starch and potassic iodide, in order to detect ozone. In describing the discovery of oxygen and the chemical composition of the air, Lavoisier is the only name mentioned. It is not unlikely that a French writer should forget Priestley and Scheele, but the English editor ought hardly to have overlooked their names. We think also that a table of the analysis of air obtained from different parts of the globe should have been supplied. All that is given is one comparatively rough determination, namely, that 100 parts of air contain 23 of oxygen and 77 of nitrogen by weight. This is termed "an analysis made with every conceivable precaution." A large part of this same chapter is devoted to impurities present in the atmosphere, but Dr. Angus Smith's classical researches are not referred to, nor even is his name mentioned. And this reminds us that the volume is incomplete without an index, which it ought to possess.

We should like also to have seen some attempt at a collation of meteorological phenomena. Meteorologists in general seem to have their eyes so close to their special observations, that they accumulate a vast mass of figures without "hunting for a cycle," which has been asserted to be their first duty. There certainly appears to be some traces of an eleven-yearly cycle in the recurring period of extremely hot summers and cold winters from 1793 to the present time, cited by M. Flammarion. By collecting and tabulating these figures (given in chapters 4 and 5 of the third book), it becomes evident that extreme winters have immediately preceded or followed very hot summers. As the dates stand, they go alternately before and after, but this, no doubt, is but an accidental coincidence.

In spite of the slight defects we have pointed out, almost inseparable from a work dealing with such a variety of subjects, we can nevertheless endorse the opinion of the editor that the volume "will be found to be readable, popular, and accurate, and it covers ground not occupied by any one work in our language."

W. F. BARRETT

OUR BOOK SHELF

Mensuration of Lines, Surfaces, and Volumes. By D. Munn, F.R.S.E. (132 pp. "Chambers's Educational Course.")

THIS little work presupposes that the student has some knowledge of algebra and geometry, and we agree with the author that "it is not until a pupil has acquired this knowledge that he can take up the subject with any degree of intelligence or derive any educational advantage from its study." The number of propositions (59) is not too great; great judgment is displayed in the selection of the properties elucidated; the proofs are concise and clear, and are followed up by more than 350 examples, which appear to be clearly drawn up and to be well suited to test the student's acquaintance with the text. The book-work is accurately printed, the most important mistakes being p. 41, line 23, p. 91, lines 23, 24, and p. 110, line 22, but these are easily corrected. The work is one of a series, and the references throughout are to the edition of Euclid brought out by the same publishers; this reference to Euclid may appear objectionable in the eyes of some readers, but it is an objection easily got over in the case of those students for whom the work is intended.

Geological Stories. A series of autobiographies in chronological order. By J. E. Taylor, F.G.S. (London: Hardwicke, 1873.)

THE mere form into which Mr. Taylor has thrown his work—that of making a characteristic specimen from each geological formation tell its own story—has not, we think, added anything to its attractiveness: on the contrary, it will be apt to give many readers an uncomfortable feeling of unreality, and seems to us to have often cramped the author's freedom of description. We do not object to the autobiographical form in the abstract, but we think the direct form would have been more suited to Mr. Taylor's mental make. Notwithstanding this little drawback, Mr. Taylor tells the "old, old story," on the whole, in a manner well calculated to interest general readers, and send them to works where they may get the outline here given filled up. Anyone who reads this book carefully, will have a very fair notion indeed of what the best geologists think has been the earth's geological history. Mr. Taylor has of course wisely avoided entering upon disputed points, though one cannot but see that he has a comprehensive and very thorough knowledge of his subject. The illustrations are plentiful, though many of them seem well worn. On the whole the work is one we would recommend to be put into the hands of anyone who needs to be enticed into a knowledge of geology. "Stories" of this class are becoming more and more common every year. Not that we think or desire that they should ever supersede "stories" of another kind; but we take it as one of the most significant signs of the permeation of culture through society, that books of this class find a remunerative public.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Originators of Glacial Theories

THE writer of a notice of Tyndall's "Forms of Water" (NATURE, vol. vii. p. 400) blames Tyndall for having revived in a popular work the Forbes-Rendu controversy, and for calling attention to the claims of Agassiz and Guyot.

It seems rather curious that the attempt to give credit to scientific investigators for the share they may have had in the development of a great theory should be the occasion of fault-finding. No property is as subtle as scientific property, and the care Tyndall has bestowed upon the historical facts bearing on the glacial theory in his various writings on glaciers, is in marked contrast to the ignorance of the true state of the case usually displayed by English authors, who ascribe to Forbes the sole credit of all recent progress in the glacial theory.

Forbes's work commenced in 1841; it was in that year that he made his memorable visit to the Glacier of the Aar, and there found Agassiz, who had at that time already spent five summers in the study of glaciers, and published in 1840 the preliminary part of the investigations carried on by himself and his companions ("Études sur les Glaciers").

Agassiz with his usual freedom in dealing with his associates, which has so often made him appear as following the lead of his pupils, freely imparted to Forbes all he had seen, and certainly had no idea that the hospitality so freely proffered would be returned by the proceedings of Forbes, who appropriated what he could, and misrepresented the nature of his intercourse with Agassiz while his guest on the Glacier of the Aar.

To Tyndall we owe a thorough sifting of the claims of each investigator on the subject, and however unpalatable it may be to national prejudices that the name of Forbes should play a secondary part in these investigations by the side of those of Venetz and Charpentier, Rendu and Agassiz, the fact remains the same, and every fair-minded investigator will thank Tyndall for what he has done.

ALEX. AGASSIZ

Cambridge, Mass., April 15

Scientific Endowments and Bequests

IN the article on scientific endowments and bequests in NATURE for April 24, there is a statement, in reference to the Trinity Natural Science Fellowship, which perhaps requires a little correction.

Although there can be no doubt that the proposed new scheme for the selection of a fellow is in every way better than the old system of selection by routine examination, it is hardly right to speak of the election of a Natural Science Fellow, which took place in October 1870, as an "unsuccessful experiment."

It is certainly much to be regretted that circumstances have prevented the gentleman then chosen from strengthening the staff of scientific workers and teachers at Cambridge; but it is equally certain, that no system of selection that could possibly be desired, would have resulted in the election of a man possessed at once of more promising scientific abilities, and of a more genuine love for science.

The writer of the article seems to think that the examiners on that occasion were in search of what he is pleased to call a "genuine zoologist;" there is no doubt that there was then as there is now, a striking absence of young men of ability, devoting themselves to zoology; but though the college had announced a preference for a physiologist, yet the examiners were empowered to recommend either a zoologist, or one following any other branch of natural science.

F. M. BALFOUR

Trinity College, Cambridge, April 20

Permanent and Temporary Variation of Colour in Fish

ONE or two episodes in the annals of the Brighton Aquarium for the week just ended deserve a passing note.

Among the Plaice, *Pleuronectes platessa*, added to the general collection, is one remarkable example, having the posterior half of its under surface, usually white, coloured and spotted as brilliantly as the upper one; the line of demarcation between these two colours again, though sinuous, is most abrupt, there being no shading through from one to the other as might have been anticipated. This specimen may be turned to good account by advocates of the Darwinian theory, as affording a remarkable instance of the occasional tendency of a specially modified type to revert to its primæval state—the Pleuronectidæ being derived from ancestors originally possessing bilateral symmetry, and an equal degree of coloration on each side.

As the spawning season advances, many of the fish, and more especially certain of the Acanthopterygian order, undergo various important modifications in both their habits and appearance. During the last week or so, many of the larger examples of the Black Bream, or Old Wife (*Cantharus lineatus*), exhibited in tank 4 on the north side of the Western corridor, have afforded a striking illustration of these phenomena. Hitherto their prevailing tint has been a delicate silvery blue, varied by irregular longitudinal lines of pale yellow, a hue scarcely in harmony with the name by which they are most popularly known. These light colours have now disappeared, or rather become absorbed, in a prevailing shade of deep leaden black, which, while deepest on the back, spreads itself over the whole surface of the fish with the exception of a few transverse lighter bands in the region of the abdomen. The males in particular are most conspicuous for this change, and these retiring from the remainder of the shoal, select certain separate and prescribed areas at the bottom of the tank, where they commence excavating considerable hollows in the sand or shingle, by the rapid and powerful action of the tail and lower portion of their body. A depression of suitable size having been produced, each male now mounts vigilant guard over his respective hollow, and vigorously attacks and drives away any other fish of the same sex that ventures to trespass within the magic circle he has appropriated to himself. Towards his companions of the opposite sex his conduct is far different; many of the latter are now distended with spawn, and these he endeavours by all the means in his power to lure singly to his prepared hollow, now discovered to be a true nest or spawning bed, and there to deposit the myriad ova with which they are laden, which he then protects and guards with the greatest care. Whether the aggregated produce of a large number of females is thus consigned to one bed, and whether the ova are guarded by the male until the young fish make their appearance, are points which, while awaiting confirmation, may be almost confidently inferred, reasoning from the very analogous nest-forming habits of the *Gasterosteidæ* or Stickleback family, already so familiar to every naturalist. The male of the Lump fish (*Cyclopterus lumpus*) is said to watch over the spawn of the female in a very similar manner, and at the particular time of the year, early spring, when it is deposited, assumes the most lively tints of red and blue, which disappear again after his paternal duties have been discharged, and are not retained through life as has been formerly supposed. On this point we have direct evidence from specimens confined within the aquarium walls. For yet another instance of change of colour in the male fish, associated with its nest-forming habits in the same Acanthopterygian order, I am indebted to a recent visit to the aquarium at the Crystal Palace, where Mr Loyd directed my attention to a male example of the Cuckoo Wrasse (*Labrus mixtus*), which had formed a deep hollow in the sand of its tank, and was endeavouring in the most persuasive manner to induce a female of the same species to share it with him, swimming backwards and forwards between her and the completed nest, and plainly exhibiting the greatest anxiety for her to follow. The normal brilliancy of this fish was supplemented by a light opaque patch that extended over a considerable portion of the back of his head and shoulders, while the tints of the remaining portion of the body were more than ordinarily deepened.

W. SAVILLE KENT

On Approach caused by Velocity and Resulting in Vibration

PROF. J. CLERK-MAXWELL, in his recent paper on "Action at a Distance," has brought under notice again the experiments of Prof. Guthrie "On Approach caused by Vibration," and has so well summarised in popular language the facts investigated and the conclusions arrived at, that fitting opportunity appears to present itself to me for calling the attention of the scientific world to phenomena closely allied to those under review although more complex in their manifestation, since in these velocity is independent of, yet initiates vibration. That they have not been referred to in the experiments either by Prof. Guthrie, Challis, and others who have taken part in the discussion is probably to be accounted for in the unfortunate although convenient habit indulged in by experimentalists of using the tuning fork as the agent for demonstration.

The following passage from Prof. J. Clerk-Maxwell's paper alluded to will best introduce my own observations—"Here is a kind of attraction with which Prof. Guthrie made us familiar. A disc is set in vibration and is then brought near a light suspended body which immediately begins to move towards the disc as if drawn towards it by an invisible cord. What is this cord? Sir W. Thomson has pointed out, that in a moving fluid the pressure is least where the velocity is greatest. The velocity of the vibratory motion of the air is greatest near the disc. Hence the pressure of the air on the suspended body is less on the side nearest the disc than on the opposite side; the body yields to the greater pressure and moves towards the disc. The disc therefore does not act where it is not. It sets the air next it in motion by pushing it, this motion is communicated to more and more distant portions of the air in turn and thus the pressures on opposite sides of the suspended body are rendered unequal, and it moves toward the disc in consequence of the excess of pressure. The force is therefore a force of the old school, a case of *vis a tergo*, a *shove* from behind."

It has been customary with me for several years, when occasion invited it, to demonstrate to my musical friends the physical action existing in the sounding organ-pipe, to show them (taking up a chance wood-shaving lying on the floor of the workshop or a strip of tissue paper) that, heterodox though the teaching be, the stream of air at the mouth of the organ-pipe constitutes a free-reed—visibly before them the film-like wood-shaving is drawn into the motion of the air, and the beautiful curve of the reed's swing displays itself beyond dispute; then to show them that the air-moulded tongue obeys every law of the free-reed, has its own definite rate of vibration, that the current is so directed that it shall *pass* not *strike* the lip, that it is an air-moulded or aeroplastic reed as definitely fashioned in substance, strength, proportion, and form, as metal reeds are to produce a required and determinate rate of vibration. First, the velocity of current, a constant upward force; then, the periodicity of vibration as a *secondary* mode of its activity. The aeroplastic reed forming with the pipe a *system* of transverse vibration associated with longitudinal vibration, and possibly another phase of vibration across the width of the reed enabling it to synchronise with the harmonic range of the pipe; the principle of action of the whole being termed, in my non-academic phraseology, suction by velocity; but if a more exact expression is found its explanation should imply, or better still, include the axiomatic phrase of Sir W. Thomson, "in a moving fluid the pressure is least where the velocity is greatest." To state the existence of an air-moulded free-reed is to give the key to its nature. Flutes, flageolets, whistle-pipes, disc-whistles, form one group with organ-pipes; all are of one type. Then there is another group of free-reeded instruments including the vocal organs, the trumpet, bassoon, oboe, harmonium, and the like, the only distinction between the two groups being that the one possesses reeds of air of definite pitch; and the other possesses reeds of grosser substance, whether it be membrane wood or metal, alike of definite pitch, but in every one the degree of elasticity or pliancy in the substance determines how much of that pitch shall be maintained as the work is done. Velocity is power, and in every conjunction of reed and pipe the reed is the dominant. Most distinctly it should be recognised that the air-reed does *work* and expends power in doing it. A rod or a string delivers up under a single blow the whole vibrating energy it is capable of—not so the air-column in the organ-pipe, which needs to be beaten the precise number of blows requisite for the pitch of tone elicited.

Reeds of the oboe are as truly free-reeds as are the vocal cords. The stream of air does not necessarily pass down the organ-pipe, but in the oboe it is essential it should pass down the pipe. The action of this orchestral instrument is best explained under the law of "least pressure," showing an identity in principle but with difference of mode; instead of the stream with a lapping action as an air-tongue at the mouth of the organ-pipe, we have an air-current passing between two sensitive reeds down a narrow straw-like tube into the main body of the pipe. The velocity in the little tube immediately causes "least pressure" in the interior, effecting approach and closure of the pair of lip-like reeds, and so on, a perpetual renewing and breaking of contacts, the periodicity of such movement being determined by the sensitiveness of the reed in relation to the air-tube through which the impulses must move before the "dispersion of the vibrations" into the air *relieves* the reed and fixes the *period* of its stroke. In further proof that the flue organ-pipe is a free-reed instrument, compare the flute, its representative, with the oboe and clarionet. So little is understood concerning the nature of these wind instruments, that, whenever in the science of acoustics they are referred to, it is stated that the clarionet is a closed pipe, and the oboe an open pipe; that the former produces the series of uneven harmonics and the latter the even series, and the explanation given is that the tube of the one is cylindrical, and the tube of the other is conical. The explanation does not really explain. It is true that the clarionet gives in relation to its length the pitch corresponding to that of a closed pipe, whilst the oboe, though of similar length (scale of key allowed for), is of the pitch of an open pipe, with relative harmonics; yet this difference arises not in any degree from the shape of bore cylindrical or conical. As well denominate the oboe "a closed pipe" if structure is compared; the one is not more a closed pipe than the other, the true cause of the diversity is in the rate of *reed-vibration* of the clarionet being only half the rate of that natural to the oboe. The proof is clear and open to anyone intent to observe. Place the oboe head on the clarionet-tube, and you will get from this same tube only the two-feet tone instead of the four-feet tone, and with this transformation of pitch the series of harmonics previously wanting. Place the flute-head on the clarionet-tube and the same results follow; showing that the velocity of vibration originates with the reed, and that the flute rightly considered is a free-reeded instrument.

The experience of years justifies me in presenting these conclusions, and should they not be disproved, questions will suggest themselves whether physicists should not look to the disturbance of the equilibrium of air-pressure as the chief element in determining the pitch of sounds produced in organ pipes; whether the long conserved doctrine of "the column of air within being alone the cause of sound" has not been detrimental to investigation as was in older times the doctrine that "nature abhors a vacuum," which, as Whewell points out, retarded science a century by pre-occupying men's minds against observation; and whether it is not through the presence of the law of "least pressure" that vibration of any kind becomes possible.

HERMANN SMITH

The Hegelian Calculus

YESTERDAY evening a copy of NATURE for the 10th instant, sent to my late address at Piershill, reached me here. The sender annexes the initials W. R. S—those, presumably, of Mr. W. R. Smith. It was only thus that I became aware of that gentleman's letter on "The Hegelian Calculus," in said issue; and, as I am called upon by name therein, I should be obliged if, in an early number of the valuable publication referred to, you would kindly allow me insertion of this explanatory word in return.

In my rejoinder, mentioned by Mr. Smith as appearing in the current number of the *Fortnightly*, and which (rejoinder) treats, as Mr. Smith truly says himself, his own paper in the same pages "as a virtual concession of the entire case," I speak thus:—

"He that, with whatever tincture of mathematics, will but cast a single glance into the situation as it veritably is, will perceive at once that Mr. Smith's present paper is of such a character as not to demand any further answer from me. It is of such a character, however, that it may be put on the level of a business transaction, and if Mr. Smith can persuade any competent mathematician—say the greatest alive, Sylvester, he being at once mathematician, metaphysician, and German scholar, and at the same time wholly unknown to myself—if, I say, Mr. Smith can persuade any such competent expert to see in this matter with Mr. Smith's eyes, I shall consent to be mulcted in what pecuniary penalty this expert may please."

Of course with reciprocity in the other event. I hope Mr. Sylvester will kindly pardon me for having thus, almost involuntarily, made free with his name; but, if I could say the above then, certainly not less can I say the above now—after this letter of Mr. Smith's. The "character" in allusion is one, I believe, hitherto unexampled in literary controversy, and such that, as I also believe, the most important interests call forth thorough understanding of it. It is in consequence of this "character" that, as I have intimated, I cannot, with any respect to myself, enter into further direct relations with Mr. Smith, and that I must confine myself to what has been said above. All, for that part, may be confidently left to time. Napoleon snipped off, and put in his pocket the alleged gold tassel, assured that use would disclose the tinsel in suspicion. So, as regards the—to me—extraordinary operations of Mr. Smith—not but every *Kenner* must see what is concerned at a glance—I can leave them fearlessly to the intromissions of the public.

Further proceeding, let me intimate in conclusion, however formidable it may look, must, so far as I am concerned, be arranged by a friend on the one part, and a friend on the other. Longer to trouble the public with these altercations can only seem to it impertinent. I, at least, shall be satisfied if it will but consider the result in the end.

Edinburgh, April 18 J. HUTCHISON STIRLING

Moving in a Circle

I HAD to cross a very large flat field in Lincolnshire one evening; the ground covered with snow, and there being a dense fog I knew my way perfectly; but on coming to the hedge found that I had deviated *to the right*. Next day I had occasion to re-visit my track and found that I had described about one quarter of a circle.

T. M. W.

JUSTUS LIEBIG

JUSTUS LIEBIG was born at Darmstadt, the native place of many eminent chemists, May 13, 1803; died at Munich, April 18, 1873.

As generations pass away, and the deeds and capacities of great men come to be truly estimated, it will be found that the name of Liebig claims a position very close to those of Lavoisier and Dalton, the greatest leaders in our science. It is not as the author of the 317 investigations the titles of which fill the pages of the Royal Society catalogue, nor even as the father of organic chemistry, nor as the great originator of a scientific physiology and agriculture, nor again as the writer of numerous handbooks, that Liebig has done most for science; his greatest influence has been a personal one, for it is to him that most chemists now living either directly or indirectly owe their scientific existence. The Giessen Laboratory was the first one in which our science was truly taught, and from this centre the flame of original research was carried throughout all lands by ardent disciples who more or less successfully continued, both as regards tuition and investigation, their master's work.

Liebig early showed his love for experimental inquiry, and his father apprenticed him—as was then usual in the case of boys who exhibit such tastes—to an apothecary. Ten months of the shop drudgery was sufficient to convince the boy that this sort of life was not what he required, and it is said that he ran away from his pill-making; at any rate, he returned to his home in Darmstadt, and soon entered the University of Bonn, and afterwards that of Erlangen, where he met with congenial spirits, and continued his scientific education. At that time (1822), however, the German universities were almost destitute of means of stimulating research, or even of imparting a knowledge of existing science in its higher and more modern forms; and for this reason the steps of all young German chemists were naturally turned towards Paris, where Gay Lussac, Thenard, Dulong, and other well-known masters were working and teaching. In 1822, being nineteen years of age, Liebig had already made himself known in his native town and to its paternal government by the investigation of the action of alkalies on fulminating silver, as well as by other publications on the composition of certain colouring materials; and the Grand Duke, anxious to promote the glory of his capital, gave his promising young townsman the means of studying in Paris. There Liebig, thanks to the friendly introduction of Alexander von Humboldt, was allowed to work in Gay Lussac's private laboratory, where he completed his investigation on fulminic acid, and became acquainted with Gay Lussac's methods of exact investigation. In Paris, too, he met Mitscherlich and Gustav Rose, and the intercourse with them and other men of science which he there enjoyed confirmed him in the choice of his profession, and in 1824 he returned home and was appointed, when twenty-one years of age, Extraordinary, and two years afterwards the Ordinary Professor of Chemistry at Giessen, the University of his country, and the scene of the great labours and triumphs of his life.

The influence which Liebig has exerted on the progress of discovery in our science is due to his possession of that peculiar gift essential to all great investigators of nature, which unites to indomitable perseverance in following out experimental details, the higher power of generalisation. His indefatigable energy in experimental investigation must be known to all who have even turned over the pages of his Annalen; there is scarcely a volume in the thirty years dating from the commencement of the journal in 1832 to 1862, which does not contain some important record of his labours, and in the height of his power the number of independent researches which he was able to carry out at once is certainly marvellous. A mere list of even the most important of his investigations in the one branch of organic chemistry would be far too long for a brief notice such as this; it may, however, be well to call to mind his productivity during the first few years of the Giessen career. In the first rank amongst his earlier researches, and serving as a necessary basis for the whole, come those in which he placed the analysis of organic substances upon a firm and simple basis. His final description of the apparatus is worth remembering—"There is nothing new in this arrangement but its simplicity and perfect reliability." The attack on this subject, commenced in conjunction with Gay Lussac in 1823, was not completed by himself till 1830; but then he furnished chemists with the simple and effectual methods which, with slight modifications, we still employ. Thus armed, the secrets of the composition of the organic acids and alkaloids were soon revealed, and among the most important discoveries we have first amongst the acids, fulminic (1822), cyanic (1827), hippuric (1829), malic, quinic, rocellic and camphoric (1830), lactic (1832), aspartic (1833), uric (1834), then we find chloral and chloroform (1831), acetal (1832), aldehyde (1835).

In 1837 he published, in conjunction with Dumas a paper, "Note sur la constitution de quelques acides," in which for the first time the theory of polybasic organic acids was put forward, Graham's researches on the phosphates proving the polybasic character of phosphoric acid having been published in 1833. In a research on the constitution of these bodies published in 1838 this was more fully worked out, and Davy's previously expressed views as to the part played by hydrogen confirmed and supported. His researches on the cyanogen derivatives (1834), on the chlorine substitution-products of alcohol (1832), and those carried on for so many years in conjunction with his life-long friend Wohler, as on the composition of sulphovinic acid (1832), and especially that on the derivatives of benzoic acid (1832) sufficed to place the theory of organic radicals on a firm basis. Then too we must not forget their conjoint researches, chiefly carried on by correspondence between Giessen and Gottingen on the oxiacids of cyanogen (1830), a most difficult subject worked out in a masterly way, or that on the formation of benzoyl hydride from amygdalin in the bitter almond (1837), or again the memorable investigations on the nature of uric acid and the products of oxidation of this substance by nitric acid (1838), in which not only a large number of new bodies are described and allantoin artificially prepared, but system and order introduced among the whole.

One of his favourite subjects was that of Fermentation, and his explanation of the phenomena as being due to the action of a substance whose molecules are in a state of motion upon the fermentable body is yet well known, though now in the minds of most supplanted by the germ theory of Pasteur.

As a critic Liebig was sharp, satirical, and sometimes even unsparing and bitter, especially when his own views were assailed; his anonymous critiques are brimfull of good-humoured satire, whilst in others to which he gives his name, he lashes his victim most unmercifully. Who can read his "Das entrathselte Geheimniss der geistigen Gahrung" "Vorlaufig briefliche Mittheilung," 1839, without amusement? His description of the minute organisms having the form of a Beindorfschen Destillirblase (ohne den Kühlapparat) feeding on sugar and excreting alcohol (aus ein rosenroth gefarbten punkt), and carbonic acid (aus dem Harnorganen) will be long remembered, and even at the present day the satire has not lost its applicability. Then again in a letter purporting to be written from Paris and signed S. C. H. Windler, though doubtless written by Liebig, he laughs to scorn the idea that the theory of substitution, which he himself upheld, could be so far extended as was by some chemists believed possible. In this letter he states, as the last great discovery of the French capital, that it had been found possible to replace in acetate of manganese, first the atoms of hydrogen by chlorine, then the atoms of oxygen, then those of manganese, and lastly that even the atoms of carbon had been replaced by this gas. So that a body was in the end obtained, which, although it contained nothing but chlorine, still possessed the essential properties of the original acetate of manganese. He adds in a note: "Je viens d'apprendre qu'il y a déjà dans les magasins à Londres des étoffes en chlor filé, très recherchés, dans les hôpitaux, et preférés à tout autres pour bonnets de nuits, calecons, etc.!"

Those who wish to read an unsparing critique, may turn to Liebig's remarks on Gerhardt (1846), to those on Mulder as regards his protein theory, or again on Gruber and Sprengel respecting a review of his own book on Organic Chemistry (1841). It was not in Liebig's nature to spare either private persons or Governments when he thought that science would be advanced by plain speaking. In his two papers on "Der Zustand der Chemie in Oestreich" (1838), and in "Preussen" (1840), whilst he points out the shortcomings of both countries, bravely asserts, in the strongest terms, the dependence of national prosperity upon original research, a subject concerning which in England, *most people, thirty years later (to our shame be it said) are altogether in the dark!*

Other and wider questions, to the solution of which Liebig in later life turned his energies, were those respecting the establishment of a Scientific Agriculture, and the foundation of a new science of Physiological Chemistry. It is in this direction that his labours are best known to the general public in England; and there is no doubt, although in many details his views have since proved erroneous, that he was correct in the main issues, and that the stimulus given to British agriculture through Liebig's writing and investigations, has been of the most important kind. Agriculturists have thus been made aware that a scientific basis for their practice exists which, if not as yet complete, can still explain much in their art of what had previously depended on mere empiricism. Then, again, the interest and attention which were thus brought to bear on these subjects, has led to the establishment of Agricultural Colleges and "Versuchs-Stationen," and to the carrying out of researches like those magnificent ones of Lawes and Gilbert, from which we are receiving information concerning the various questions relating to plant life such as long-continued investigation and observation alone can yield.

In the year 1852, having lectured for sixty semestres in Giessen, he left the university to which he had given a world-wide fame, to become the centre of a galaxy of men of science whom Maximilian II. of Bavaria had called to Munich. There, having built himself a good laboratory and a spacious house adjoining, he spent the remainder of his days in quiet labour and well-earned and honoured repose. The active period of his life having passed, he entirely withdrew from discussions on purely theoretical questions, and occupied himself with investigations chiefly of a practical character, such as those on the extract of meat, and on infants' food. He continued to re-edit his various books, indulging occasionally in his old habit of a sharp hit at the views of some scientific brother. His last investigation and critical discussion of the labours of other chemists was published in 1870, "On Fermentation and the Origin of Muscular Force." In this he strenuously upholds his old theory of fermentation against Pasteur's explanation of the phenomena, and his views and arguments are as forcibly and clearly expressed as we find them in his early publications. The last of his hundreds of communications to the Annalen is a notice on the discovery of chloroform, published in March of last year, in which he calls attention to the fact that the discovery of this important substance is due to himself in 1831, and not to Soubeiran, as is generally supposed, although Liebig overlooked the small quantity of hydrogen (0·8 per cent.) which chloroform contains, and termed it a chloride of carbon.

As an author, Liebig is remarkable for the lucidity and grace of his style. The best examples of this are to be found in his "Familiar Letters on Chemistry." His mode of popular treatment of a somewhat obscure subject is seen in the well-known chapter (xxiv.) in his "Familiar Letters," on "Spontaneous Combustion of the Human Body." He there goes step by step through all the better authenticated cases, shows the want of sufficient evidence in each case, points out the fallacies of the theories proposed to explain them, and concludes with proving, by the application of known physical and chemical laws, that the supposed phenomena cannot possibly occur.

Looking once more back upon the labours of Liebig, we again come to the conclusion that the chief and characteristic glory of his life is the impulse which he gave to the study of our science and the personal influence which he exerted among his numerous and distinguished pupils.

The present short and imperfect sketch of the scientific bearings of a great life is not one in which personal qualities can be discussed; suffice it to say that though Liebig was an awkward adversary, he was a faithful friend, and always ready and anxious to assist deserving merit.

H. E. ROSCOE

NOTES FROM THE "CHALLENGER"

WE left Santa Cruz on the evening of Friday, the 14th of February. The weather was bright and pleasant with a light breeze—force equal to about 5—from the north-east. Our course during the night lay nearly westward, and on the morning of the 17th we sounded, about 75 miles from Teneriffe, and 2,620 miles from Sombrero Island, the nearest point in the Virgin group, in 1,891 fathoms, with a bottom of grey globigerina ooze, mixed with a little volcanic detritus. The average of two Miller-Casella thermometers gave a bottom temperature of 2° C.

The slip water-bottle which was used by Dr. Meyer and Dr. Jacobsen in the German North-Sea Expedition of last summer was sent down to the bottom, and Mr. Buchanan determined the specific gravity of the bottom water to be 1·02584 at a temperature of 17°·9 C., the specific gravity of surface water being 1·02648 at a temperature of 18°·5 C.

All Sunday, the 16th, we spent sailing with a light air from the northward, and by Monday morning we had made about 130 miles from our previous sounding. The dredge was put over at 5.15 A.M. with 2,700 fathoms rope, and a weight of 2 cwt. 300 fathoms before the dredge.

After steaming up to the dredge once or twice, hauling-in was commenced at 1.30 P.M., and the dredge came up at 3.30 half full of compact yellowish ooze. The ooze was carefully sifted, but nothing was found in it with the exception of foraminifera, some otolites of fishes, some dead shells of pteropods, and one mutilated specimen of what appears to be a new Gephyrean. This animal has been examined by Dr. von Willemoes-Suhm, who finds that it shows a combination of the character of the Sipunculacea and the Priapulacea. As in the former group, the excretory orifice is near the mouth, in the anterior part of the body, while, as in the latter, there is no proboscis and there are no tentacles. The pharynx is very short, and is attached to the walls of the body by four retractor muscles. The pharynx shows six to seven folds ending in a chitinous border. The mouth is a round aperture, beset with small cuticular papillæ. The perisom is divided into four muscular bands, the surface large, showing a tissue of square meshes, in each of which there are four to five sense-bodies. For the reception of this singular species Dr. von Willemoes-Suhm proposes to establish the genus *Leioderma*, which will represent a family intermediate between the Sipunculids and the Priapulids.

On the 18th we sounded at 9 A.M. in 1,525 fathoms, lat. 25° 45′ N., long. 20° 12′ W., 160 miles S.W. of the Island of Ferro, and 50 miles to the west of the station of the day before, in 1,525 fathoms. The "Hydra" tube brought up no bottom, and we sounded again with a depth of 1,520 fathoms, and again no bottom. It thus seemed that we had got upon hard ground, and as the sounding of the following day gave 2,220 at a distance of only 19 miles, we had evidently struck the top of a steep rise. The dredge was lowered at 10 A.M. with 2,220 fathoms of line and 2 cwt. leads 300 fathoms before the dredge. At 5.30 P.M. the dredge was hauled up, and contained a few small pieces of stone resembling the volcanic rocks of the Canary Islands, and some large bases of attachment and some branches of the calcareous axis of an Alcyonarian polyp allied to *Corallium*. Some of the larger stumps were nearly an inch in diameter; the central portion very compact, and of a pure white colour: the surface longitudinally grooved, and of a glossy black. The pieces of the base of the coral which had been torn off by the dredge were in one or two cases several inches across and upwards of an inch thick, forming a thick crust from which the branches of the coral sprang. The crust was of a glossy black on the surface, showing a fine regular granulation, and a fracture through the crust was of a uniform dark brown colour and semi-crystallised. The whole of the coral was dead and appeared to have been so for a long time. It was so fresh in its texture, however, that it was scarcely possible to suppose that it was sub-fossil, although from the comparatively great depth at which it was found, and the many evidences of volcanic action over the whole of this region, one could scarcely avoid speculating whether it might not have lived at a higher level and been carried into its present position by a subsidence of the sea-bottom. I hope we may have an opportunity of determining this question in returning over the same ground later in the season.

Attached to the branches of the coral there were several specimens of a magnificent sponge belonging to the Hexactinellidæ. One specimen, consisting of two individuals united together by their bases, is about 60 centimetres across, and has very much the appearance of the large example of the tinder-fungus attached to the trunk of a tree (Fig. 1). Both surfaces of the sponge are covered with a delicate network of square meshes closely resembling that of *Hyalonema*, and formed by spicules of almost the same patterns. The sponge is bordered by a fringe of fine spicules, and from the base a large brush of strong, glassy, anchoring spicules project, fixing it to its place of attachment. The form of the barbed end of the anchoring spicules is as yet unique among sponges. Two wide, compressed flukes form an anchor very much like that of one of the skin-spicules of *Synapta*. The sponge when brought up was of a delicate cream colour. It was necessary to steep it in fresh water to free it from salt, and the colour changed to a leaden grey. A number of small examples of the sponge, some of them not much beyond the condition of gemmules, were found attached to the larger specimens and to branches of the coral, so that we have an opportunity of studying the earlier stages of its development.

For this sponge, which forms the type of a new genus, I propose the name *Poliopogon*[1] *amadou*.

Attached to the sponge were two examples of a fine Annelid which Dr. v. Willemoes-Suhm refers to the family Amphinomidæ, sub-family Euphrosyninæ, with many of the characters of the genus *Euphrosyne*. The body is 12 mm. long and 5 mm. broad, and consists of fifteen segments. The surface of the head is covered with a caruncle extending over the anterior segments, and the whole surface is clothed with milk-white two-branched setæ, which radiate over each segment like a fan.

On the following day a series of temperatures were taken from the surface to 1,500 fathoms at intervals of 100 fathoms.

Depth.	Temp.	Depth.	Temp
Surface	19°·5C.	800 fathoms . .	5°·6C.
100 fathoms	17·2	900 ,, . . .	4·7
200 ,, ...	13·7	1000 ,, . . .	4·6
300 ,, ...	11·0	1100 ,, . . .	3·8
400 ,, ...	9·5	1200 ,, . . .	3·5
500 ,, ...	7·6	1300 ,, . . .	3·1
600 ,, ...	6·5	1400 ,, . . .	2·8
700 ,, ...	6·2	1500 ,, . . .	2·6

The dredge was not used, but, as is our custom whenever the rate of the ship is such as to make it practicable, a large towing-net was put out astern.

In hot, calm weather the towing-net is usually unsuccessful. It seems that the greater number of pelagic forms retire during the heat of the day to the depths of a few fathoms, and come up in the cool of the evening and in the morning, and in some cases in the night. The larger phosphorescent animals are frequently abundant during the night round the ship and in its wake, while none are taken in the net during the day. Mr. Moseley has been specially engaged in working up the developmental stages of *Pyrosoma*, and the intricate structure of the tissues and organs of some of the surface groups, whose extreme transparency renders them particularly suitable for such researches.

Feb. 21.—Up to 2.15 P.M. sailing under all plain sail at the rate of six knots an hour before the N.E. trades, force 3 to 4.

The dredge was put over at 5 P.M. with 3,400 fathoms of line, and was kept down till one o'clock A.M. on the following morning, the ship drifting slowly. Our position at noon on the 21st was about 500 miles S.W. of Teneriffe, lat. 24° 22′ N., long. 24° 11′ W., Sombrero Island S. 58° W. 2,220 miles. Work began early on the 22nd, and the dredge, which had begun its ascent at 1.15 A.M., came up at 5.45 half full of a yellowish ooze, which was not so tenacious as usual, and on the whole singularly poor in higher living things. A careful and laborious sifting of the whole mass gave us three small living mollusca, referred to the genera *Arca*, *Limopsis*, and *Leda*; and two Bryozoa apparently undescribed. Foraminifera were abundant, many examples of miliolines being of unusually large size. Some beautiful radiolarians were sifted out of the mud. These may have been taken into the dredge on its way up, or more probably they may have lived on the surface or in intermediate water and have sunk to the bottom after death, since they consist of continuous fenestrated shells of silica.

On Tuesday the 25th a small dredge was lowered at 6.30 A.M. with 3,500 fathoms of line (2,500 fathoms of 2¼ in. rope and 1,000 of 2-in.), and 2 cwt. leads attached 300 fathoms in advance. At 7.30 we sounded in 2,800 fathoms, with a bottom of the same reddish ooze, and a temperature of 2° C. A series of temperatures were taken at intervals of 100 fathoms down to 1,000, the result agreeing closely with those of the previous series. At 5.15 P.M. the dredge came up clean and empty. It had either never reached the bottom, owing to some local current or the drift of the ship, or else everything had been completely washed out of it on its way to the surface. The bottom water gave a specific gravity of 1·02504 at 10°·6 C., that of the surface being 1·02617 at 21°·3 C. While sounding, the current-drag was tried, and indicated a slight north-westerly current.

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[1] Πολιός, white, and πώγων, a board.

As the attempt to dredge on the previous day had been unsuccessful, it was determined to repeat the operation with every possible precaution on the 26th. The morning was bright and clear, and the swell, which had been rather heavy the day before, had gone down considerably. A sounding was taken about 10 o'clock A.M. with the "Hydra" machine and 4 cwt. The sounding was thoroughly satisfactory, a sudden change of rate in the running out of the line indicating in the most marked way when the weight had reached the bottom. During the sounding a current-drag was put down to the depth of 200 fathoms, and it was then ascertained that, by means of management and by meeting the current by an occasional turn of the screw, the ship scarcely moved from her position during the whole time the lead was running out. The depth was 3,150 fathoms; the bottom a perfectly smooth red clay, containing scarcely a trace of organic matter—merely a few coccoliths, and one or two minute granular masses. The thermometer indicated a bottom temperature of 1°·9 C.

The small dredge was sent down at 2.15 P.M. with two hempen tangles; and, in order to ensure its reaching the bottom, attached to the iron bar below the dredge which is used for suspending the tangles, a "Hydra" instrument with detaching weight of 3 cwt. Two additional weights of 1 cwt. each were fixed to the rope 200 fathoms before the dredge. 3,600 fathoms of rope were payed out—1,000 fathoms 2 in. in circumference, and the remainder (2,600 fathoms) 2½ in. The dredge came up at 10.15 P.M. with about 1 cwt. of red clay.

This haul interested us greatly. It was the deepest by several hundred fathoms which had ever been taken, and, at all events coincidentally with this great increase in depth, totally different from what we had been in the habit of meeting with in the depths of the Atlantic. For a few soundings part of the ooze had been assuming a darker tint, and showed on analysis a continually lessening amount of calcareous matter, and, under the microscope, a smaller number of foraminifera. Now calcareous shells of foraminifera were entirely wanting, and the only organisms which could be detected after washing over and sifting the whole of the mud with the greatest care, were three or four foraminifera of the Cristellarian series, with their tests made up of particles of the same red mud. The shells and spines of surface animals were entirely wanting; and this is the more remarkable as the clay-mud was excessively fine, remaining for days suspended in the water, looking in colour and consistence exactly like chocolate, indicating therefore an almost total absence of movement in the water where it is being deposited. When at length it settles, it forms a perfectly smooth red-brown paste, without the least feeling of grittiness between the fingers, as if it had been levigated with extreme care for a process in some refined art. On analysis it is almost pure clay, a silicate of alumina and the sesquioxide of iron, with a small quantity of manganese.

It is of course a most interesting question whether the peculiar nature of this deposit is connected in any way with the extreme depth. I am certainly inclined at present to believe that it is not. The depth at Station 5 was 2,740 fathoms, and on that occasion foraminifera were abundant, and several bivalve mollusca were taken living. I cannot believe there can be any difference between a depth of 2,740 fathoms and one of 3,150 so essential as to arrest the life of the organisms to the secretions of whose tests the grey Atlantic ooze is due. I am rather inclined in the meantime to attribute this peculiar deposit to the movement of water from some special locality—very possibly the mouths of the great South American rivers—the movement possibly directed in some measure by the form of the bottom. This, however, is a question for the solution of which we may hope to procure sufficient data.

WYVILLE THOMSON

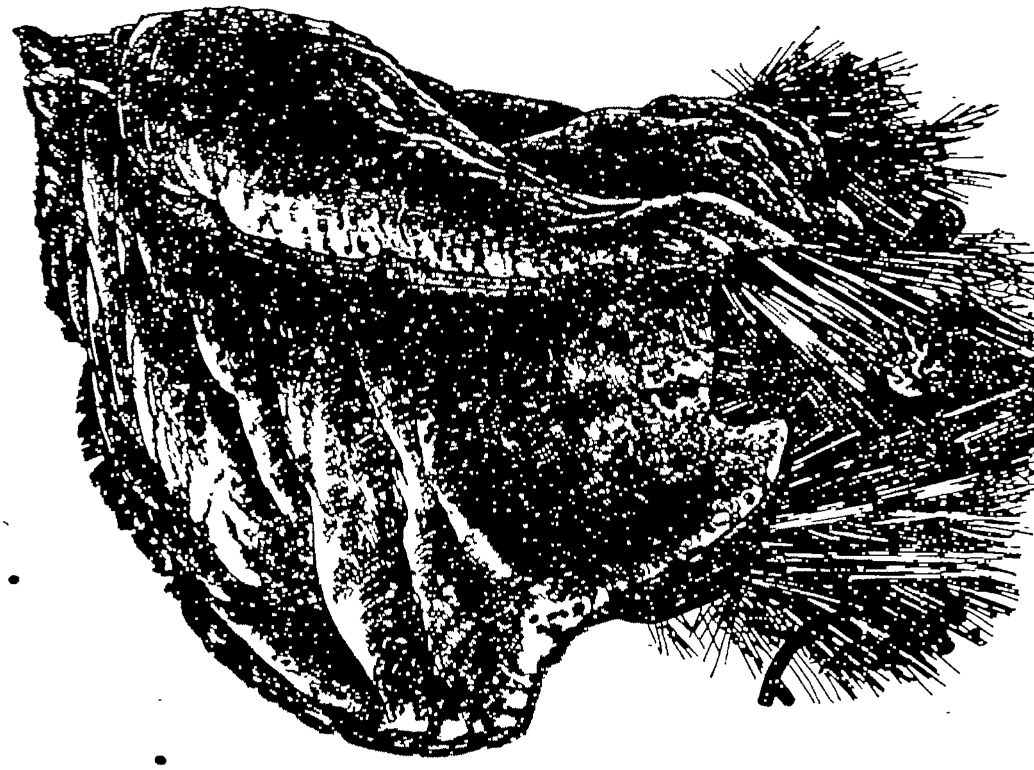

FIG. 1.—POLIOPOGON AMADOU WY. T.

ON THE ORIGIN AND METAMORPHOSES OF INSECTS *

III.

THE INFLUENCE OF EXTERNAL CONDITIONS ON THE FORM AND STRUCTURE OF LARVÆ

THE facts recapitulated very briefly in the preceding chapters show, that the forms of insect larvæ depend greatly on the group to which they belong. Thus the same tree may harbour larvæ of Diptera, Hymenoptera, Coleoptera, and Lepidoptera; each presenting the form typical of the group to which it belongs.

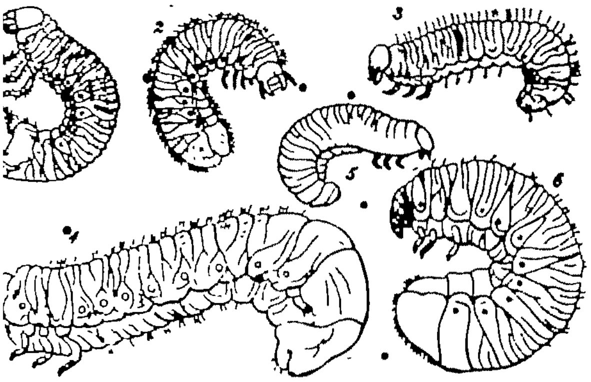

FIG. 1, Larva of the Cockchafer (Melolontha) (Westwood. Int. to the Modern Classification of Insects, v. 1., p. 194.) 2, Larva of Cetonia, 3, Larva of Trox 4, Larva of Oryctes. 5, Larva of Aphodius (Chapuis and Candeze, Mem Soc. Roy. Liege, 1853. 6, Larva of Lucanus. (Packard, "Guide to the Study of Insects," Fig. 403).

If, again, we take a group, such, for instance, as the Lamellicorn beetles, we shall find larvæ extremely similar in form, yet very different in habits. Those for instance of the common cockchafer (Fig. 1) feed on the roots of grass, those of *Cetonia aurata* (Fig. 2) are found in ants' nests; the larvæ of the genus Trox (Fig. 3) on dry animal substances; of Oryctes (Fig. 4) in tan-pits: of Aphodius (Fig. 5) in dung; of Lucanus (the stag-beetle, Fig. 6) in wood.

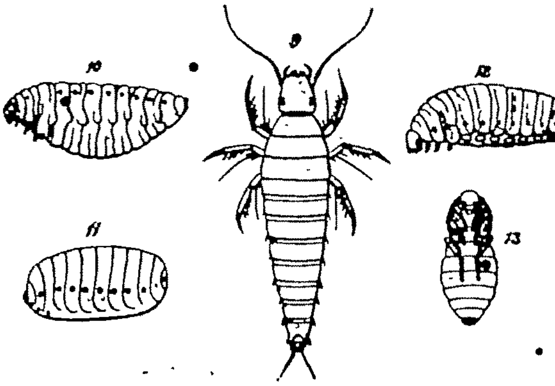

FIG 9, Larva of Sitaris humeralis (Fabre, Ann. d. Sci. Nat. Ser. 4, vol. vii) 10, Larva of Sitaris humeralis, in the second stage. 11, Larva of Sitaris humeralis, in the third stage. 12, Larva of Sitaris humeralis, in the fourth stage. 13, Pupa of Sitaris.

In the present chapter it will be my object to show that the form of the larva depends also very much on its mode of life. Thus, those larvæ which are internal parasites, whether in animals or plants, belong to the vermiform state; and the same is the case with those which live in cells, and depend on their parents for food. On the other hand, larvæ which burrow in wood have strong jaws and generally somewhat weak thoracic legs; those which feed on leaves have the thoracic legs more developed, but less so than the carnivorous species. Now, the Hymenoptera, as a general rule, belong to the first category: the larvæ of the Ichneumons, &c., which live in animals,—those of the Cynipidæ, which inhabit galls,—and those of ants, bees, wasps, &c., which are fed by their parents, are all fleshy, apodal grubs. On the other hand, the larvæ of *Sirex*, which are wood-burrowers, quit the type which is common to the majority of the order, and remain in the egg until they have developed small thoracic legs. Again, the larvæ of the Tenthredinidæ, which feed upon leaves, closely resemble the caterpillars of Lepidoptera, even to the presence of abdominal prolegs. There is, however, some little variety in this respect, some species having eleven pairs, some ten, some nine, while the genus *Lyda* has only the three thoracic pairs.

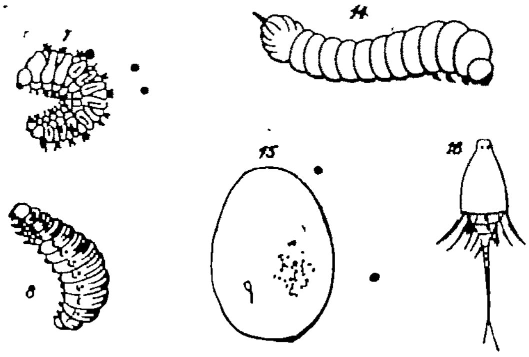

FIG 7, Larva of Brachytarsus (Ratzeburg, Forst Insecten). 8, Larva of Crioceris (Westwood, l.c) 14, Larva of Sirex (Westwood l.c.) 15, Egg of Rhynchites, showing the parasitic larva in the interior. 16, the parasitic larva more magnified

Again, the larvæ of beetles are generally active, hexapod, and more or less flattened: but on the other hand with those species which live inside vegetable tissues, such as the weevils, they are apod fleshy grubs, like those of Hymenoptera. Pl 2, Fig. 6, represents the larva of the nut-weevil, Balaninus (Pl. 1, Fig. 6), and it will be seen that it closely resembles Pl. 2, Fig. 5, which represents that of a fly (Anthrax), Pl. 1, Fig. 5, and Pl. 2, Figs. 7, 8, and 9, which represent respectively those of a Cynips or gall-fly (Pl. 1, Fig. 7), an ant (Pl. 1, Fig. 8), and wasp (Pl. 1, Fig. 9). Nor is this the only group of Coleoptera which affords us examples of this fact. Thus in the genus Scolytus (Pl. 1, Fig. 4), the larvæ (Pl. 2, Fig. 4),

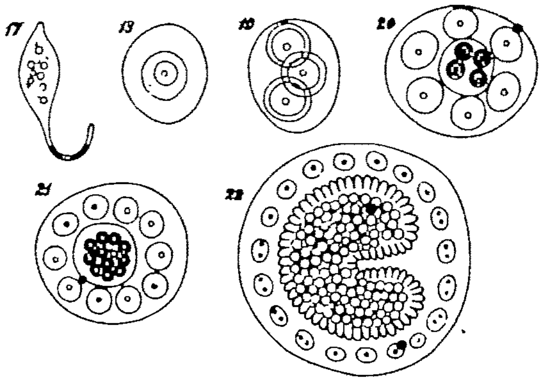

FIG. 17, Egg of Platygaster (after Ganin) 18, Egg of Platygaster showing the central cell. 19, Egg of Platygaster after the division of the central wall 20, Egg of Platygaster more advanced 21, Egg of Platygaster more advanced. 22, Egg of Platygaster showing the rudiment of the embryo.

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* Continued from vol. vii. p. 489.

which, as already mentioned, feed on the bark of the elm, closely resemble those just described, as also do those of Brachytarsus (Fig. 7). On the other hand the larvæ of certain beetles feed on leaves, like the caterpillars of Lepidoptera; thus the larva of *Crioceris Asparagi* (Fig. 8), which, as its name denotes, feeds on the asparagus, closely resembles that of certain Lepidoptera, as for instance of *Thecla spini*. A striking illustration of this is afforded by the genus Sitaris (Pl. 3, Fig. 4), a small beetle allied to Cantharis, the blister-fly, and Meloe, the oil-beetle. The habits of this species have been very carefully investigated by M. Fabre.*

The genus Sitaris is parasitic on Anthophora, in the galleries in which it lays its eggs. These are hatched at the end of September or beginning of October; and M. Fabre not unnaturally expected that the young larvæ, which, as already mentioned, are active little creatures with six serviceable legs (Fig. 9), would at once eat their way into the cells of the Anthophora. No such thing: till the month of April following they remain without leaving their birth-place, and consequently without food; nor do they in this long time change either in form or size. M. Fabre ascertained this, not only by examining the burrows of the Anthophoras, but also by direct observation of some young larvæ kept in captivity. In April, however, his specimens at last threw off their long lethargy, and hurried anxiously about their prisons. Naturally inferring that they were in search of food, M. Fabre supposed that this would consist either of the larvæ or pupæ of the Anthophora, or of the honey with which it stores its cell. All three were tried without success. The two first were neglected, and when placed on the latter the larvæ hurried away, or perished in the attempt, being evidently unable to deal with the sticky substance. M. Fabre was in despair: "Jamais expérience," he says, "n'a éprouvé pareille déconfiture. Larves, nymphes, cellules, miel, je vous ai tous offert; que voulez-vous donc, bestioles maudites?" The first ray of light came to him from our countryman, Newport, who ascertained that a small parasite found by Léon Dufour on one of the wild bees, and named by him Triungulinus, was, in fact, the larva of the Meloe. The larvæ of Sitaris much resembled Dufour's Triungulinus; and acting on this hint, M. Fabre examined many specimens of Anthophora, and at last found on them the larvæ of his Sitaris. The males of Anthophora emerge from the pupæ before the females, and he ascertained that as they come out of their galleries, the little larvæ fasten upon them. Not, however, for long: their instinct teaches them that they are not yet in the straight path of development; and, watching their opportunity, they pass from the male to the female bee. Guided by these indications, M. Fabre examined several cells of Anthophora: in some, the egg of the Anthophora floated by itself on the surface of the honey; in others, on the egg, as on a raft, sat the still more minute larva of the Sitaris. The mystery was solved. At the moment when the egg is laid, the Sitaris-larva springs upon it. Even while the poor mother is carefully fastening up her cell, her mortal enemy is beginning to devour her offspring. For the egg of the Anthophora serves not only as a raft, but as a repast. The honey, which is enough for either, would be too little for both; and the Sitaris, therefore, in its first meal, relieves itself from its only rival. After eight days the egg is consumed, and on the empty shell the Sitaris undergoes its first transformation, and makes its appearance in a very different form as shown in Fig. 10.

The honey which was fatal before is now necessary; the activity which before was necessary, is now useless; consequently, with the change of skin the active, slim larva changes into a white, fleshy grub, so organised as to float on the surface of the honey, with the mouth below, and the spiracles above the surface; "grâce à l'embonpoint du ventre," says M. Fabre, "la larve est à l'abri de l'asphyxie." In this state it remains till the honey is consumed; then the animal contracts, and detaches itself from its skin, within which the other transformations take place. In the next stage, which M. Fabre calls the pseudo-chrysalis (Fig. 11), the larva has a solid corneous envelope and an oval shape, and in its colour, consistency, and immobility reminds one of a Dipterous pupa. The time passed in that condition varies much. When it has elapsed, the animal moults again, and again changes its form, and assumes that shown in Fig. 12; after this it becomes a pupa (Fig. 13) without any remarkable peculiarities; and finally, after these wonderful changes and adventures, in the month of August the perfect Sitaris (Pl. 3, Fig. 4) makes its appearance.

On the other hand, there are cases in which larvæ diverge remarkably from the ordinary type of the group to which they belong, without, as it seems in our present imperfect state of information, any sufficient reason.

Thus the ordinary type of Hymenopterous larvæ, as we have already seen, is a fleshy apod grub; replaced however in the leaf-eating and wood-boring groups, Tenthredinidæ and Sirecidæ (Fig. 14) by caterpillars, more or less closely resembling those of Lepidoptera. There is, however, a group of minute Hymenoptera, the larvæ of which reside within the eggs or larvæ of other insects. It is difficult to understand why these larvæ should differ from those of Ichneumons, but as will be seen by the accompanying figures, they assume very remarkable and grotesque forms. The first of these curious larvæ was observed by De Filippi,*

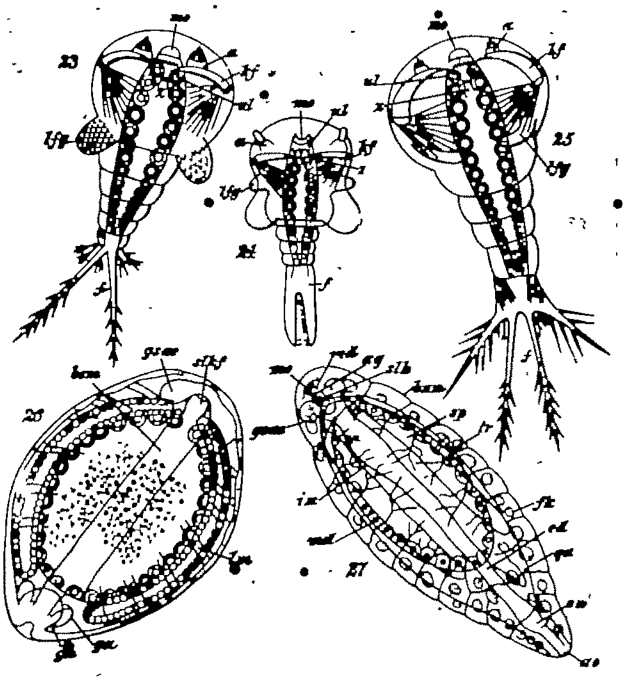

FIG. 23, Larva of Platygaster (after Ganin)—*m o*, mouth; *a*, antennæ; *hf*, hooked feet; *r*, toothed process; *lfg*, lateral process, *f*, branches of the tail. 24, Larva of another species of Platygaster. The letters indicate the same parts as in the preceding figure. 25, Larva of a third species of Platygaster The letters indicate the same parts as in the preceding figures. 26, Larva of Platygaster in the second stage—*m o*, mouth; *slhf*, œsophagus; *gsoe*, supra-œsophagal ganglion; *lm*, muscles; *bsm*, nervous system; *gagh*, rudiments of the reproductive glands. 27, Larva of Platygaster in the third stage—*m o*, mouth, *md*, mandibles; *gsoe*, supra-œsophagal ganglion; *slk*, œsophagus; *ag*, ducts of the salivary glands; *bnm*, ventral nervous system, *sp*, salivary glands, *msl*, stomach, *im*, imaginal discs; *tr*, tracheæ; *fk*, fatty tissue; *ed*, intestine; *go*, rudiments of reproductive organs, *ew*, wider portion of intestine; *ao*, posterior opening.

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* Ann. des. Sc. Nat. V. vii. T. 4. See also Natural History Review, April 1862.

* Ann. and Mag. of Nat. His., 1852.

who had collected some of the transparent ova of *Rhynchites betuleti* and to his great surprise found more than half of them attacked by a small parasite, which proved to be the larva of a minute Hymenopterous insect belonging to the Pteromalidæ. Fig. 15 shows the egg of Rhynchites, with the parasitic larva, which is represented on a larger scale in Fig. 16. Recently, however, this group has been more completely studied by M. Ganin,* who thus describes the development of Platygaster. The egg, as in other allied hymenopterous families, for instance in Cynips, is elongated and club-shaped (Fig. 17). After a while a large nucleated cell appears in the centre (Fig. 18); this is a new formation not derived from the germinal vesicle. This nucleated cell divides (Fig. 19) and subdivides. The outermost cells continue the same process, thus forming an outer investing layer. The central one, on the contrary, enlarges considerably, and develops within itself a number of daughter cells (Figs. 20 and 21), which gradually form themselves into a mulberry-like mass, thus giving rise to the embryo (Fig. 22).

Ganin met with these larvæ in those of a small gnat, Cecidomyia. Sometimes as many as fifteen parasites occurred in one host, but as a rule only one attained maturity. The three species of Platygaster differed considerably in form, as shown in the three following Figs. (23-25). They creep about in the egg by means of the strong hooked feet, *kf*, somewhat aided by movements of the tail. They possess a mouth, stomach, and muscles, but the nervous, vascular, and respiratory systems do not make their appearance until later. After some time the larva changes its skin and assumes the form represented in Fig. 26. In this moult the last abdominal segment of the first larva is entirely thrown off: not merely the outer skin as in the case of the other segments, but also the hypodermis and the muscles. This larva, as will be seen by the figure, is in the form of a barrel or egg, and ·870 mm. in length, the external appendages having disappeared, and the segments being indicated only by the arrangement of the muscles; *stkf* is the œsophagus leading into a wide stomach which occupies nearly the whole body, *sgsae* is the rudiment of the supraœsophagal ganglia, *bsn* the ventra nervous cords. The ventral nervous mass has the form of a broad band, with straight sides; it consists of embryonal cells, and remains in this undeveloped condition, during the whole larval state.

At the next moult the larva enters its third state, which, however, as far as the external form (Fig. 27) is concerned, differs from the second only in being somewhat more elongated. The internal organs, however, are much more complex and complete. The tracheæ have made their appearance, and the mouth is provided with a pair of mandibles. From this point the metamorphoses of Platygaster do not appear to differ materially from those of other Hymenoptera.

An allied genus, Polynema, has also very curious larvæ. The perfect insect is aquatic in its habits, swimming by means of its wings; flying, if we may say so, under water. It lays its eggs inside those of Dragon flies; and the larva, as shown in Fig. 28, leaves the egg in the form of a bottled-shaped mass of undifferentiated embryonal cells, covered by a thin cuticle, but without any trace of further organisation. Protected by the egg shell of the Dragon fly, the young Polynema is early able to dispense with its own; and bathed in the nourishing fluid of the Dragon fly's egg, it imbibes nourishment through its whole surface, and increases rapidly in size. The digestive canal gradually makes its appearance, the cellular mass forms beneath the original cuticle a new skin, distinctly divided into segments, and provided with certain appendages. After a while the old cuticle is thrown off, and the larva gradually assumes the form shown in Fig. 29. *asck* are the antennal discs, or rudiments of the antennæ, *flsch* of the wings, *bsch* of the legs, *vfg* are lateral projections, *gsch* of the ovipositor, &c., *fk* is the fatty tissue. The subsequent metamorphoses of Polynema offer no special peculiarities.

From these facts—and, if necessary, many more of the same nature might have been brought forward—it seems to me evident that while the form of any given larva depends to a certain extent on the group of insects to which it belongs, it is also greatly influenced by the external conditions to which the animal is subjected; that it is a function of the life which the larva leads and of the group to which it belongs.

The larvæ of insects are generally regarded as being nothing more than immature states—as stages in the development of the egg into the imago; and this might more especially appear to be the case with those insects in which the larvæ offer a general resemblance in form and structure (excepting of course so far as relates to the wings) to the perfect insects. Nevertheless we see that this would be a very incomplete view of the case. The larva and pupa undergo changes which have no relation to the form which they will ultimately assume. With a general tendency, as regards size and the production of wings, to this goal, there are combined other changes bearing reference only to their existing wants and condition. Nor is there in this, I think, anything which need surprise us. External circumstances act on the insect in its preparatory states, as well as in its perfect condition. Those who believe that animals are susceptible of great, though gradual, change through the influence of external conditions, whether acting, as Mr. Darwin has suggested, through natural selection, or in any other manner, will see no reason why these changes should be confined to the mature animal. And it is evident that creatures which, like the majority of insects, live during different parts of their existence in very different circumstances, may undergo considerable changes in their larval organisation, in consequence of forces acting on their larval condition; not, indeed, without affecting, but certainly without affecting to any corresponding extent, their ultimate form.

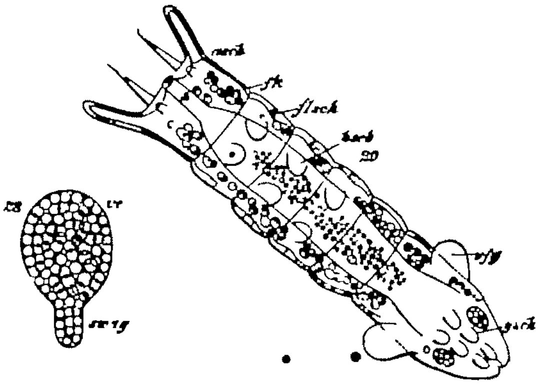

Fig. 28. Embryo of Polynema (after Ganin) 29, Larva of Polynema, *asck*, rudiments of the antennæ; *flsck* of the wings; *bsck* of the eggs; *vfg*, lateral projections; *gsck*, rudiments of the ovipositor, *fk*, fatty tissue

I conclude, therefore, that the form of the larva in insects, whenever it departs from the original vermiform—or the later Campodea—type, depends in great measure on the conditions in which it lives. The external forces acting upon it are different from those which affect the mature form; and thus changes are produced in the young, which have reference to its immediate wants rather than to its final form.

And, lastly, as a consequence, that metamorphoses may be divided into two kinds, developmental and adaptional.

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* Zeits. f. Wiss. Zool., 1869.

NOTES

THE following are the names of the fifteen candidates who have been selected by the Council of the Royal Society, for election this year into that body:—William Aitken, M.D., Sir Alexander Armstrong, M.D., K.C.B., Robert Stawell Ball, LL.D., John Beddoe, M.D., Frederick Joseph Bramwell, C.E., Staff-Captain Edward Kilwick Calver, R.N., Robert Lewis John Ellery, F.R.A.S., Lieut.-Col. J. Augustus Grant, C.B., C.S.I., Clements Robert Markham, C.B., George Edward Paget, M.D., George West Royston-Pigott, M.D., Osbert Salvin, M.A., The Hon. John William Strutt, M.A., Henry Woodward, F.G.S., James Young, F.C.S.

THE University of Cambridge has accepted the offer made by Dr. Anton Dohrn of the Zoological Station at Naples, through Dr. Michael Foster and Prof. Newton, of a working table in the laboratory of the station; and last week, on the recommendation of the Board of Natural Sciences, a grace passed the senate without opposition to the effect that from the Worts Travelling Bachelors' Fund the sum of 100*l.* per annum be granted for three years, for the purpose of securing to such members of the University, as the Board shall from time to time nominate, facilities of studying in the station.

WITH reference to a short article entitled "Survival of the Fittest," in NATURE, vol. vii. p. 404, Prof. L. Agassiz writes us that the observations therein attributed to him are taken from an unauthorised newspaper report, from which we infer that he disclaims them.

WITH reference to our report of the American Philosophical Society for August 16, 1872 (NATURE, vol. vii. p. 335), Prof. Cope writes that we have been misinformed as to the date at which his communication on the discovery of Proboscidia in the Wyoming Eocene was communicated to the Society. The paper was not announced to the Society till its meeting on September 20, and was not published till February 6, 1873.

MR. PENGELLY writes us that the specimens referred to by Mr. Everett (NATURE, April 17) did reach him through Mr. Everett's mother, and were duly acknowledged. The labels were rotten with wet, and the specimens consisted of shells and bones, the latter including human teeth and portions of a skull, incisors of some rodent, and a large hog-like molar.

PENIKESE ISLAND, the gift of which for the study of natural history to Prof. Agassiz by Mr. Anderson we have already more than once spoken of, was handed over by the donor on Monday, April 21, in a very simple way, accompanied by some speech-making. Prof. Agassiz and his generous admirer then met for the first time, and for the first time Agassiz set foot on the future sphere of his labours. The short deed of conveyance was read and handed over, and Prof. Agassiz briefly returned thanks, announcing that he intended to christen the institution to be founded on the island, "The Anderson School of Natural History." Preparations for the school, which will open this summer, will be immediately commenced. Plans have already been drawn for a two-story wooden building 100 ft. long and 25 ft. wide. The lower floor is intended for laboratories and working-rooms, of which there will be eight, with a large hall. The second story will contain twenty-six sleeping-rooms, two bath-rooms, and a large room for the Superintendent of the Institution. Several friends of Mr. Anderson in New York have become interested in the school, and will probably give liberally towards its endowment. The island of Penikese, Penekese, or Penequese, and often called Pune by the pilots, is one of a group of the Elizabethan Isles, lying between Buzzard's Bay and Vineyard Sound, and stretching southward from Cape Cod to a point nearly opposite the coast of Rhode Island. Penikese is just inside and on starboard hand of the entrance to Buzzard's Bay. It is twelve miles from New Bedford. The island is three-fourths of a mile long and half a mile wide, and contains ninety-seven acres of land, some of which is of good quality. A young tree was pointed out that had grown in one season higher than anybody in the party could reach. The surface is hilly, the highest point being about a hundred feet above the water. Mr. Anderson reserves a peninsula of some fifteen acres on the east end of the island, and here he proposes to build a house next year. Prof. Agassiz states that Penikese is a much better location for the school than the one originally contemplated at Nantucket. The school is to be devoted mainly to the study of fish and marine objects in the summer season, and a much larger variety is found in Penikese. The Sound and waters in the vicinity of Nantucket have almost invariably a sandy bottom, while the diversity in marine topography in Buzzard's Bay invites and fosters a corresponding variety of animal and vegetable life.

AT the meeting of the Iron and Steel Institute recently held in London, Mr. Lowthian Bell was elected president, and delivered a very interesting address. He pointed out the great success which had attended the organisation of the society, which although only in the fifth year of its existence, now numbered on its rolls 522 members. He expressed his opinion that the Institute had far from reached its limits. Referring then to the instances which still exist here and there, of a disregard for scientific inquiry, the result, perhaps, of considerable success effected independently of philosophical research, in which cases practical experience, as it is called, is the only rule admitted, Mr. Bell remarked, that on the other hand, abstract science, correct as it may be in every step employed in its elaboration, when introduced into the workshop may be found unable to stand the rude but inevitable test of commercial practicability; hence the necessity of a convenient method of effecting a sound union between these two great principles, and to obtain this was the object of the organisation of the Iron and Steel Institute, where are brought face to face men, some distinguished for their practical knowledge, and others equally eminent for their attachments to scientific observation. He then proceeded to consider the present aspect of foreign competition, and thought the progress in other countries in iron manufacture had arisen from an adaptation of our own appliances, and not from any important discoveries abroad. In speaking of the recent scarcity of coal, although it was his impression that an important addition can and will be made to their present output, he yet contemplated the possibility of a time being now approaching, when "any extension of manufacturing operation in this country would have to be regulated, not by the requirements of society for their produce, but by the means our coal mines might possess of furnishing the fuel required. Mr. Bell, after referring to several improvements in the plant and processes for manufacturing iron, looking forward to the future, expressed his opinion that, unless new discoveries of coal be made in Europe, the great rival we have to fear in the iron manufacture is the United States, which possesses unlimited quantities of ores of the finest quality, and such enormous deposits of coal, that our own wealth in that mineral is but comparative poverty. At the proceedings on April 30, a paper by Dr. C. William Siemens, "On the Manufacture of Iron and Steel by Direct Process," was read. Dr. Siemens described his rotative regenerative gas furnace.

A SPECIAL meeting of the Council and Natural History Committee of the Asiatic Society was held at Calcutta a few weeks since, for the purpose of considering Mr. Schwendler's scheme for the establishment of a Zoological Garden in Calcutta. After considerable discussion it was resolved that the Council of the Society should once more record their opinion as to the great advantage to Natural History Science, as well as to the public which would result from the successful establishment of a Zoological Garden. In addition a Committee was appointed to report on the scheme. Few places are more suitable for the establishment of such gardens than Calcutta—climate, facilities for procuring animals, and an enormous floating population are all in their power. We are glad to learn that several of the native princes have already promised large donations, and that the local and Imperial Governments will give the scheme their support.

DR. SCHOMBURGK'S Report on the Botanic Garden at Adelaide, South Australia, gives an interesting view of the usefulness of such an institution in a new country. Although, according to the director, young Australia has very little taste for the science of botany, yet the number of persons who frequent the gardens for the purpose of getting various kinds of information increases yearly. Part of the report deals with the subject of state conservation of forests. In many districts of the colony the supply of wood for timber and fuel appears to be altogether exhausted, or is soon about to become so. The effect of the *déboisement* on the climate is much dreaded by Dr. Schomburgk, and no doubt, even if the belief in a diminution of the rainfall be not well founded, clearing certainly promotes evaporation, and sooner or later brings about the drying up of springs. Various economic plants have been introduced, including esparto (*Macrochloa tenacissima*). The climate allows of the planting out of many palms in the open air, such as *Latania borbonica*, *Rhapis flabelliformis*, *Sabal Blackburniana*, several species of *Chamærops* and others. There are grand possibilities for a well-managed botanic garden in such a climate.

THE third part of Mr. D. G. Elliott's superb "Monograph of the Paradiseidæ or Birds of Paradise," has just been published, it contains six plates beautifully executed by Mr. Wolf and Mr. Smit.

THE first part of a new biological work has recently been published at Moscow, entitled "Priroda"—popularnoi estestvenno—istoricheski sbornik. It contains a paper by M. Severtzoff on the sheep of Asia, but from being written in Russian, it is beyond the reach of most English readers, and would probably be worthy of translation.

LETTERS received from Mr. R. Swinhoe announce his removal from the Consulship of Ning-po to the more northern Chinese port of Che-fow, on the south shore of the Gulf of Petchelee. M. Swinhoe also announces the despatch of a living specimen of the very interesting hornless deer, *Hydropotes inermis*, first described by him in 1870, for the Zoological Society's Menagerie.

WE understand that Dr. John Anderson, F.Z.S., director of the Indian Museum at Calcutta, will return to England in the autumn for a leave of two years.

WE learn from *Sirius* that the Russian Government has devoted 70,000 roubles to the observation of the Transit of Venus, and is to send out twenty-four expeditions to various parts of the world.

FROM *Sirius* we learn that recently 84 pages of a manuscript of Copernicus have been discovered.

THE Archæological Institute of Great Britain and Ireland will hold its annual meeting at Exeter on July 29 and following days. Lord Devon has consented to fill the office of President.

THE Royal Microscopical Society hold a conversazione in the Large Hall, King's College, on Wednesday evening, May 14.

IN reference to the Natural Science Scholarship at Trinity College, Cambridge, to which, as mentioned last week, Mr. Bridge has just been elected, we are informed that Mr. Alfred Milnes Marshall, of St. John's College, was also highly recommended by the examiners for a second scholarship, but the master and seniors decided that only one should be given. Mr. Bridge, we may add, has for some time past, been a non-collegiate member of the University.

A VERY interesting publication is the "Memoir of the Founding and Progress of the U.S. Naval Observatory," at Washington, prepared by Prof. J. E. Nourse, by order of Rear-Admiral B. F. Sands, the present Superintendent of the observatory. The large pamphlet gives details of the history of the observatory from the first attempt in 1810 to move the American Government to take steps to establish a meridian for America, so as to make that country independent of the meridians of Greenwich and Paris, down to the present time, when by the liberality of the Government and the zeal and knowledge of American astronomers and meteorologists, it has become one of the most efficient observatories in the world. The present observatory was founded in 1842, and the first superintendent was the late Commander M. F. Maury, whose successors have been Capt. J. M. Gilliss, Rear-Admiral C. H. Davis, and Rear-Admiral B. F. Sands. In their attempts to render their observations, astronomical, meteorological, and magnetic, as thorough and wide as possible, the officials have been well backed by the American Government, the result being, as we have said, that the observatory is perhaps the most efficient institution of the kind in the world, both with regard to the higher aims and the practical results of the sciences with which it is connected. Every year, almost every month, as the readers of our "Notes" must have seen, are new ramifications being developed, and new means of greater efficiency being added. For the purpose of circulating accurate time, the observatory is connected with all the telegraphic offices in the United States, and every day at 12 o'clock, the exact time is by this means made known throughout the country. At present, as we noted some time ago, there is being constructed for the observatory by Messrs. Clark, of Cambridgeport, at a cost of 50,000 dollars, a refracting telescope of the largest size; and as we also noted several months since, preparations on the most liberal scale are being made for observing the forthcoming Transit of Venus.

A CORRESPONDENT puts the following case:—A strong man is suddenly struck dead by lightning. What has become of the potential energy he possessed the instant before he was struck? To this we have received the following reply:—His potential energy would be where it was before, viz., within the space bounded by his external surface. What the lightning has done has been to destroy the mechanism for realising that potential energy. A small portion of the man's potential energy might have been converted into actual energy by the lightning, as, for instance, in the shape of heat; but the great bulk would be got by anybody who chose to eat his body.

AN International Monument to the late Commodore Maury has been proposed, and there is no doubt his memory well deserves such a tribute. It has been mooted that an appropriate form in which to embody the monument would be a lighthouse on Rocas, which is sighted by all vessels on the route to Rio de Janeiro.

AT a meeting held in Edinburgh last week, it was resolved to appeal to the public for subscriptions in order to procure the erection in Edinburgh of Mrs. D. O. Hill's statue of Dr. Livingstone. The sum proposed to be raised is 3,500*l*.

THE U.S. signal office has begun the publication of a brief monthly review of the weather, in which special attention is, of course, given to the storms that visit the United States. It appears from these that there were enumerated during the month of January twelve storms, during February ten, and during March eleven. The paths pursued by the centres of these storms are classified as follows:—Twenty-one passed from the Upper Missouri Valley, and possibly from Oregon and British Columbia, eastward, over the lakes to Canada or New England; nine passed from the south-west, north and eastward, to the Middle or Eastern States; three passed from the south-west, eastward, to the South Atlantic States, and thence north-eastward; and two passed up north-eastward some distance off the Atlantic coast. Several of these storms divided into two portions, pursuing separate routes; and, with but one or two exceptions, they all increased in severity as they advanced eastward. The rainfall returns show a general deficiency on the Pacific coast; that, however, which was reported in the States east of the Rocky Mountains in March is probably compensated by the excess during January and February. During the entire three months the temperature has been colder than usual—at least for the country east of the Rocky Mountains.

We have received the programme of the Leeds Naturalist's Field Club for the quarter April to June, from which we see that alternately with "exhibition of specimens and conversation," which takes place once a fortnight, papers on subjects of scientific interest are to be read. Excursions also take place on an average once a fortnight, the first object of the Club being "the minute investigation of the natural history, in all its branches, of the immediate neighbourhood of Leeds, and a more general investigation of the whole of the West Riding." This Society was founded in 1870, and was reorganised on a broader basis in March 1872, and seems to be doing good work.

A correspondent writes, asking information with reference to the etymology of the word *aphis*.

The following additions to the Brighton Aquarium have been made during the past week:—Picked Dogfish (*Acanthias vulgaris*), Larger Spotted Dog-fish (*Scyllium stellare*), Lesser do. (*Scyllium canicula*), Monkfish (*Rhina squatina*), Spotted Rays (*Raja maculata*), Sharp-nosed do. (*Raja lintea*), Streaked Gurnards (*Trigla lineata*), Grey Gurnards (*Triglia gurnardus*), Greater Weevers (*Trachinus draco*), Lesser do. (*Trachinus vipera*), Gemmeous Dragonets (*Callionymus lyra*), Lump Fish (*Cyclopterus lumpus*), Sea Snail (*Liparis vulgaris*), Yarrell's Blenny (*Blenniops ascani*), Sand Smelts (*Atherina presbyter*), Turbot (*Rhombus maximus*), Brill (*Rhombus lævis*), Sail Fluke (*Rhombus punctatus*), Plaice (*Pleuronectes platissa*), Flounders (*Pleuronectes flesus*), Soles (*Solea vulgaris*), Minnows (*Leuciscus phoxinus*), Tench (*Tinca vulgaris*), Masked Crab (*Corystes cassivelaunus*), Tube Worms (*Serpula contortuplicata*), Sea Mice (*Aphrodite aculeata*), Sun Starfish (*Solaster papposa*), Mediterranean Corals (*Balanophyllia verrucari*), Golden Cup Coral (*Balanophyllia regia*), Devonshire Cup Coral (*Caryophyllia smithii*), Sea-fingers (*Alcyonium digitatum*), Sea-anemones (various).

The additions to the Zoological Society's Gardens during the past week include an Indian leopard (*Felis pardus*), two Indian jackals (*Canis aureus*), presented by Capt. Henry; a Malabar Squirrel (*Sciurus maximus*), presented by Mr. Whiteside; three Egyptian cats *Felis chaus* (?) from Cashmere, presented by Capt. J. J. Bradshaw; two Egyptian geese (*Chenalopex ægyptiaca*), presented by Mr. H. W. Thornton; a hawfinch (*Coccothraustes vulgaris*), from the British Isles, presented by the Viscountess Downe; four European Terrapins (*Emys lutaria*) and a green lizard (*Lacerta viridis, var. chloronotus*), presented by Lord A. Russell; two black-handed spider monkeys *Ateles melanochir*); a white-throated Capuchin (*Cebus hypoleucus*); a blue-fronted Amazon (*Chrysotis æstiva*); a yellow-fronted Amazon (*C. ochrocephala*), and an orange-winged Amazon (*C. amazonica*), from Cartagena; a crested agouti (*Dasyprocta cristata*) from Colon; an alligator, and a red and yellow macaw (*Ara chloroptera*), from Baranquilla; a golden eagle (*Aquila chrysaetus*), purchased; a bladder-nosed seal (*Cystophora cristata*), from the North Atlantic, deposited.

ON THE HYPOTHESES WHICH LIE AT THE BASES OF GEOMETRY[*]

III.—*Application to Space.*

§ 1.—By means of these inquiries into the determination of the measure relations of an n-fold extent the conditions may be declared which are necessary and sufficient to determine the metric properties of space, if we assume the independence of line-length from position and expressibility of the line-element as the square root of a quadric differential, that is to say, flatness in the smallest parts.

First, they may be expressed thus: that the curvature at each point is zero in three surface-directions; and thence the metric properties of space are determined if the sum of the angles of a triangle is always equal to two right angles.

Secondly, if we assume with Euclid not merely an existence of lines independent of position, but of bodies also, it follows that the curvature is everywhere constant; and then the sum of the angles is determined in all triangles when it is known in one.

Thirdly, one might, instead of taking the length of lines to be independent of position and direction, assume also an independlence of their length and direction from position. According to this conception changes or differences of position are complex magnitudes expressible in three independent units.

§ 2.—In the course of our previous inquiries, we first distinguished between the relations of extension or partition and the relations of measure, and found that with the same extensive properties, different measure-relations were conceivable; we then investigated the system of simple size-fixings by which the measure-relations of space are completely determined, and of which all propositions about them are a necessary consequence; it remains to discuss the question how, in what degree, and to what extent these assumptions are borne out by experience. In this respect there is a real distinction between mere extensive relations, and measure relations; in so far as in the former, where the possible cases form a discrete manifoldness, the declarations of experience are indeed not quite certain, but still not inaccurate; while in the latter, where the possible cases form a continuous manifoldness, every determination from experience remains always inaccurate: be the probability ever so great that it is nearly exact. This consideration becomes important in the extensions of these empirical determinations beyond the limits of observation to the infinitely great and infinitely small; since the latter may clearly become more inaccurate beyond the limits of observation, but not the former.

In the extension of space-construction to the infinitely great, we must distinguish between *unboundedness* and *infinite extent*, the former belongs to the extent relations, the latter to the measure-relations. That space is an unbounded three-fold manifoldness, is an assumption which is developed by every conception of the outer world; according to which every instant the region of real perception is completed and the possible positions of a sought object are constructed, and which by these applications is for ever confirming itself. The unboundedness of space possesses in this way a greater empirical certainty than any external experience. But its infinite extent by no means follows from this; on the other hand if we assume independence of bodies from position, and therefore ascribe to space constant curvature, it must necessarily be finite provided this curvature has ever so small a positive value. If we prolong all the geodesics starting in a given surface-element, we should obtain an unbounded surface of constant curvature, *i.e.*, a surface which in a *flat* manifoldness of three dimensions would take the form of a sphere, and consequently be finite.

¶ 3. The questions about the infinitely great are for the interpretation of nature useless questions. But this is not the case with the questions about the infinitely small. It is upon the exactness with which we follow phenomena into the infinitely small that our knowledge of their causal relations essentially depends. The progress of recent centuries in the knowledge of mechanics depends almost entirely on the exactness of the construction which has become possible through the invention of the infinitesimal calculus, and through the simple principles discovered by Archimedes, Galileo, and Newton, and used by modern physic. But in the natural sciences which are still in want of simple principles for such constructions, we seek to discover the causal relations by following the phenomena into great minuteness, so far as the microscope permits. Questions about the measure-relations of space in the infinitely small are not therefore superfluous questions.

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[*] (Continued from page 17.)

If we suppose that bodies exist independently of position, the curvature is everywhere constant, and it then results from astronomical measurements that it cannot be different from zero; or at any rate its reciprocal must be an area in comparison with which the range of our telescopes may be neglected. But if this independence of bodies from position does not exist, we cannot draw conclusions from metric relations of the great, to those of the infinitely small; in that case the curvature at each point may have an arbitrary value in three directions, provided that the total curvature of every measurable portion of space does not differ sensibly from zero. Still more complicated relations may exist if we no longer suppose the linear element expressible as the square root of a quadric differential. Now it seems that the empirical notions on which the metrical determinations of space are founded, the notion of a solid body and of a ray of light, cease to be valid for the infinitely small. We are therefore quite at liberty to suppose that the metric relations of space in the infinitely small do not conform to the hypotheses of geometry; and we ought in fact to suppose it, if we can thereby obtain a simpler explanation of phenomena.

The question of the validity of the hypotheses of geometry in the infinitely small is bound up with the question of the ground of the metric relations of space. In this last question, which we may still regard as belonging to the doctrine of space, is found the application of the remark made above; that in a discrete manifoldness, the ground of its metric relations is given in the notion of it, while in a continuous manifoldness, this ground must come from outside. Either therefore the reality which underlies space must form a discrete manifoldness, or we must seek the ground of its metric relations outside it, in binding forces which act upon it.

The answer to these questions can only be got by starting from the conception of phenomena which has hitherto been justified by experience, and which Newton assumed as a foundation, and by making in this conception the successive changes required by facts which it cannot explain. Researches starting from general notions, like the investigation we have just made, can only be useful in preventing this work from being hampered by too narrow views, and progress in knowledge of the interdependence of things from being checked by traditional prejudices.

This leads us into the domain of another science, of physic, into which the object of this work does not allow us to go today.

Synopsis

PLAN of the Inquiry:

I. Notion of an n-ply extended magnitude.
  § 1. Continuous and discrete manifoldnesses. Defined parts of a manifoldness are called Quanta. Division of the theory of continuous magnitude into the theories
    (1) Of mere region-relations, in which an independence of magnitudes from position is not assumed;
    (2) Of size-relations, in which such an independence must be assumed.
  § 2. Construction of the notion of a one-fold, two-fold, n-fold extended magnitude.
  § 3. Reduction of place-fixing in a given manifoldness to quantity-fixings. True character of an n-fold extended magnitude.
II. Measure-relations of which a manifoldness of n dimensions is capable on the assumption that lines have a length independent of position, and consequently that every line may be measured by every other.
  § 1. Expression for the line-element. Manifoldnesses to be called Flat in which the line-element is expressible as the square-root of a sum of squares of complete differentials.
  § 2. Investigation of the manifoldness of n-dimensions in which the line-element may be represented as the square root of a quadric differential. Measure of its deviation from flatness (curvature) at a given point in a given surface-direction. For the determination of its measure-relations it is allowable and sufficient that the curvature be arbitrarily given at every point in $n\frac{n-1}{2}$ surface directions.
  § 3. Geometric illustration.
  § 4. Flat manifoldnesses (in which the curvature is everywhere = 0) may be treated as a special case of manifoldnesses with constant curvature. These can also be defined as admitting an independence of n-fold extents in them from position (possibility of motion without stretching).
  § 5. Surfaces with constant curvature.
III. Application to Space.
  § 1. System of facts which suffice to determine the measure-relations of space assumed in geometry.
  § 2. How far is the validity of these empirical determinations probable beyond the limits of observation towards the infinitely great?
  § 3. How far towards the infinitely small? Connection of this question with the interpretation of nature.

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*THE DEVELOPMENT THEORY IN GERMANY**

III.

Chorology: or, the Geographical Distribution of Living Beings

THE importance of the theory of Evolution does not consist in its accounting for this or that particular fact, but in its explaining all biological facts collectively. It is found to be confirmed in every detail by the mode of distribution of the various organisms on the surface of the earth. This distribution had already been studied by Alexander von Humboldt and Fr. Schouw for plants, by Berghaus and Schmarda for animals. But previous to Darwin and Wallace, this study had produced only a collection of unsystematised facts; Haeckel has attempted to create out of it a special science under the name of *Chorology*.

With the exception of the monocellular protozoa, which, on account of their simplicity, have been able to appear at the same time or at several times in different places; with the exception also of species which owe their origin to a hybrid or bastard generation, and which it has been possible to reproduce in different circumstances wherever the parent species have previously spread, it must be admitted that each of the other species has only been originated a single time and in a single place. But, once produced, they must, as a consequence of the struggle for existence, and in virtue of the laws of population, or rather of excess of population, tend to spread to the widest possible extent. Animals and plants migrate as well as man, both actively and passively.

In the case of animals, which have, more than plants, freedom of movement, active migration plays the principal part. The more easy locomotion is in the case of any species, the more rapidly is the species bound to spread. This is why birds and insects, furnished with wings, although referable to a less number of orders or natural groups than other animals, yet present a very great diversity of species slightly distinguishable from one another; this is to be ascribed to the fact that the facility with which they can move from place to place has subjected them to the modifying influences of the most varied localities. After birds and insects the swiftest runners among the denizens of the land, the best swimmers among the inhabitants of the water have been subject to the widest extension. With regard to animals which are fixed or immovable while being developed, corals, tubicolæ, tunicata, crinoids, &c., they usually enjoy during their youth so much of the power of movement as admits of their displacement. A great number of floating plants are also transported to great distances by water.

But the spread of a large number of plants and of certain animals can be explained only by a passive migration. The wind sweeps to great distances, sometimes over seas, eggs of small animals, seeds, and sometimes even minute organisms; this explains the well-known phenomena of showers of frogs. These eggs, these seeds, these small organisms, sometimes fall into the water, which transports them to still greater distances. Trunks of trees, which traverse the ocean under the direction of the currents, and those which the tempest hurls from the mountain tops, can carry with them, hidden in their interstices, in the moss or the parasitical plants with which they are covered, in the earth which adheres to their roots, innumerable germs to be developed in new regions. The icebergs of the polar sea have landed foxes and bears even on the shores of Iceland and Britain. Birds, insects, mammals which are removed, carry with them thousands of parasites, microscopic beings, eggs or germs. Man himself carries them about more abundantly still along with the varied materials he employs for his works and his industry.

The fact of the distribution of certain species which cannot be explained by migration, either active or passive, may be accounted for by geological facts. In consequence of the imperceptible but unceasing change of the level of the seas, in consequence of the phenomena of subsidence and elevation of the land, lands at one time united have been divided, watercourses which communicated have been separated, thus accounting for the fact that fishes of the same species are found in different rivers, that islands are tenanted by the same mammals as the continents. England has been united to Europe at two different times; at a certain epoch our continent must have been united by land to N. America. The South-sea Islands are the remains of what was at one time a single land; so in the Indian Ocean land has at one time stretched along the South of Asia from Sunda to Africa; this great continent which Sclater has called *Lemuria*, on account of the apes which were peculiar to it, is probably the cradle where the human race was developed from the anthropoid apes. Mr. Wallace has proved that the Malay Archipelago consisted of two entirely different parts: one, comprehending Borneo, Java, and Sumatra, was united to Asia by the peninsula of Malacca, while the other, comprehending the Celebes, the Moluccas, New Guinea, the Salomon Isles, &c., was immediately attached to Australia.

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* Continued from vol. vii. p. 434.

Another cause which has favoured the dispersion of species all over the globe, was the uniformity of temperature which prevailed up to the tertiary geological period. Previous to the freezing of the polar regions, species found everywhere a climate equally warm and agreeable, favourable to migrations in all directions; since that period, on the contrary, a new difficulty of existence has arisen,—organisms have to undergo acclimatisation; those which have the power of adapting themselves to the lower temperature of regions at a distance from the equator, have been transformed by selection into new species; while those which have found such adaptation impossible, have been compelled, under pain of extinction, to remove to more favourable climates. When, at a later period, occurred that strange phenomenon—of which, as yet, no satisfactory explanation has been given—known as the Glacial Period, animals and plants were compelled to migrate anew; the living population of the earth, condensing itself between the tropics, a terrible struggle for existence took place between the old inhabitants of these regions and those that fled thither for refuge; many species were bound to disappear, while many new ones were originated. There is still another chorological phenomenon which is to be accounted for by the glacial period, viz., the resemblance of many of the inhabitants of mountains to those of the Polar regions; as those animals and those plants are not found in the intermediate countries, it is absolutely necessary to suppose a migration which, considering the habits of these creatures, could only have taken place at the glacial epoch. It is probable that at this period the gentians, the saxifrages, the Polar hare and fox, inhabited the central part of Europe; but as the temperature rose, some of these creatures retired towards the north, while the remainder found a refuge upon the summits of the European mountains.

When plants or animals migrate to new regions, they are subjected to new conditions of existence to which they must adapt themselves. The new climate, new food, relations with new organisms, all this obliges the emigrants to submit to modifications under pain of annihilation, and, as a consequence, to form new varieties or new species; it is in these circumstances, in fact, that natural selection acts with the greatest intensity. In ordinary circumstances, individuals which have changed breed with individuals who have not changed, and the products of such crossings have a tendency to revert to the primitive type; but when a migration has taken place, when modified individuals are separated from the others by mountains or by seas, they can no longer interbreed, and this isolation insures the preservation of the newly acquired forms. It is of course evident that these considerations apply only to species in which the sexes are separate.

There still remain three other chorological phenomena which furnish an important proof of the truth of the evolution theory. There is first the likeness of form, the family resemblance which exists among the local species characteristic of each region, and the extinct and fossil species of the same region; in the second, the no less striking family resemblance which exists among the inhabitants of certain groups of those of the neighbouring continents, whence the population of these islands must have come; and lastly, the special character presented by the collective fauna and flora of the islands. All the facts adduced by Darwin, Wallace,* and Moritz Wagner,† as well as all those other facts which geographical and topographical dispersion of organisms present to us are simply and completely explained by the theory of selection and migration, while it would be impossible to explain them without it.

Palæontology

Thanks to the theory of evolution, the natural classification of animals and plants, which was previously only a record of names for arranging the different forms in an artificial order, or a record of facts expressing summarily the degree of resemblance among them, tends to become the genealogical tree of organisms. In order to construct it the student has only to combine the data furnished by the three parallel developments referred to above—the palæontological development, the embryological development, and the systematic development in the order of perfection or of comparative anatomy. The writer in the *Revue Scientifique* here gives a table presenting a view of the geological and palæontological doctrines of Haeckel. Between the stages generally admitted by geologists, Haeckel intercalates others which he calls inferior or intermediate stages in relation to the superior stages. Haeckel accepts completely the system of gradual and continuous evolution as propounded by Lyell, and rejects the system of sudden catastrophes which has been advocated by Cuvier and his disciples. He places the probable appearance of man in the Miocene, and his certain existence in the Pliocene. Many attempts have been made to determine approximately how many thousand years each geological period has lasted; these conjectures are principally framed on the relative thickness of the different beds. The total thickness of the Archæolithic or Primordial beds, in which Haeckel includes the Laurentian, Cambrian, and Silurian, is 70,000 ft.; that of the Primary, from the Devonian to the Permian, 42,000 ft.; that of the Secondary, 15,000 ft.; that of the Tertiary, 3,000 ft.; while the thickness of the beds of the "Anthropolithic" or Quaternary age is only from 500 to 700 ft. From these figures, the following relative duration of the successive ages may be deduced:—

Primordial Age	53·6
Primary „	32·1
Secondary „	11·5
Tertiary „	2·5
Quaternary „	0·5

Thus the Primordial age has existed longer than the other four put together. As to the number of centuries or of millenniums necessary for the deposition of one bed only one foot thick, that depends on circumstances so variable that it is impossible to give any measure: It is longer in the depths of mid-ocean, in the beds of very long rivers, in lakes which receive no affluents; it is shorter on the sea-margins, at the months of great rivers whose course is long and straight, in lakes which receive many tributary streams. It results from such considerations that every estimate of the duration of a geological epoch must be relative.

It will be necessary, moreover, to take into consideration, elevations and depressions of the ground, which, according to Haeckel, will be alternative, and will correspond to the mineralogical and palæontological differences which exist between two systems of beds and between two formations of these systems. When a certain region, after having remained for many thousand centuries beneath the water, emerges for a certain time, and is again submerged, it will be readily admitted that the bed which is deposited after such an interval ought to present characteristics different from those of the lower bed: for time is bound to accomplish change of all organic and inorganic conditions. This theory has been disputed by Huxley, who finds it inconsistent with the existence of a large number of beds, in which are found united organic forms, holding a middle place between those of adjacent formations; the English naturalist adduces, for example, the beds of Saint Cassian, in which are found mingled the forms of the primary and secondary formations.

It is certain that even yet our knowledge of palæontology is very imperfect, and far from enabling us to write, with anything like exactness, the history of the production of organic species. We know with what difficulties this study is surrounded. The fossil remains of the most remote ages appear to have been destroyed by the great heat of the lower beds in which they were deposited. *Eozoon Canadense* is the only fossil which has hitherto been found in the formations of the Laurentian period; while the beds of carbon and of crystallised lime (graphite and marble) give us the assurance that in them have existed animal and vegetable petrifactions. Another difficulty lies in the fact that hitherto the field of geological exploration has been very restricted. Outside of England, Germany, and France, very few formations have been seriously studied; almost the only successful explorations have been in railway-cuttings. One indication of what may be discovered elsewhere is furnished by the remarkable petrifactions which have resulted from some researches prosecuted in Africa and Asia, in the neighbourhood of the Cape, and on the Himalayas: forms have been discovered which fill up important gaps in palæontological classification. It must be remembered also that only the hard and solid parts of organisms have been preserved, that entire forms, such as the Medusæ, shell-less molluscs, many articulata, nearly all worms, could leave no trace behind. The most important parts of plants, the flowers, have completely disappeared. Moreover, terrestrial organisms have been petrified only in accidental instances, where they have fallen into the water and been covered with mud; it is not to be wondered at then if the number of fossils of this kind is relatively much less considerable than that of those kinds which have inhabited the sea or fresh water. This explains also the apparently strange fact that of many fossil mammals, especially those of the secondary, we recognise only the lower jaw. This arises from the fact that that bone is easily separated from the dead body; while the rest swims on the surface of the water and is carried to the bank, the jaw falls to the bottom, and is buried in the mud, where it is petrified. The traces of those which have been found in different beds of sandstone, and especially in the red sandstone of Connecticut, belong to organisms whose bodies are entirely unknown to us, and prove that we are far from possessing remains of all actual forms. What gives us reason to think that an immense number must remain unknown is the fact that of those whose fossil remains we possess, only one or two examples have come to light. It is only ten years since a bird of the highest importance was discovered in the Jura; till then no intermediate form was known between the birds proper and reptiles, which are, nevertheless, the class most closely related to the former. Now this fossil bird, which possesses the tail, not of an ordinary bird, but of a lizard, confirms the hypothesis that birds are descended from the saurians. A couple of small teeth which have been found in the Keuper of the Trias are, up to the present, the only proof that mammals have existed from the Triassic period, and that they did not appear only in the Jurassic period, as was previously believed.

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* "Malay Archipelago."

† The "Darwinian Theory and the Law of Migration of Organisms" Leipzig, 1868).

Fortunately we are able to supplement the insufficient data of palæontology by those of embryology, since individual development is, as it were, a reproduction or recapitulation brief and rapid, by means of heredity and adaptation of the development of species. Embryology is especially valuable for the light which it throws on the more ancient forms of the primordial period; by it alone do we learn that these primitive forms must have been simple cells, similar to eggs; that these cells, by their segmentation, their conformation, and their division of labour, have given birth to the infinite variety of the most complicated organisms.

To the valuable data respecting the relations of organisms furnished by palæontology and embryology must be added those derived from comparative anatomy. When organisms, whose exterior is very different, resemble each other in their interior construction, we may conclude with certainty that this resemblance is due to heredity, while the differences are a result of adaptation. If, for example, we compare the limbs or extremities of different mammifers, the arm of man, the wing of the bat, the anterior members of the mole adapted for digging, those of other mammifers made for leaping, climbing, or running; if we consider, besides, that in all these members variously formed, the same bones are found, equal in number, in the same place, disposed in the same manner, are we not forced to admit the close relationship of organisms? This homology can be explained only by heredity, by descent from common ancestors. And to go still further, if we find in the wing of the bird, in the anterior members of reptiles and amphibia, the same bones as in the arms of man, or in the anterior limbs of other mammifers, can we not affirm with certainty the common descent of all these vertebrate animals?

SCIENTIFIC SERIALS

Ocean Highways, May.—The first paper in this number is an article on Mexico, by Mr. Maurice Kingsley, accompanied by a map showing the course of the Vera Cruz and Mexico Railway. This is followed by a very interesting article on "Railway Communication between London and Calcutta," with a map showing the proposed line from Ostende, by Vienna, Constantinople, Diabeker, Herat, Cabul, Lahore, Delhi, Cawnpore, and Calcutta. By this route the land journey would amount to 6,336 miles, with only 73 miles of sea, which could be accomplished in 214 hours, or about 9 days; while by the present shortest route, the sea-journey amounts to 3,941 miles, and the time taken is 492 hours, or upwards of 20 days. Dr. Robert Brown contributes a paper entitled "A Cruise with the Whalers in Baffin's Bay," which is followed by "Notes on Mr. Stanley's Work," by Capt. R. F. Burton, in which that gentleman points out several things in Stanley's book that he thinks are capable of amendment. Burton thinks Stanley "wants only study and discipline, to make him a first-rate traveller." This is followed by a very valuable paper on "The Steppes to the North of Bokhara," by A. Vámbéry. Then follow the usual reviews, notes, reports of societies, &c.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, May 1.—Dr. Odling, F.R.S., president, in the chair.—Dr. H. Sprengel, "On a new class of explosives," gave an account of some new explosives, consisting of two liquids inexplosive by themselves, but which when mixed and fired with a detonating charge are as effective as nitroglycerine.—Prof. Abel of the Royal Arsenal, Woolwich, drew attention to the great difference produced by variations in the mechanical state of the explosive.—On Zirconia, by Mr. J. B. Hannay.—On Pyrogallate of lead and lead salts, by Mr. W. H. Deering.

Royal Horticultural Society, April 16.—General meeting, Sir Coutts Lindsay, Bart., in the chair. The Rev. M. J. Berkeley commented on the plants exhibited, and remarked that the unused archways of railways might be profitably employed for the production of mushrooms.—Mr. W. A. Lindsay (the secretary) enumerated the concessions which the Council had made for this year to Her Majesty's commissioners for the Exhibition, including a passage-way across the gardens: the society would receive in return the sum of 1000*l*.—Scientific committee—Prof. Westwood, F.L.S., in the chair. The Rev. M. J. Berkeley commented on an article in the recent number of the journal of the Royal Agricultural Society on the injury suffered by horses fed upon mouldy oats. There was an evident error with respect to the fungus figured as *Aspergillum* (sic) which was clearly the common bread-mould *Ascophora Mucedo*. With respect to the diseased coffee-plants from Natal brought forward at the last meeting he was disposed to think that climatic conditions were the cause, of their malady. The differences between the summer and winter temperatures had been too slight to check the growth of the coffee trees. There are often three flowerings instead of one, or at all events two. It seemed on the whole probable that growth was over-estimated, and that, consequently, when the drought came, the plants were unable to support it. There was a minute immature black fungus, which might be referred to *Depazea*, on the twigs. Prof. Thiselton Dyer read a letter addressed to Dr. Hooker from Dr. Henderson in charge of the Calcutta Botanic Garden, describing the disease of the opium poppy. This appeared to be favoured by moist weather, and the plants affected were infested with *Peronospora arborescens*, and also with a fungus (which Mr. Berkeley identified as *Macrosporium cheiranthi*, a peculiar form of *Cladosporium herbarum*) The places attacked were black, and the disease progressed from below, upwards. If the plant has not flowered when attacked, it never does so; but if it is on the point of flowering, the sepals, petals, and stamens, do not drop off as they would do in healthy plants. The effect of guano, even in very small quantities, was remarkable in increasing the crop.

Institution of Civil Engineers, April 29.—Mr. T. Hawksley, president, in the chair.—"On the Rigi Railway," by Dr. William Pole, F.R.S., M. Inst. C.E. The object of this railway was to convey passengers to the top of the Rigi, a mountain near Lucerne, from which there was a view so celebrated as to attract large numbers of visitors in the summer months. The line commenced at Vitznau, on the Lake of Lucerne, and was about four miles long. The works are mostly formed by cutting and benching on the rocky slope of the mountain. There was but one short tunnel, and only one iron bridge over a ravine. The gauge was 4 feet 8½ inches.

GLASGOW

Geological Society, April 10.—Mr. John Young, vice-president, in the chair.—The chairman exhibited a specimen of carboniferous limestone from Braidwood, near Carluke, containing in great abundance the tests or shells of a species of Foraminifer, *Saccamina carteri*. Similar organisms had been found in a limestone from the Elf Hills, Northumberland, and described by Dr. H. B. Brady in 1871. They had also been found once or twice in the limestones of the east of Scotland, but so far as he was aware, this was the first instance in which it had been recognised in the limestones of the Lanarkshire coal field.—Mr. J. Thompson, F.G.S., read a paper which he had prepared in conjunction with Mr. Henry Caunter, on the geology of the neighbourhood of Stornoway, island of Lewis. The authors briefly described the relations of the gneissic or Laurentian rocks to the Cambrian strata of the island. The junction of the two formations is seen in the bed of a small stream that flows into the sea in the harbour of Stornoway; also in Garabost Bay, about seven miles to the east. The Laurentians dip N.W., while the lower members of the Cambrian dip at an angle of 23° to the N.E. These beds have been termed by Sir R. Murchison, Upper Cambrian. The authors next described the more recent deposits of the island, beginning with the boulder drift, with its transported striated erratics, all of which belong to the Laurentian system, and are traceable to the west and north-west. They then referred to the gravels and drift-sand which overlie the remains of an extensive bed of peat seen in Stornoway Bay, where it attains a depth of 15 feet. At the lower extremity of this bed, and only seen at extreme low tides, are numerous stumps of trees of considerable dimensions, the roots of which rest upon and pass down through a bed of clay which forms the subsoil. From this it would seem that there has been an extensive subsidence of the island at a comparatively recent period, and that the climatal conditions must have been very different during the time when such trees grew from those which prevail at the present day.

PARIS

Academy of Sciences, April 28.—M. de Quatrefages, president, in the chair.—The following papers were read:—On the actions produced in capillary spaces by molecular attractions, by M. Becquerel. The author described the various results produced by inserting solutions contained in cracked vessels into other vessels containing solutions capable of producing precipitates in them, *e.g.* baric nitrate and potassic sulphate. After a few days the solutions communicate by the crack and electric currents are started.—On the heat disengaged by the reactions between the alkalis and water: potassic and sodic hydrates by M. Berthelot. The results obtained lead the author to suppose that there is a potassic hydrate intermediate between the ordinary fused and crystallised hydrates.—On the combinations produced by the electric discharge between marsh gas and carbonic anhydride, and between carbonic oxide and hydrogen, by MM. P. and A. Thenard.—On certain particular spectroscopic observations by Father A. Secchi.—On the application of the pandynamometer to the measurement of the work performed by a steam engine, by M. G. A. Hirn.—On the application of the mathematical theory of elasticity to the study of articulated systems formed by elastic rods, by M. Maurice Levy.—On the composition of the thermic mineral waters of Vichy, Bourbon l'Archambault, and Neris, as regards those substances which invariably exist in water in minute proportions, by M. de Gouvernain.—An examination of the difference produced in the spectrum of chlorophyll by different solvents, by M. J. Chautard.—On the unwholesome nature of the Versailles water supply, by M. E. Decaisne.—On the awakening of the Phylloxera in the month of April 1873, by M. Faucon.—On nebulæ discovered and observed at the Marseilles observatory, by M. E. Stephan.—On characteristics in the theory of conics, on planes, and in space, and on second order surfaces, by M. Halphen.—On the vapour emitted at the same temperature by the same body in two states, by M. J. Moutier.—On the spectrum of erbia, by M. Lecocq de Boisbaudran. The author has found that erbia and erbic phosphate give, when heated, different band spectra, of which the author exhibited plates and tables. These spectra he has carefully investigated, and finding it impossible to attribute either of them to another body, he concluded that they were both due to erbium in different states of combination.—Observations on M. du Moncel's late note on the history of the silent discharge, by M. Arn. Thenard.—On the Manufacture of ammonic sulphate from nitrogenous waste products, by M. L'Hote.—On the conditions of formation of extra silicious pig in blast furnaces, by M. S. Jordan. Experiments on the effects of dynamite, by MM. Roux and Jarrou.—On necrobiosis and gangrene, an experimental study on the phenomena of mortification and putrefaction as they occur in the living body, by M. Chauveau.—On the geology of Mount Léberon, by M. A. Gaudry.

DIARY

THURSDAY, May 8.

Royal Society, at 8.30.—Contributions to the Study of the Errant Annelides of the older Palæozoic Rocks: Prof. Alleyne Nicholson.—Researches in Spectrum Analysis in Connection with the Spectrum of the Sun: J. Norman Lockyer.—The Action of Light on the Electrical Resistance of Selenium: Lieut. Sale.

Mathematical Society, at 8.—On Bicursal Curves, and Plan of a Curvo-tracing Apparatus: Prof. Cayley.—On an application of the Theory of Unicursal Curves: M. Hermite.

Society of Antiquaries, at 8.30.

Royal Institution, at 3.—Light: Prof. Tyndall.

FRIDAY, May 9.

Royal Institution, at 3.—A Fortnight in Asia Minor: Mr. Grant Duff, M.P.

Astronomical Society, at 8.

Quekett Club, at 8.

SATURDAY, May 10.

Royal Institution, at 3.—Ozone: Prof. Odling.

MONDAY, May 12.

Royal Geographical Society, at 8.30.

London Institution, at 4.—Elementary Botany: Prof. Bentley.

TUESDAY, May 13.

Royal Institution, at 3.—Roman History and Architecture: J. H. Parker.

Photographic Society, at 8.—On Instantaneous Landscape Photography: F. R. Elwell.—Improvements in Carbon Printing: A. Marion.

WEDNESDAY, May 14.

Society of Arts, at 8.—Improvements in Rifles: Capt. O'Hea.

Geological Society, at 8.—Notes on Structure in the Chalk of the Yorkshire Wolds: J. R. Mortimer.—On the genus *Palæocoryne*, Duncan and Jenkins, and its affinities: Prof. P. Martin Duncan.—On *Platysiagum sclerocephalum* and *Palæospinax priscus*, Egerton: Sir Philip de M. Grey-Egerton.—On a new genus of Silurian Asteridæ: Dr. Thomas Wright.

Archæological Association, at 8.—Anniversary.

London Institution, at 7.—Paper and Discussion.

Society of Telegraph Engineers, at 7.30.—On the Block System of Working Railways: W. H. Preece and Capt. Mallock.

THURSDAY, May 15.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Chemical Society, at 8.—On Isomerism: Dr. H. E. Armstrong.

Numismatic Society, at 7.

Royal Institution, at 3.—Light: Prof. Tyndall.

BOOKS RECEIVED

English.—Comet's Tails no longer a Mystery: T. A. R. (Reeves and Son).—Manual for Medical Officers of Health: E. Smith (Knight & Co.).—Manchester Science Lectures, 1871–73. 3rd Series: J. Heywood & Co.—Tropical World. New Edition: Dr. G. Hartwig (Longmans).—The Life of von Humboldt, Vols. 1 and II: Bruhns, translated by Lassell (Longmans).—Astronomical Plates from the Observatory of Harvard College (Trübner).—Text-books of Science; Electricity, and Magnetism: F. Jenkin (Longmans).—Critiques and Addresses: Thomas Huxley (Macmillan & Co.)—The Familiar History of British Fishes: Frank Buckland (published by the Society for promoting Christian Knowledge).—The Cruise of the *Curacoa* among the South Sea Islands, 1865: J. C. Brenchley (Longmans).

Foreign.—Zeitschrift für Biologie, Part 1, Vol. ix.—Zoologische Botanische Gesellschaft in Wien, Vol. xxii., 1872.—Die Naturkräfte, Munich. Edited by Dr. K. A. Zittel.

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THURSDAY, MAY 15, 1873

A VOICE FROM CAMBRIDGE

II.

THE questions raised by the Cambridge Memorial to which we referred last week are so important that no excuse is necessary for recurring to them. In the first place it may be remarked that the answer of Mr. Gladstone to the Cambridge memorialists, is quite such as any reasonable man might have looked for. University reform is not at present a political question in the vulgar sense of that word. The heart of the masses is not stirred by proposals concerning the tenure of fellowships. The religious element, or rather the sectarian element, has now been largely eliminated from the matter; there remains scarcely anything at stake save the interests of learning and science; and these, as we know, are things of very little value in the eyes of the present Government.

The more one looks at the matter the more it is difficult to see what good the Cambridge Reformers expected to result from their respectable document. No fault can be found with the propositions of the memorial so far as they go. They are just such sound steady-going sober proposals as would naturally come from a body of quiet moderate officials who, on the whole, content with the general state of things, desired to see some practical amendments introduced, but dreaded to agitate, had a wholesome fear of radical changes, and above all, were not clear about the broad features of the necessities which have to be met, or of the changes which have to be brought about.

Until the public mind, to say nothing of the University mind, has gained some clear definite notions about the functions of a University, all attempts at reform must be partial or complete failures.

The prevalent theories concerning the office of a University may be put in three categories.

The first regards the University as an ecclesiastical nursery. This was the original view, but now-a-days is passing out of mind, though tenaciously clung to by some resident members at either University. It only needs to be mentioned to be dismissed.

The second looks upon Oxford and Cambridge as places where the young Tartars of modern English society are covered with a varnish of "culture," and polished into gentlemen. Dr. Lyon Playfair said in the House the other day that the Scotch University taught a man how to make a thousand a year, the English University how to spend it; and in saying this he simply put into forcible language the ideas which are prevalent among many members of the Universities. They distinctly and emphatically discard the idea that it is the duty of the University to equip a man for the struggle for a livelihood, to train him for business, for the arts, for the professions. Their token is "culture," not culture in the sense of higher learning, but in the sense of personal varnish, in the sense of a mental equipment which does not pay, and which is of no use to the owner in practical life, which is a luxury and not a need, a sort of evening dress of the mind, which may be ornamental under the artificial lights of society, but is ill suited for every-day work. Now this sort of culture is not much sought after; for by hard-headed fathers whose sons have to get or to keep their living by their own exertions, it is sought for less and less year by year. The advocates of the view we are dealing with see this very clearly, and accordingly they contend, very logically, that since the world does not care greatly for this kind of culture, and will not send its sons to a University for that only, some other inducements must be provided. And these are found in the prize fellowships, more especially in the non-resident fellowships. A lad of parts whose friends would not send him to Oxford simply to gain that liberal education, "which softens the character and prevents its being strong," goes there because by show of possessing that culture which he despises or even hates, he gains a good round sum of money which it is worth his while to waste three or four years in getting.

The third view, which at present has but few advocates, teaches that the University is a place where anyone and everyone may be trained for any and every respectable path of life, and where at the same time all the interests of higher learning and science are cared for. The advocates of this view say, Do not bribe men by fellowships to come to a University from which they will go carrying with them a very little learning, and that for the most part useless, and an artificial culture of doubtful value. Make it worth their while to come to the University, teach them there what they want to be taught, train them there as they desire to be trained, and there will be no need to bribe them with fellowships. They will then come to Oxford and to Cambridge as they are now going to Owens College, to London, to Newcastle, and to Germany. Take care at the same time that the teaching be not narrow and professional, broaden it with the diligent nurture of higher learning and science, and then there will be every hope of seeing true culture and useful education going hand in hand. Let the youth of the University have the opportunity of seeing the master-minds of the age at their work, so that they may be inspired by them to the highest reaches of thought.

It appears to us that many of those who signed the Cambridge memorial had no clear ideas as to which of the above views they adhered; and hence the uncertain sound of their trumpet. Apparently the document was so loose that supporters of all three views signed it conscientiously; no wonder it fell without effect.

It is unnecessary for us to say that the third view we have mentioned is one which we ourselves support. The real difficulty lies in this, how to change the old Universities to suit these new views, how to ring out the old ecclesiasticism and false culture and ring in useful training with high science and deep active learning and research. The difficulty of this task cannot be exaggerated. Long years of misrule have left suckers of jobbery, like bindweed in an old garden, which come up refreshed with every stirring of the soil. There is a mass of powerful conservatism which has to be striven against. There is a careless public and a still more careless Government which has to be roused. There are plenty of difficulties in the way. If the memorialists really have the reform of the old universities at heart, they will cease to memorialise feebly a feeble administration, and search diligently for some broad scheme of reform which may be introduced without danger, which will render all fellowships unnecessary, which will at once provide for the professional student and the original investigator, and that in such a way that an ignorant Parliament shall have no excuse for tampering with it. And if they do this quickly, they may do it before the Association for Academical Organisation has begun to stretch its limbs.

LONGMANS' TEXT-BOOKS OF SCIENCE

Electricity and Magnetism. By Fleeming Jenkin, F.R.SS. L. and E., M.I.C.E., Professor of Engineering in the University of Edinburgh. (London: Longmans and Co., 1873.)

THE author of this text-book tells us with great truth that at the present time there are two sciences of electricity—one that of the lecture-room and the popular treatise; the other that of the testing-office and the engineer's specification. The first deals with sparks and shocks which are seen and felt, the other with currents and resistances to be measured and calculated. The popularity of the one science depends on human curiosity; the diffusion of the other is a result of the demand for electricians as telegraph engineers.

The text-book before us, which is the work of an engineer eminent in telegraphy, is designed to teach the practical science of electricity and magnetism, by setting before the student as early as possible the measurable quantities of the science, and giving him complete instructions for actually measuring them.

> "The difference between the electricity of the schools and of the testing office has been mainly brought about by the absolute necessity in practice for definite measurement. The lecturer is content to say, under such and such circumstances, a current flows or a resistance is increased. The practical electrician must know how much resistance, or he knows nothing; the difference is analogous to that between quantitative and qualitative analysis."

It is not without great effort that a science can pass out of one stage of its existence into another. To abandon one hypothesis in order to embrace another is comparatively easy, but to surrender our belief in a mysterious agent, making itself visible in brilliant experiments, and probably capable of accounting for whatever cannot be otherwise explained; and to accept the notion of electricity as a measurable commodity, which may be supplied at a potential of so many Volts at so much a Farad, is a transformation not to be effected without a pang.

It is true that in the last century Henry Cavendish led the way in the science of electrical measurement, and Coulomb invented experimental methods of great precision. But these were men whose scientific ardour far surpassed that of ordinary mortals, and for a long time their results remained dormant on the shelves of libraries. Then came Poisson and the mathematicians, who raised the science of electricity to a height of analytical splendour, where it was even more inaccessible than before to the uninitiated.

And now that electrical knowledge has acquired a commercial value, and must be supplied to the telegraphic world in whatever form it can be obtained, we are perhaps in some danger of forgetting the debt we owe to those mathematicians who, from the mass of their uninterpretable symbolical expressions, picked out such terms as "potential," "electromotive force" and "capacity," representing qualities which we now know to be capable of direct measurement, and which we are beginning to be able to explain to persons not trained in high mathematics.

Prof. Jenkin has, we think, made great progress in the important work of reducing the cardinal conceptions of electromagnetism to their most intelligible form; and presenting them to the student in their true connection.

The distinction between free electricity and latent, bound, combined, or dissimulated electricity, which occurs so frequently, especially in continental works on electricity, is not, so far as we can see, even alluded to in these pages; so that the student who takes Prof. Jenkin as his sole guide will not have his mind infected with a set of notions which did much harm in their day. On the other hand, terms which are really scientific—the use of which has led to a clearer understanding of the subject—are carefully defined and rendered familiar by well-chosen illustrations.

Thus we find that men of the most profound scientific acquirements were labouring forty years ago to discover the relation between the nature of a wire and the strength of the current induced in it. By the introduction of the term "electromotive force" to denote that which produces or tends to produce a current, the phenomena can now be explained to the mere beginner by saying that the electromotive force is determined by the alterations of the state of the circuit in the field, and is independent of the nature of the wire, while the current produced is measured by the electromotive force divided by the resistance of the circuit. To impress on the mind of the student terms which lead him in the right track, and to keep out of his sight those which have only led our predecessors, if not ourselves, astray, is an aim which Prof. Jenkin seems to have kept always in view.

To the critical student of text-books in general, there may appear to be a certain want of order and method in the first part of this treatise, the different facts being all thrown into the student's mind at once, to be defined and arranged in the chapters which follow. But when we consider the multiplicity of the connexions among the parts of electrical science, and the supreme importance of never losing sight of electrical science as a whole, while engaged in the study of each of its branches, we shall see that this little book, though it may appear at first a mighty maze, is not without a plan, and though it may be difficult to determine in which chapter we are to look for any particular statement, we have an excellent index at the end to which we may refer.

The descriptions of scientific and telegraphic instruments have all the completeness and more than the conciseness which we should look for from a practical engineer, and in a small compass contain a great deal not to be found in other books. The preface contains an outline of the whole subject, traced in a style so vigorous, that we feel convinced that the author could, with a little pains bestowed here and there, increase the force of his reasoning by several "Volts," and at the same time diminish by an "Ohm" or two the apparent stiffness of some of the paragraphs, so as to render the book more suitable to the capacities of the "Microfarads" of the present day.

ZOOLOGICAL MYTHOLOGY

Zoological Mythology; or, the Legends of Animals. By Angelo De Gubernatis, Professor of Sanskrit and Comparative Literature in the Istituto di Studii Superiori e di Perfezionamento at Florence. 2 vols. (London: Trübner and Co., 1872.)

THE claims which these volumes make to our consideration as students of Nature is that their stories of birds, beasts, and fishes are treated as being Natural History, not indeed in an ordinary, but in an extraordinary sense. It is asserted that they are descriptions in mythical language of the great phenomena of the earth and sky. To no small extent this assertion is indisputably true. In ancient poetry or story, it often happens that the teller of a myth incidentally lets us know what his underlying meaning is. Thus many a passage from the Veda shows that the minds of that poetic race of herdsmen, the ancient Aryans, were so moulded to the dominant ideas of the pasture and the stall, that they saw throughout all heaven and earth the analogues of their beloved herds. The winds chasing the clouds seem, to their fancy, bulls rushing among the cows. The sky is a beneficent cow, giving rain for milk. Indra, the Heaven-god, is a bull of bulls, whose horns are the thunderbolts, who smites in storm the mountain cavern where the cloud-cows are imprisoned, and sets them free. The sun may be fancied a herdsman, as in this ancient Vedic riddle: "I have seen a shepherd who never set down his foot, and yet went and disappeared on the roads; and who, taking the same and yet different roads, goes round and round amidst the worlds." Horses, too, as we moderns know by the classic chariot of the sun, figure in mythic astronomy. Prof. De Gubernatis gives us the beautiful little Russian nature-tale of the maiden Basiliça, who, on her way to the old witch's house, sees a black horseman all in black on a black horse, and then night falls; then she sees a white horseman on a white horse, and day dawns; then a red horseman on a red horse, and the sun rises. The story has been told already in England, but deserves telling again for its absolute certainty of meaning, which hardly requires the old witch's explanation that the black, white, and red horsemen are mythic personifications of night, day, and sun. If, then, we meet with stories very like unquestionable nature-myths, there is a strong case for the mythologists who say these stories are also nature-myths, whose original meaning has been forgotten, so that they have fallen into the state of mere fanciful tales. Thus, in an Esthonian story quoted by our author, this same notion appears of the three horsemen who are personifications of the great periods of light and darkness. The hero comes to deliver the princess from the glass mountain where she sleeps, and he comes dressed first in bronze colour on a bronze-coloured horse, next in silver on a silver-coloured horse, and lastly in golden garb on a golden horse. This certainly looks like a story suggested by the victorious noonday sun coming at last with glowing rays to accomplish the task he had failed to perform in darkness or twilight, to deliver the Spring from the icy fortress of Winter, or, as our nursery tale has it, to awaken the Sleeping Beauty in the Palace where the spell of Winter has bound her and hers in numbness and silence. *Valeat quantum.*

The scientific study of mythology will be advanced by the collection of mythic episodes made with extraordinary learning by Prof. De Gubernatis. It is a museum of material, and a good many of the author's rationalisations of old legends seem plausible. For instance, he adds new versions to the group of tales (to which belong "Tom Thumb" and "Little Red Ridinghood") in which the night is dramatised as a wolf or other monster, which swallows and afterwards releases the hero who represents the sun or day. He goes on to interpret in the same way the stories where the hero is shut up in the sack or chest and cast into the water, but comes safe to land after all, as the sun, shrouded in the shades of evening, crosses the ocean and reappears at morning. The value of such interpretations as these depends, of course, on careful comparison of evidence. Unhappily, however, the general method of the book is unscientific. The author has no strict logic in him. His argument is substantially this: natural phenomena often suggest to tale-tellers or poets ideas which they shape into cock-and-bull stories; therefore, the way to interpret cock-and-bull stories in general is to guess at some natural phenomena which may have suggested them. The consequence of such a principle of interpretation is a network of tangled guesses, which often only mystify the legends they pretend to explain. The ease with which such a method can be applied, and the worthlessness of its results when it is applied, are shown in the author's treatment of common proverbs. As a rule, proverbs really require no explanation; their origin is intelligible at a glance, as it always was; we feel we might have made them ourselves, if we had been clever enough, and proverb-making had been still in fashion. Not so our author. "The black cow gives white milk" means to him that the night produces the dawn, or the moon, or the Milky Way (we are allowed to take our choice which we like best). "Though the cow's tail waggles, it does not fall," seems to us to require no recondite explanation; but to Prof. De Gubernatis it connects itself with a whole fabric of speculations about the night-monster running after the dawn-cow's tail to clutch it. On the whole, we can hardly better characterise the work before us, in its combination of curious material and absurd argument, than by quoting the following piece of amazing nonsense, ending in a parenthesis with a little fact which will be new to most of our readers, and which shows that modern Italy has so kept up old classic customs, that the proverb "Ab ovo usque ad mala" still explains itself, just as we might now say, "From soup to dessert":—

"The hen of the fable and the fairy tales, which lays golden eggs, is the mythical hen (the earth or the sky) which gives birth every day to the sun. The golden egg is the beginning of life in Orphic and Hindoo cosmogony; by the golden egg the world begins to move, and movement is the principle of good. The golden egg brings forth the luminous, laborious, and beneficent day. Hence it is an excellent augury to begin with the egg, which represents the principle of good, whence the equivocal Latin proverb, 'Ad ovo ad malum,' which signified 'From good to evil,' but which properly meant 'From the egg to the apple,' the Latins being accustomed to begin their dinners with hard-boiled eggs, and to end them with apples (a custom which is still preserved among numerous Italian families)."

It is clear that a theorist who can thus turn the practical sense of his own dinner-table into mythological nonsense about sky-hens and sun-eggs, is no fit guide to students of Comparative Mythology. But his book will be useful to those who can profit by his learning and ingenuity, without being misled by his fantastic extravagance.

OUR BOOK SHELF

The Year-Book of Facts in Science and Art: exhibiting the most important discoveries and improvements of the past year in mechanics and the useful arts, &c. By John Timbs. (London: Lockwood and Co., 1873.)

WE are glad to notice in Mr. Timbs's annual volume an improvement in some of the points in which last year we called attention to very serious deficiencies. There is a more copious reference to the original authorities, though this is still too frequently withheld, and the statements thus deprived of all scientific value; and the references are in general to more trustworthy sources. There is also a sensible diminution in the number of glaring errors of the press, which have been so conspicuous a feature in earlier volumes. The compilation shows, as does everything from the hand of the same editor, unwearied industry; but with all that a lack of the power of distinguishing the worthless from the really valuable. Many of the paragraphs belong unquestionably to the former category, and it is difficult to see what purpose they serve except that of "padding." On the other hand some really important discoveries or applications of the year are altogether unnoticed. Considerable further improvement will be necessary before "Timbs's Year-book" becomes either an adequate or a trustworthy record of the scientific events of the year. The portrait of Dr. Carpenter given by way of frontispiece is exceedingly good.

Das Leben der Erde. Blicke in ihre Geschichte, nebst Darstellung der wichtigsten und interessantesten Erazen ihres Natur-und Kulturlebens. Ein Volksbuch von A. Hummel. (Leipzig: Verlag von Friedrich Fleischer, 1872).

Physikalische und chemische Unterhaltungen. Ein Volksbuch von Dr. Otto Ule und A. Hummel. (Leipzig: Verlag von Friedrich Fleischer, 1873.)

TILL the publication of Hummel's "Leben der Erde" there were scarcely any popular scientific works published in Germany, which may seem strange, seeing that that country has claimed, probably with justice, the intellectual leadership of the world for many years past. It is possible there is less need for popularising the results of science in Germany than in England and France, seeing that the German system of education is so thorough and comprehensive. Germans also have a greater tendency to go about everything in a systematic way; and this is shown with great force and clearness by Mr. Matthew Arnold to be especially the case in their educational organisation, which discourages the acquirement of knowledge in an irregular and haphazard way. In this country again, as well as in France, "the people" generally make their first acquaintance with subjects in which the German people are grounded when at school, long after they have left school from popular scientific treatises. These two works are constructed on somewhat the same plan as the well-known French works of Flammarion, Guillemin, and Reclus, and appear to us to be well and often eloquently written, and so far as we have been able to test them, are accurate and wonderfully full. In the second the authors aim at giving every-day illustrations of physical and chemical laws, and at showing their practical and economical bearings. They divide it into four sections:—1. General phenomena of motion as applied to solid, liquid, and aeriform bodies. 2. Sound, light, and heat. 3. Magnetic and electric phenomena. 4. Chemical phenomena. Hummel's *Leben der Erde*, we should think, would be the more popular of the two, both from the subjects treated of, the greater picturesqueness of language, and the greater abundance and attractiveness of the illustrations, some of which are very fine, though on the whole, not so well executed as such illustrations generally are in corresponding English and French works. He endeavours to show the relation of the earth to other heavenly bodies, gives its geological history, describes its physical geography, including the phenomena of land, water, and air, and concludes with a very eloquent account of the organic life of the earth. On the whole, both works seem to us very creditable to their authors.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Agassiz and Forbes

THE letter from Mr. Alexander Agassiz, published in last week's NATURE, revives an attack which was made by Agassiz and Desor more than thirty years ago. It was then promptly met. (See Forbes's "Historical Remarks on the first Discovery of the real Structure of Glacial Ice," Edin. New Phil. Journal, 1843.) I possess correspondence which abundantly shows that the scientific world (English and Foreign) was thoroughly satisfied with the answer given by Forbes. Much of this correspondence can, if necessary, be published. But the reply given at the time, and which I am confident will satisfy any unprejudiced person, may be found *in extenso* in Appendix B to the "Life and Letters of James David Forbes" (Macmillan and Co., 1873). No answer was ever attempted by Mr. Agassiz to the paper in question, and the facts it contains could not have been allowed to pass by him unchallenged, had they not been accurately given. Mr. Alexander Agassiz may never have read the original paper. The date of his letter shows that he cannot have seen the reprint in the Life of Forbes.

This impeachment of Forbes's character by Mr. Agassiz (made, I willingly grant, with the best motives, and in ignorance of the details of the case) demands an explanation. I am aware that few would give credence to imputations of dishonesty in Forbes's character; but the matter is also of historical interest, and deserves an historical examination. I will therefore, with your permission, lay before the readers of NATURE next week the facts from which they shall judge whether the assertions in Mr. Agassiz's letter are supported by the evidence, or not.

Blackheath, May 10 GEORGE FORBES

Venomous Caterpillars

IN Mr. A. Murray's paper on venomous caterpillars in NATURE of May 1, I observe that in discussing the distinction between the terms poison and venom, he says in reference to the action of snake poison:—"It is said that you may swallow the venom of the rattlesnake with impunity, and I imagine you may, if it does not get absorbed through the mucous membrane; but Dr. Fayrer's experience, lately published, of the effects of the semi-swallowing, which occurs in extracting the venom from a poisoned wound would rather seem to show that such extremely virulent venom would penetrate the mucous membrane and act as if actually introduced by a wound, his throat having become dangerously ulcerated from sucking the poison from the wound of a man bitten by a cobra."

If Mr. Murray will refer to my investigations on this subject, he will find that snake poison produces the same effect when applied to a mucous membrane, and introduced into the stomach, the eye, the intestine, or applied to the exposed surface of a muscle or peritoneum, though not so rapidly as when injected directly into the vascular system. The idea that it may be swallowed with impunity being quite incorrect. But I must disclaim (having no title to it) the experience he assigns to me in reference to the dangerously ulcerated throat, never having made myself a martyr to science by so experimenting in *propria persona.* I have no doubt, however, as I have elsewhere stated, that this method of treating a cobra bite would not be devoid of danger to the operator.

As to venomous caterpillars. There is one much dreaded by sportsmen in the Himalayan Terai. It is said to be apt to fall from the trees on to persons passing or resting beneath their branches, and causes great irritation of the parts with which it comes in contact, amounting, I have been told, in some cases to erysipelatous inflammation. It is a moderate-sized, dark-coloured, hairy caterpillar, and known (I believe) in those parts of the Terai where I have been, as the *kemla*[*] I have never seen it, but during my tiger-shooting expeditions into the Terai, it was always one of the probable inconveniences to be looked for in a camp in the tree jungle. I have heard many stories of the painful and irritating effects of contact with this creature, whose hairs are said to cause those results not only by breaking into but by also inoculating some irritating secretion into the skin.

London, May 4 J. FAYRER

I HAVE just read with interest your report of the paper on "Venomous Caterpillars," which appeared in your last. Towards the end of the report Mr. A. Murray refers to a hairy caterpillar which he received from Brazil, and remarks that "if the caterpillars have a special venom, then, as in the nettle, there should be a gland at the base of each hair, which should be hollow." I think I know the caterpillar to which he refers, and if I am right, its hairs are not exactly venomous, but produce a considerable amount of irritation in the skin. When in Brazil in 1859, I collected some of these caterpillars. They are very similar in appearance to the larvæ of the British Arctia, but when their hairs are examined under a microscope, they are found to consist of a series of barbed points, the point of each succeeding barb fitting into the divergence of the preceding barbs; at least, that is my recollection, for I have not examined them since then, and cannot find any specimens to do so now. The caterpillar is called in Maranham, "largata de fogo," that is, "fire caterpillar." After these hairs have afforded their protection to the caterpillar during its life, it carefully removes them from its body and weaves them in its cocoon, so that the pupa is thus as safe from intruders as the larva itself was. When a child, I recollect that Maranham was occasionally visited by great numbers of a particular kind of moth, the dust of whose wings produced a very great irritation on the skin, the least touch of one being sufficient to render you miserable for the rest of the evening. I perfectly remember a drove of these putting a quick termination to a small dance at home, as you may easily conjecture that ladies in evening costume are not well protected against such visitors. When in Maranham in 1859, I heard that these moths had not been seen there for many years. I believe their visits were during the rainy season. Some of the British Bombices, *B. quercus*, for example, and some of other genera, are said to possess irritable hair. But in *B. quercus* the hairs are not barbed, and, not being an entomologist, I can give no information respecting the others. HENRY S. WILSON

Anatomical School, Cambridge, May 5

On some Errors of Statement concerning Organ-pipes in Recent Treatises on Natural Philosophy

THAT our best teachers of science, both in their books and lectures make statements which are erroneous in fact, and inferences which are misleading whenever they touch upon the subject of wind instruments is not a little surprising, considering that intellects so highly trained hold in aversion any approach to inexactness, and the strangeness of it is that the errors arise through an ancient human custom, now supposed obsolete among philosophers, of "speaking without knowledge."

The evidence, if tendered, would fill some few pages of this paper, and if names were appended to the quotations the list would include authors most esteemed and honoured.

To cite two instances among many—and they are from works of unquestioned value and authority, and supposed to bring down sciences to the latest date—in the recently completed translation by Prof. Everett of Prof. Privat-Deschanel's "Natural Philosophy," the following passage occurs in explanation of the organ-pipe:—"The air from the bellows arrives through the conical tube at the lower end, and before entering the main body of the pipe has to pass through a narrow slit, in issuing from which it impinges on the thin end of the wedge placed directly opposite, called the lip. This lip is itself capable of vibrating in unison with any note lying within a wide range, and the note which is actually emitted is determined by the resonance of the column of air in the pipe." In another equally valuable work, the "Physics," by Prof. Ganot, translated by Prof. Atkinson, this description is given respecting the free-reed—"the tongue which vibrates alternately before and behind the aperture, merely grazing the edges as seen in the harmonium, concertina, &c., such a reed is called a free reed." Four professors responsible for statements so perversely at variance with facts that it is not possible either writer can have even attempted to ascertain, still less to demonstrate that the facts are as asserted. Practical experience affirms that the lip of the organ-pipe does not vibrate; press it with your hand or hold it in a vice to deaden the assumed vibration, and you will not alter one iota of the pitch of the sounding note: that the free-reed does not in its vibration "merely graze the frame;" it would be fatal to its proper speech if it did, and its vibrations would be checked in a jarring rattle. The facts are too simple to need argument; all that was required was observation.

When Ganot, describing a metal free reed, affirms as a law that when the force of air is increased the pitch of the reed rises, his statement is inexact, for it depends entirely on the accident of taking up a reed more or less rigid in proportion to scale, whether the experimentalist shall prove his assertion or prove the reverse. In the harmonium, of a set of five octaves of reeds, half will go more or less sharp, and half will go more or less flat, as the force of wind is increased, a fact which, if more generally known, might induce players to mitigate some of the insufferable harshness and jangling inflicted on listeners. That "a sharp edge" is essential to the functions of the flue organ pipe is one of the commonest errors entertained by philosophers, and it forms the groundwork for whole pages of false theory. In treatise after treatise it is stated "the air is driven against the sharp edge," "is split upon the sharp edge and by concussion caused to proceed intermittingly," "the air strikes the sharp edge," "is divided," "is lacerated," "strikes against the upper lip, and a shock is produced which causes the air to issue in an intermittent manner." Another equally common misstatement, and important because so strongly influencing theory, is that "a closed pipe gives a note an octave in pitch lower than an open pipe of the same length; the length of a closed organ-pipe is one-fourth that of the sonorous wave it produces in the air." Proved facts give different results. At my hand this morning there stood a sounding-pipe perfect in finish, its lip quite blunt, by measurement at the edge half an inch in thickness; and whole ranks of pipes were there in various grades of conformation, showing that the sharp edge was immaterial to the functions of a speaking-pipe. Sometimes the chamfering of the lip is desirable, sometimes not, and the builder decides according to the quality and character of each stop. The art in "voicing" a pipe consists in so directing the stream of air that it shall avoid striking the lip, and shall smoothly glide past without shock or noise, or concussion; you get no tone until it does. Actual experiment will show that a closed pipe gives a note only a major seventh below the note it gives as an open pipe, not an octave below; indeed, in the higher range of pipes it will be a whole tone short of the octave, to sound which the pipe would need to be made considerably longer. As having some significance in connection with this, it may be mentioned that there is in an open pipe, whilst sounding, a centre of equilibrium of pressure; it does not occur, as supposed, at the true half of the length, but somewhat below that division; as evidence, take the Flute Harmonique, when desiring to strike the node, it will always be found below the half. Further, as to length. If the open diapason pipe beside me, giving as fine a tone (CCC) as musician can desire, measures 14 ft. 10 in. in length, and its corresponding sound-wave claims 16 ft. or nearer 17 ft., the wide divergence merits better investigation than it has hitherto received. The experiments of Regnault and Seebeck are highly important to this question, but do not reach the conditions pressing for explanation in a speaking organ-pipe. To attempt to demonstrate the laws of organ-pipes with a tuning-fork is as inconclusive as sending galvanic electricity through a dead body and calling the movement life.

There is little difficulty in understanding how it happens that errors respecting wind instruments arise and are perpetuated. Experimental philosophers are occupied with the weightier matters of science, are rarely musicians or familiar with wind instruments; of the trouble and anxiety their caprices give at home and in the workshop they have no knowledge. The organ-pipe is brought into the lecture-room, it is caused to prove what is wanted, more is not looked for; it comes like a beauty in a ballroom, dressed up to play a part and be amiable and gracious: the practical man knows that organ-pipes are very like human beings, of whom Goethe says, "We do not learn to know people when they come to us; to learn their real peculiarities we must go to them."

April 18 HERMANN SMITH

Rock Inscriptions of Brazil

BEING unable to attend the reading of Mr. Whitfield's paper, at the Anthropological Institute, April 22, the following observations are offered.

The rock inscriptions of Brazil are worthy of attention, because they appear to belong to a vast series, to which Mentone affords a large contribution. The suggestion that in the very earliest epochs tally records existed, lends interest to the investigation. It appears probable that military tallies of the levy of men preceded the registers in the historical period of the tribute of men, arms, and money by provinces, such as we find in Herodotus with regard to Persia.

In reference to the possible general connection of such inscriptions as these with the eastern world, it may be observed that Brazil has participated in at least two great migrations.

The Kiriri and Sabuyah of Bahia are allied by language to the ancient Pygmean or Negrito stock. This race is everywhere very low, and cannot have produced even these inscriptions.

The greater part of Brazil is covered by the Guarani or Tupi (Agua) languages allied to the Agau of the Nile region, the Avkhass of Caucasia, &c. It is worth inquiry whether the Mentone inscriptions may not belong to this epoch.

HYDE CLARKE

Abnormal Coloration in Fish

SEEING Mr. W. S. Kent's letter on this subject in NATURE of the 8th inst., a similar instance was recalled to my memory. About three weeks ago I observed in a fishmonger's shop a plaice, nearly one third of the under side of whose body (at the tail) had the usual colour and orange spots of the upper. In this specimen the spots were more numerous and brilliant than usual. The line of demarcation was irregular, but abrupt. The circumstance struck me because I have seen great numbers of Pleuronectidæ, but never one marked thus. The fishmonger told me that he had never seen a like specimen.

ARTHUR NICOLS

Phosphorescence in Wood

FROM the description given by your correspondent, Richard M. Barrington (vol. vii. p. 464) of phosphorescence in coniferous wood, I should imagine it to be extremely probable that the pieces of Scotch fir in question were infested with the spawn of *Polyporus annosus* Fr., a fungus very common on the Coniferæ. The mycelium of this plant (as well as the perfect fungus) is well known to be at times highly phosphorescent, and in the *Gardener's Chronicle* for September 28, 1872, I have figured the perfect state of it as seen so commonly in a luminous condition in the coal mines of Glamorganshire. In these deep pits the spawn of this fungus ramifies about the old shoring timber, and is so highly phosphorescent as to be clearly seen from a distance of twenty yards. Many other fungi with their mycelia are known to be at times phosphorescent, as *Polyporus sulfureus* Fr. and *Corticium cæruleum* Fr., both common on decaying wood. In the *Gardener's Chronicle* for September 21, 1872, the Rev. M. J. Berkeley has published a remarkable case of phosphorescence in logs of larch. Here the most luminous parts were where the mycelium was most developed, and the wood gave out such a blaze of white light that although the pieces were wrapped in five folds of paper, yet the light shone through as if the specimens were exposed. The phosphorescence appears to accompany the decomposition of the wood on which the fungi at the same time prey.

W. G. SMITH

Coincidence of the Spectrum Lines of Iron, Calcium, and Titanium

IN Prof. Young's letter published in NATURE, vol. vii., p. 17, some coincidences of the lines of different substances which "are too many and too close to be all the result of accident" are referred to, those of iron with calcium and titanium being especially cited. Two explanations are offered, first that "the metals operated upon by the observers who first mapped out the spectra were not absolutely pure," and second, that "there is some such similarity between the molecules of the different metals as renders them susceptible of certain synchronous periods of vibration."

If we are driven to this second explanation the received inductions of spectrum analysis and the deductions of celestial chemistry based upon them are shaken at their foundation, for if more than one known terrestrial element can display identical lines in the spectrum, the suggestion that other unknown celestial elements may do the same is freely opened. It is therefore very desirable that the spectroscopist should receive all the aid which the studies of chemical specialists can afford him towards the solution of this problem.

I may venture to speak to the instances quoted by Prof. Young. First as regards calcium and iron. In making analyses of a large number of brands of pig iron I found that they all contained calcium, but in very variable proportions, and endeavoured by observing their properties, and by further examination of finished iron, to learn how the presence of calcium affected the quality of iron, but failed to solve this problem. In the course of these investigations, I found that the finished iron, like the pig, presented considerable variations as regards the quantity of calcium contained in it, *but I never found a sample of iron or steel quite free from some trace of calcium.* As I was operating for the most part on superior qualities of iron which had been submitted to the utmost practicable degree of commercial purification, this experience renders it extremely probable that Prof. Young's first explanation is the correct one, so far as iron and calcium are concerned.

The want of any chemical reagent by which minute traces of titanium can be detected in the presence of large quantities of iron, or of a means of completely separating these metals, places a serious difficulty in the way of directly answering the question whether iron is usually associated with traces of titanium; but there are indirect evidences of its very common existence in ordinary iron. The most decided of these is afforded by the common, almost universal, occurrence of the beautiful copper-coloured crystals of cyano-nitride of titanium in the hearth bottoms of blast-furnaces. In many cases their concretions form large masses, where the ores that have been used are not supposed to be titaniferous.

Metallic iron obtains impurities, not only from its ore, but also from the fuel and flux used in reduction, and besides these from the furnace or crucible in which it has subsequently been fused or raised to its welding point. The difficulty of completely purifying iron is so great that many such coincidences as those referred to may be expected *a priori.*

W. MATTIEU WILLIAMS

Musical Stones

WHEN roaming over the hills and rocks in the neighbourhood of Kendal, which are composed chiefly of mountain limestone, I have often found what we call here "musical stones." They are generally thin flat weather-beaten stones, of different sizes and peculiar shapes, which when struck with a piece of iron or another stone, produce a distinct musical tone, instead of the dull heavy leaden sound of any ordinary stone. The sound of these stones is, in general, very much alike, but I know gentlemen who possess sets of eight stones which are said to produce, when struck, a distinct octave. Being only an amateur geologist, I am unable to account for this fact, and would be glad if any of your numerous readers would take the trouble to explain to me, through the medium of your columns, the peculiar composition of the stone in question, and the distinct qualifications necessary to form a musical stone.

RICHARD J. NELSON

Acquired Habits in Plants

IN NATURE of May 1, p. 7, which I chance not to have seen till now, Mr. Babbington puts a question on the subject of my climbing specimen of violet which I fear I am not botanist enough to answer.

I described it as a "dog" violet simply because it bore leaves and flowers on the same stem, which in my simplicity I supposed was enough to settle its species. But though the subdivisions of *V. canina* be new to me, a word or two of remark and description may elucidate the required point to other eyes. I would add that the specimen, such as it is, is very much at Mr. Babbington's service should he care to see it. It is still recognisable, no doubt, though it suffered considerably from having no better protection for some hours than a fly-book.

In the first place it was not growing in a moist situation or one to account for luxuriance. Though near the river, it was many feet above the water, and was on the further side of a small high road. In this position it had, as I before mentioned, attained a height of two feet and a half, and the flower which first attracted my eye was almost on a level with my waist. The plant had climbed through the hedge like a vetch or a fumitory. On comparing it with the most robust specimens of *V. canina* which I can find this spring, the following points of resemblance and of divergence present themselves. The stem of mine is channelled in the ordinary way, and the leaves tolerably like in shape though rather more pointed. On the other hand, the leaf-stalks and peduncles are in mine much shorter, the upper leaves being almost sessile. The position of the bracts is similar, but instead of the conspicuous stipules of *V. canina*, mine has those parts so small as almost to escape notice. Again, while the stem of *V. canina* does not in my experience branch, the stem of mine has, in two places, thrown off a small branch bearing leaves and flowers. Also there was not, as far as I remember, any trace of any shoot from the root except the one stem, while *V. canina*, as ordinarily found, sends up a greater and a lesser flowering stem and a bunch of leaves besides.

I hope that these particulars will shed more light on the subject than I can myself. J. G.

St. Asaph, May 10

JOHN STUART MILL

BORN MAY 20, 1806; DIED MAY 8, 1873

THOUGH it has not been the custom among specialists to regard Mr. John Stuart Mill as a scientific man, yet we venture to say that he has not left behind him in this country any man who has done more for the general advancement of science. Before Mr. Mill's time men found their way to great discoveries, and succeeded in proving to each other that what they had discovered was scientific truth. But they could tell each other very little about the method of scientific investigation. Indeed Whately, the then greatest authority in logic, pronounced a theory of induction impossible. Mr. Mill, however, did formulate the canons of induction, and in so doing he lit a lamp which will for ever burn a steady guiding light in the path of the scientific inquirer. And the value of this light need be regarded as none the less even if we consider that its chief service lies in guiding us past the snares and pit-falls of error, and the entrances to those mazes and endless labyrinths of unreality in which so many powerful intellects have toiled and spent their strength for nought; nay, worse than in vain, for their brilliant struggles have fascinated thousands and drawn them from the sober highway of truth, which alone is the road to usefulness—to happiness. The vast and still growing influence that Mr. Mill has exerted in this direction is fully recognised by those who regret it most, because they believe that Truth may be reached by other and nobler paths. We are content to note the fact that among the great men of our day no one has done so much as he, to widen the domain of science and to subdue to its methods all subjects of human interest. Choosing for the field of his more serious labours several of the most difficult subjects of research, those that had most eluded the grasp of the understanding, he has enriched the world with works that will long remain monuments of science. His "Logic" is our text-book of the science of evidence. His "Political Economy" is our text-book of the science of wealth. And if there is a scientific work on politics it is Mr. Mill's "Representative Government." One feature of Mr. Mill's character deserves special notice in this connection. He had the true scientific temper, a disinterested love of truth, in a degree not to be surpassed. If it could be shown that in any particular his teaching was unsound, and none were ever able to do this so well as his own disciples, the men whom he had trained to think, no one was more glad that error had been detected than was Mr. Mill himself. It will be enough to remind our readers of one notable example of this. When Mr. Thornton showed that the universally accepted doctrine of the wage-fund was a huge fallacy, Mr. Mill came forward with alacrity to acknowledge that he in common with all other political economists had fallen into a grave error, and that Mr. Thornton had made a most valuable contribution to economic science. If all scientific men could as completely subordinate their personal vanity to the pursuit of truth, progress would be more rapid than at present. The daily papers have already made the reader familiar with the many-sided richness and beauty of Mr. Mill's character. He was an object of loving admiration to all who had the happiness to enjoy his personal acquaintance. The world, while it mourns his loss, does not, cannot know how great and how good a man has been taken away; and still less does it know how ill it can afford to lose such a man.

MINERS' RULES IN THE SEVENTEENTH CENTURY

ON looking over a packet of old papers I have found some documents, of which I enclose copies, written by a German miner, named Brandshagen, who was employed by my ancestor, Sir Philip Egerton, to superintend the attempt to work copper in the New Red Sandstone strata of Cheshire in the year 1697. As the *rules* for miners of that age afford so strong a contrast to the *unruly* behaviour of that class at the present day, they may perhaps interest some of the readers of NATURE. P. DE M. GREY-EGERTON

Worthy & most honourable Sir,—

Your worship give most humbly thanks for employment meself and my countrymen about your Worship mines, which I have enjoyed now above 4 weekes, & not to be att all further unacquainted unto your Worship, I could not forbeare to give a true & plain account of what I have observed in this time about these mines, as good as my smal understanding in y^{e} English linguage would permit, & if it was in any way acceptable then my wishes & desires where fullfilled. I have this time also endeavored to blow up y^{e} rocks by guns powder, as the best way to kill them, butt in y^{e} first time I found y^{e} elements as aire & water where against my designe, y^{e} last I have conquered, & I hope I shall doe so y^{e} other next time when I have occasion for it. I found also some other smal things which would not so soon agree with my hands, for there are many years past, that I did work under ground with my owne hands, butt all these things are now disceased, onely that I was lately too covetous & would have more rocks blown up then my powder was able to; what other blasts for effect have done, your Worship can be informed of it by Mr. Smith. I shall endeavour all what is in my power to serve your Worship with that understanding I have about mines to which I have employed meself now above 15 year, in spending a great deal of money as well for learning as travelling in many places in Europe where good mines where, to come to any perfection in this art. I have received now my things for examination of y^{e} oare, which I will doe as soon as possibly I can come to it in this desolate place, where nothing in y^{e} world is to be had for any commodities what soever it may be, & whilst we are strangers here, & must buy all things for ready, it is impossible to life of what your Worship has allowed unto us & therefore I doubt not your Worship will make a distinction between workmen & workmen, with which I recommend me into your Worship' favour allways remaining

Your Worship most humble Servant,
J. A. BRANDSHAGEN

Bickerton, Sept. y^{e} 24th, 1697
For the Right Honourable S^{r} Phillipp Egerton, Knt., these.

Rules for all Workmen in general

One of every Workmen he may be of what sort he will shall come half an hour before y^{e} duely time & give a certain number of strucks. with a hammer on an Iron plate, erected to this purpose, to give a Signe to y^{e} other workmen to come att work, half an hour after he shall doe so att a second time by an other number of strucks & shall streike no more then y^{e} duely strucks by forfeiting 2*d.*, he has y^{e} same signes to give all day when y^{e} miners shall come out & goe under ground again, & this shall doe one workmen after an other from day to day, & he who has done y^{e} businesse this day shall remember to his follower that he has to doe y^{e} same next day, & he that wilfully neglected these remembrance shall be punished together with him that shall doe this businesse next day (if he neglect it) for he himself must be carefull about y^{e} time & day to doe this, & he that shall give y^{e} signs too late, has forfeited 6*d.*, & he that shall not doe it att all shall loose all his wages, due to him, & by consent of y^{e} mines Lords shall be turned of from y^{e} work.

In y^{e} morning before y^{e} last struck is done on y^{e} Iron plate every workman belonging to y^{e} mines must appeare to y^{e} appointed place near y^{e} work, or he has forfeited 2*d.*, & he that comes half-an-hour after, 2*d.* more, & so following for every half-an-hour 2*d*, & this is understood of all times when y^{e} signe is given.

When they are together they may doe a short prayer that God may give his blessing to their work, that it may raise to y^{e} honour & glory of him, & to y^{e} benefit & blessinesse of y^{e} mines Lords & their whole familie.

After this every one must goe to his post, & diligently performe to what y^{e} steward shall order him, in doing y^{e} contrary he shall be duely punished, & he who shall leave y^{e} work within y^{e} duely hours & before y^{e} signe is given, shall loose 6*d.* or for every half-an-hour 2*d.* as y^{e} steward shall think fitt, & he that is found neglectfull shall every time have forfeited 2*d.*

When it is pay-day, every workmen before he gett money must shew to y^{e} steward his tools & other things what is trusted in his hand by y^{e} lost of all his wages, & if there should want any of such things, he must leave so much money of his wages as it is worthy in y^{e} stewards hand, till he restores y^{e} same.

He that hindered one an other in his work it may be in what way it will, either by ill words, quarreling or in other ways, must duely be punished as y^{e} steward thinks fitt, because every one must be quiet with his work; have they any thing one against an other they may bring it before y^{e} steward, or cleare their things after y^{e} work is done att an other place.

No body shall be permitted without leave of y^{e} steward to take any oare away for a shewing piece, or under any other pretext, butt he may y^{e} same aske from y^{e} steward & be content with that he gives him, and if any should doe y^{e} contrary, he is so heigh to punish as y^{e} steward shall think sufficient.

No body shall bring any person or persons not belonging to y^{e} mines, either under ground or at any other place where y^{e} oares or other things are, without permission of y^{e} steward, & that by y^{e} penalty of one shilling.

Every man must be in a Christian-like beheaviour, and he that speekes blasphemes, or gives scandales, or does other things near y^{e} mines with which God is offended, shall every time be punished with 4*d.* or more according to his crime,

When it is pay-day every one must be of a modest behaviour against ye steward, and must not murmer against him when his wages is decurted for punishement, butt must bring his complaints (if he has any against it) before y^{e} mines Lord, if neverthelesse that he has gotten his wages, he must not goe from y^{e} steward away, till y^{e} whole payment is done, & can give witnesse that every one has received his due.

No workmen shall make more holy days in y^{e} year besides y^{e} Sunday, then y^{e} Lords of y^{e} mines shall allow them, or shall be punished as one that leaves y^{e} work for a whole day.

He that turned y^{e} hour glasse in a wrong way shall loose one shilling.

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*SUPPRESSION OF SCENT IN PHEASANTS**

THE pheasant, from nesting on the ground, is peculiarly exposed to the attacks of four-footed or ground vermin, and the escape of any of the sitting birds and their eggs from foxes, polecats, hedgehogs, &c., appears at first sight almost impossible. This escape is attributed by many, possibly by the majority, of sportsmen to the alleged fact that in the birds when sitting the scent which is given out by the animal at other times is suppressed; in proof of this statement is adduced the fact that dogs, even those with the keenest powers of smell, will pass within a few feet, or even a less distance, of a sitting pheasant without evincing the slightest cognizance of her proximity, provided she is concealed from sight. By others this circumstance is denied, they reason *à priori* that it is impossible for an animal to suppress the secretions and exhalations natural to it—secretion not being a voluntary act. I believe, however, that the peculiar specific odour of the bird is suppressed during incubation, not, however, as a voluntary act, but in a manner which is capable of being accounted for physiologically. The suppression of the scent during incubation is necessary to the safety of the birds, and essential to the continuance of the species. I believe this suppression is due to what may be termed vicarious secretion. In other words, the odoriferous particles which are usually exhaled by the skin are, during such time as the bird is sitting, excreted into the intestinal canal, most probably into the cæcum or the cloaca. The proof of this is accessible to every one; the excreta of a common fowl or pheasant, when the bird is not sitting, have, when first discharged, no odour akin to the smell of the bird itself. On the other hand, the excreta of a sitting hen have a most remarkable odour of the fowl, but highly intensified. We are all acquainted with this smell as increased by heat during roasting; and practical poultry keepers must have remarked that the excreta discharged by a hen on leaving the nest have an odour totally unlike those discharged at any other time, involuntarily recalling the smell of a roasted fowl, highly and disagreeably intensified. I believe the explanation of the whole matter to be as follows: the suppression of the natural scent is essential to the safety of the bird during incubation; that at such time vicarious secretion of the odoriferous particles takes place into the intestinal canal, so that the bird becomes scentless, and in this manner her safety and that of her eggs is secured. This explanation would probably apply equally to partridges and other birds nesting on the ground.

The absence of scent in the sitting pheasant is most probably the explanation of the fact that foxes and pheasants are capable of being reared in the same preserves; at the same time the keepers are usually desirous of making assurance doubly sure, by scaring the foxes from the neighbourhood of the nests by some strong and offensive substance.

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* From Mr. Tegetmeier's forthcoming work on "Pheasants for the Covert and the Aviary."

THE NEW PROFESSOR OF ENGINEERING AT GLASGOW

IT has already been announced in NATURE that the Crown authorities have appointed Prof. James Thomson, C.E., LL.D., to succeed the late Prof. W. J. M. Rankine in the Glasgow Chair of Engineering and Mechanics; and as that gentleman has been deemed worthy to occupy the Chair that was long filled by a man of world-wide eminence, it may not be undesirable to give a brief sketch of his professional and scientific career.

Prof. Thomson is the elder brother of Sir William Thomson, and son of Dr. James Thomson, a former Professor of Mathematics in the University of Glasgow. The early part of his education was obtained in the Royal Belfast Academical Institution, and he completed his studies in Glasgow, where he obtained the degree of M.A. in 1840, with honourable distinction in Mathematics and Natural Philosophy. During the year 1841–42, he was a student in the class of Civil Engineering and Mechanics under Prof. Lewis D. B. Gordon, C.E., Rankine's predecessor, and even then he was distinguished for his accurate mathematical and physical knowledge, and for his ready appreciation of the principles of applied mechanics. He afterwards became an industrious pupil in the Horseley Iron Works and Manufactory, near Tipton, in South Staffordshire, and subsequently he entered the service of Mr. (now Sir) William Fairbairn, in whose workshops on the Isle of Dogs and in Manchester he had the benefit of assisting to execute engineering works of the greatest magnitude, and of great variety. After prosecuting his profession for several years in England and Scotland, he ultimately settled down in Belfast as a civil engineer.

When the Professorship of Civil Engineering in Queen's College, Belfast, became vacant in the year 1857, Mr. Thomson obtained the appointment. He has now occupied that position for a period of fifteen years.

Besides attending to the duties of his class, Prof. Thomson carried on an extensive practice as a consulting engineer, both at home and abroad, chiefly in connection with water supply, irrigation, the drainage of sugar plantations in Demerara and Jamaica, and other swampy lands, and in designing machinery for the same, and in other hydraulic works. One of his earliest inventions was the well-known Vortex Turbine, which affords an admirable example of an unusual combination of great scientific knowledge and practical skill in the same person. This application of mechanical principles is one of the most successful means of turning water power to advantage that has hitherto been placed at the service of the engineering profession. Many examples of the Vortex Wheel are now in successful operation in various parts of the world, and the invention was deemed to be so important that the Privy Council renewed the patent when the ordinary period of fourteen years had expired. Another of his useful inventions is the Jet Pump and Intermittent Reservoir for the drainage of swampy lands.

Among Prof. Thomson's inquiries in the domain of pure physics a prominent place must be given to those which he instituted regarding the lowering of the freezing temperature of water by pressure. This he determined by theoretical considerations entirely, and the result announced by Prof. James Thomson was afterwards exactly confirmed by the experiments instituted by his distinguished brother. The "arrival by theory without the aid of experiment at so extraordinary a physical fact, calls to my mind most forcibly," says Joule, "the discovery of Neptune by Adams and Leverrier, and is one great step towards the position to which we may eventually hope science to attain, when a perfect acquaintance with theoretical principles will enable us to dispense with the appeal to experiment so necessary, in most cases, at the present time." This discovery and its experimental verification immediately suggested a perfect solution of the problem of the descent of glaciers, and it has since led to many kindred discoveries in pure science. Like his predecessor, Prof. Thomson has extensively contributed to the advancement of science through the medium of the British Association. On five separate occasions he has been selected as the Secretary of the Mechanical Section of that body, and he has been a number of times specially deputed to make reports and conduct experimental researches for the solution of questions in practical engineering. The tendency of Prof. Thomson's mind may be, to some extent, judged of by the character of the papers on physical, mathematical, and mechanical subjects which he has published or communicated to various scientific bodies. They are nearly forty in number, and are published in full or abstract in the *Cambridge and Dublin Mathematical Journal*, the *Edinburgh New Philosophical Journal*, the Transactions of the Royal Societies of London and Edinburgh, the Proceedings of the British Association, and the Transactions of the Institution of Engineers in Scotland.

Prof. Thomson's honorary degree of LL.D. was obtained from the University of Glasgow about two years ago. His formal induction by the Senatus of the University took place last month, and his professional duties in his *alma mater* will commence in the ensuing winter session.

JOHN MAYER

THE FERTILISATION OF THE WILD PANSY

AMONG the accurate and acute observations of C. C. Sprengel towards the close of last century,* which have received but scant attention from his successors, even down to our own day, was one on the subject of the colouring of variegated flowers. This botanist, with an insight into the mutual relationships of animal and vegetable life far in advance of his age, suggests that this colouring may serve as a guide to insects in seeking for the honey which serves for their food, and the search for which is so powerful an agent in the conveyance of the pollen, and the consequent fertilisation of the flower. Sprengel pointed out that in almost all variegated flowers the variegation follows a regular pattern, and that when it consists of streaks or stripes, these streaks almost invariably point to the nectary, or the receptacle of the sweet secretions which form the food of insects, in whatever part of the flower it may be situated. With this idea as a starting point, an interesting line of inquiry may be carried out as to the connection between the presence of scent and the absence of variegation in flowers. It will be found as a general rule, though not without exceptions —and it would be very interesting to attempt to trace the reason of these exceptions—that those flowers which possess a powerful odour are (in the native state) self- or whole-coloured, while brilliantly variegated flowers are, as a rule, scentless. On the hypothesis that each of these properties has for its object the attraction to the flower of the insect necessary for the fertilisation of its seeds, it is easy to be seen that the presence of both in the same flower is needless; and hence we find that Nature is in the habit of husbanding her resources, and not supplying needlessly to the same flower two different provisions for securing the same end.

Having had an opportunity during the present spring of observing the structure, with reference to the phenomena of fertilisation, of the flower of the common Wild Pansy (*Viola tricolor* sub-sp. *arvensis* of Hooker's "Student's Flora") I have thought a description of it might be of interest to the readers of NATURE, and especially to anyone who is able to contrast the phenomena in the variegated and scentless pansy with those in the scented and almost whole-coloured sweet violet.

The corolla of the wild pansy consists of five petals

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* Das entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen: von Christian Konrad Sprengel. Berlin, 1793.

(Figs. 1, 2), the two upper ones of which, *a*, *b*, have no colouring, the two lateral petals *c*, *d*, have each one conspicuous broad streak, and are furnished near the base with a tuft of hairs; while the lowest, *e*, has a number of streaks, usually either 5 or 7, and is also provided with a tuft of hairs near the base; this petal is prolonged below into a spur. All the streaks, on both the lateral and the lowest petal, point exactly towards the centre of the flower *f*, where are the stamens and pistil. The stamens (Figs. 3, 4, 5) are also five in number; the filaments, *a*, are very short; the anthers, *b*, form a circle surrounding the pistil, closely applied to it, and also closely touching one another at their edges; each anther has the connective, *c*, prolonged above into an orange-coloured appendage; and these also, somewhat overlapping one another, form a complete ring round the pistil. Two of the stamens are prolonged below into remarkable kneed appendages, both of which project down into the spur of the lower petal, partially filling it up. The pistil (Figs. 6, 7) consists of a nearly globular ovary, *a*, an irregularly curved style, *b*, much narrower below, and furnished in front with a remarkable wedge-shaped black line, *c*, and of a single stigma, *d*, hooded in shape, the viscid stigmatic surface of which is contained in a deep cavity near its summit. In the open flower, this stigma (*e*, Fig. 3) has a most grotesque resemblance to a monkey's or old man's face. The anthers open laterally and rather within, for the discharge of the pollen, so that it falls naturally on the lower part of the style, which they completely invest, and it is difficult to see how, without artificial means, any of it will reach the stigma; the flower is also distinctly protandrous, the stigmatic cavity not being fully matured till the flower has been some time open and the pollen fully discharged. The "nectary," or part specially devoted to the secretion of the honey, is the termination of the two appendages of the stamens which project into the spur of the corolla (indicated at *f*, Figs. 3 and 5). When the sweet juice is collected here in sufficient quantities, it drops down into the bottom of the spur, to which all access of rain is prevented by the hairs that fringe the petals around the entrance of the passage to the spur.

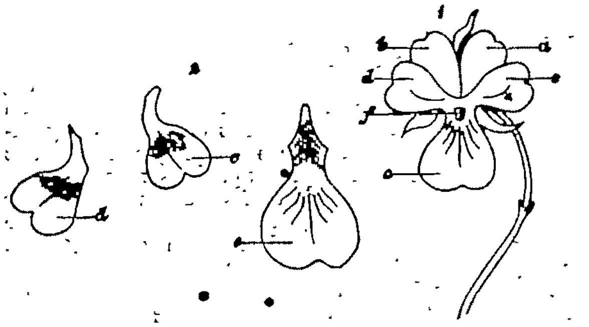

Fig. 1.—1, Flower of *Viola arvensis* *a*, *b*, upper petals; *c*, *d*, lateral petals *e*, lower petal, *f*, centre of flower. 2, The petals separated; *c*, *d*, lateral petals, *e*, lower petal.

With regard to the fertilisation of the violets, which, as has been mentioned, can obviously scarcely take place without foreign aid, Sprengel gives a long and very full description of the manner in which the sweet violet is visited by bees and humble bees, the insertion of whose proboscis into the spur of the corolla, and then its withdrawal, will necessarily remove some of the pollen, and bring it into contact with the stigma either of the same or of a different flower. It seems hence to have been assumed rather than observed that the wild pansy is fertilised in the same manner; although Sprengel states that he has not usually seen this species visited by bees, and Muller's observations* are by no means decided. My own view is that the wild pansy is fertilised chiefly, if not entirely, by very minute insects of the Thrips kind. During a long observation one morning this spring of a field in which these flowers were very abundant, I never once saw them visited by a humble-bee or other large species, and the only insect observed to frequent them was a little species of Thrips, and these only in small numbers, which I attribute to the circumstance that my only opportunity was the first warm sunny morning after a long period of cold weather, when but few insects had yet left their winter retreats. Sprengel indeed says that the wild pansy is greatly frequented by Thrips, although he believes the fertilisation to be effected by bees.

If this view be correct, the markings of the flower furnish the insect with a most remarkable series of guide-posts (or, as Sprengel terms it, "Saftmaal") to the nectar which serves as its food. The streakings on the lateral and lower petals form a sure guide, as soon as the little visitor reaches the flower, all converging (as shown in Fig. 1) to the centre of the flower and summit of the ring formed by the connectives of the anthers. Here even a minute Thrips can with difficulty force its way between the style and the closely adjacent ring of anthers, the deep orange tips of which would naturally attract it; but here it meets with a most curious and valuable assistance in the wedge-shaped streak on the front side of the style (as seen at *c* in Figs. 6 and 7), the broad upper end of which is distinctly visible above the anther-ring, tapering downwards to a sharp point near the bottom of the style, where the insect would be at once landed on the upper part of the kneed appendages, along which it has now simply to descend until it reaches the nectar, the object of its journey. The style is much narrower towards the base than above, and hence there is room for a considerable accumulation of pollen here, as it escapes from the anthers. The insect must necessarily carry away a considerable quantity of the pollen in its descent and ascent of the style; whether for the purpose of pollenising the stigma of the same or of a different flower is not at first sight clear. The heteracmy of the flower (*i.e.* the male and female organs being mature at different periods) favours the idea of cross-fertilisation, which may very well happen from the active little Thrips visiting many flowers in the course of a day. The ovules of the wild pansy are indeed abundantly fertilised, much more generally, in fact, than those of the sweet violet, the mature capsules of which frequently result from the unopened, self-fertilised, "cleistogenous" flowers, which have not, as far as I am aware, been observed in the pansy.

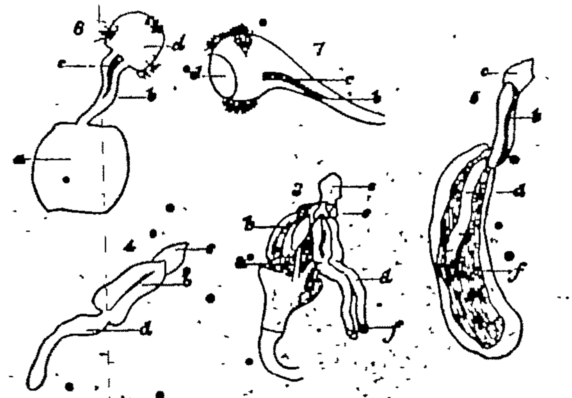

Fig. 2.—3, Pistil and stamens; *a*, filaments; *b*, anthers; *c*, connectives, *d*, appendages to lower stamens; *e*, stigma, *f*, honey-glands. 4, Lower stamen (enlarged); *b*, anther; *c*, connective, *d*, appendage 5, The same, seen within the spur of the corolla 6, Pistil; *a*, ovary; *b*, style; *c*, wedge-shaped streak; *d*, stigma. 7, The same, seen laterally at a later stage.

ALFRED W. BENNETT

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* Die Befruchtung der Blumen durch Insekten und die gegenseitigen Anpassungen beider: von Dr. Hermann Muller. Leipzig, 1873.

NOTES FROM THE "CHALLENGER"*

II.

On Sunday, March 2, we saw the first patches of gulf-weed drifting past the ship, and flying-fish were abundant. Our position at noon was lat. 22° 30′ N., long. 42° 6′ W., Sombrero Island distant 1,224 miles. At night the phosphorescence of the sea was particularly brilliant, the surface scintillating with bright flashes from the small crustaceans, while large cylinders and globes of lambent light, proceeding probably from *Pyrosoma* and some of the Medusæ, glowed out and slowly disappeared in the wake of the vessel at a depth of a few feet.

The next morning we sounded at 7 A.M. in 2,025 fathoms with No. 1 line, the "Hydra" machine and 3 cwt., a slip water-bottle, and one thermometer; a stop-cock water-bottle was bent on at 925 fathoms from the bottom. The corrected bottom temperature was 1°·9 C., the temperature of the surface being 22°·8 C. During the morning the naturalists were out in a boat with the towing-net, and they brought back a number of fine examples of *Porpita*, several of *Glaucus atlanticus*, some shells of *Spirula* bearing groups of small stalked cirripeds, and many large radiolarians. One of the Spirula shells was covered with a beautiful stalked infusorian.

We proceeded in the evening under all plain sail. The soundings on the chart in advance of us seemed to indicate an extensive rise, with a depth of water averaging not much more than 1,700 fathoms, and it was determined to dredge again on the following day.

On the morning of March 4 we sounded in lat. 21° 38′ N., long. 44° 39′ W., in 1,900 fathoms, with No. 1 line, the "Hydra" and 3 cwt., the slip water-drop, and a thermometer. The bottom was grey ooze, as on the day before, and the bottom temperature 1°·9 C. The dredge was put over at 8 A.M. It was intended to attach a "Hydra" tube with disengaging weight a little below the bottom of the dredge; the weight slipped, however, close to the surface, and the dredge was lowered in the ordinary way with 1½ cwt. 500 fathoms in advance. The dredge came up about 4 o'clock with a small quantity of ooze containing some red clay, a large proportion of calcareous débris, and many foraminifera, chiefly *Orbulina* and *Rotalia*.

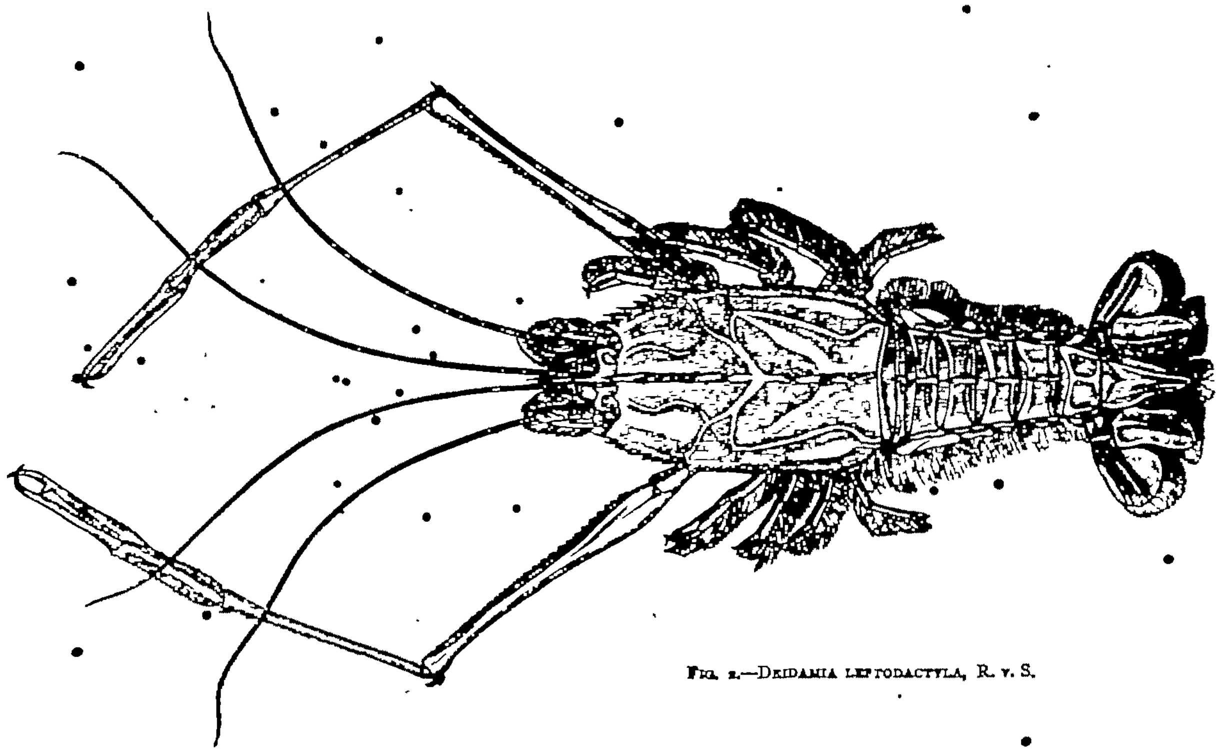

FIG. 2.—DEIDAMIA LEPTODACTYLA, R. v. S.

Warped in the hempen tangle there was a fine specimen of a handsome decapod crustacean, having all the principal characters of the family Astacidæ, but differing from all the typical decapods in the total absence of eye-stalks and eyes. Dr. v. Willemoes-Suhm has given this interesting deep-sea form such a preliminary examination as is possible in the absence of books of reference. I quote from his notes. *Deidamia leptodactyla*, n.g. and sp. (Fig. 2). The specimen, which is a male, is 120 mm. in total length and 33 mm. in width across the base of the cephalo-thorax, which is 60 mm. in length. Three rows of spines, one in the middle line and one on each side, run along the cephalo-thorax, which is divided by a transverse sulcus into an anterior and a posterior part, the former occupied by a central gastric and lateral hepatic regions, and the latter by a central cardiac and latent bronchial regions. The abdomen, which consists as usual of seven segments, has the central series of spines of the cephalo-thorax continued along the middle line. The sixth segment bears the caudal appendages, and in the seventh, the telson, we find the excretory opening. The lateral borders of the body, and all the appendages with the exception of the first pair of ambulatory legs, are edged with a close and very beautiful fringe of a whitish-yellow colour.

There are two pairs, the normal number, of antennæ, then come mandibles, then maxillæ; three pairs of maxillipeds, five pairs of ambulatory legs, and five pairs of swimmerets. As most of the appendages differ from those usually met with in the Astacidæ only in detail, I need here only mention that the anterior antennæ have two pairs of flagella, one of which is very long, longer than the external flagellum of the external pair.

The form of the first pair of ambulatory legs is singularly elegant. They are 155 mm. in length—considerably longer than the body; they are very slender, and end in a pair of very slender denticulated chelæ, with a close, velvet-like line of hairs along their inner edges. The rest of the ambulatory legs are much shorter, and all bear chelæ, a character which will demand a certain relaxation of the diagnosis of the Astacidæ if *Deidamia* is to be placed in that family.

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* Continued from p. 30.

The specimen captured being a male, the first pair of swimmerets are somewhat modified. The four other pairs of swimmerets, which are 33 mm. in length, bear each two narrow swimming processes richly fringed with hair, and a short flagellum.

The absence of eyes in many deep-sea animals and their full development in others is very remarkable. I have mentioned ("The Depths of the Sea," p. 176), the case of one of the stalk-eyed crustaceans, *Ethusa granulata*, in which well-developed eyes are present in examples from shallow water. In deeper water, from 110 to 370 fathoms, eye-stalks are present, but the animal is apparently blind, the eyes being replaced by rounded calcareous terminations to the stalks. In examples from 500 to 700 fathoms in another locality, the eye-stalks have lost their special character, have become fixed, and their terminations combine into a strong pointed rostrum. In this case we have a gradual modification, depending apparently upon the gradual diminution and final disappearance of solar light. On the other hand, *Munida*, from equal depths, has its eyes unusually developed and apparently of great delicacy. Is it possible that in certain cases, as the sun's light diminishes, the power of vision becomes more acute, while at length the eye becomes susceptible of the stimulus of the fainter light of phosphorescence? The absence of eyes is not unknown among the Astacidæ. *Astacus pellucidus*, from the Mammoth Cave, is blind, and from the same cause—the absence of light; but morphologically the eyes are not entirely wanting, for two small abortive eye-stalks still remain in the position in which eyes are developed in all normal decapods. In *Deidamia* no trace whatever remains either of the eyes of sight or of their pedicels.

On Thursday the 6th we sounded in 2,325 fathoms, sending down a thermometer and the slip water-bottle. The temperature registered was 1°·7 C., and the specific gravity of the sample of water was 1·02470 at 21° C., that of the surface water being 1·02556, at 23°·3 C.

A good deal of gulf-weed drifted past during the day, and a boat was sent out to collect some. About half a dozen closely twined bundles were procured, and on examining them it was found that the bundle was bound together by strings of the viscid secretion of *Antennarius marmoratus*, and formed a nest containing the eggs of the fish. Several young examples of this grotesque little animal have been from time to time brought in among the gulf-weed; also many crustaceans, several of the nudibranchiate mollusca characteristic of the gulf-weed fauna, such as *Stillæa pelagica* p., and many planarians.

The dredge came up at 4.15 P.M. with a small quantity of red mud, in which we detected only one single but perfectly fresh valve of a small lamelli-branchiate mollusk. In the mud there were also some sharks' teeth of at least two genera, and a number of very peculiar black oval bodies about an inch long, with the surface irregularly reticulated, and within; the reticulates closely and symmetrically granulated the whole appearance singularly like that of the phosphatic concretions which are so abundant in the greensand and trias. My first impression was that both the teeth and the concretions were drifted fossils, but on handing over a portion of one of the latter to Mr. Buchanan for examination, he found that it consisted of almost pure peroxide of manganese.

The character both of the exterior and interior of the nodule strongly recalled the black base of the coral which we dredged in 1,530 fathoms on the 18th of February; and on going into the matter, Mr. Buchanan found not only that the base of the coral retaining its external organic form had the composition of a lump of pyrolusite, but that the glossy black film covering the stem and branches of the coral gave also the reaction of manganese. There seemed to be little doubt that it was a case of slow substitution, for the mass of peroxide of manganese forming the root showed on fracture in some places the concentric layers and intimate structure of the original coral. The coral, where it was unaltered, had the ordinary composition, consisting chiefly of calcic carbonate. Whether the nodules dredged on March 7th are pieces of rolled coral, the ornament on their surface being due to an imperfect crystallisation of the surface layer of the peroxide of manganese, or whether they form another case of pseudomorphy, the peroxide of manganese replacing some other organism, we have not the means of determining. The whole question is a very singular one.

Some of our party, using the towing-net and collecting gulf-weed on the surface from a boat, brought in a number of things beautiful in their form and brilliancy of colouring, and many of them strangely interesting for the way in which their glassy transparency exposed the working of the most subtle parts of their internal machinery; and these gave employment to the microscopists in the dearth of returns from the dredge. Our position was now lat. 19° 57′ N., long. 53° 26′; Sombrero distant 558 miles.

Sunday was a lovely day. The breeze had fallen off somewhat, and the force was now only from 2 to 3. The sky and sea were gloriously blue, with here and there a soft grey tress on the sky, and a gleaming white curl on the sea. A pretty little Spanish brigantine, bright with green paint and white sails, and the merry, dusky faces of three or four Spanish girls, came in the morning within speaking distance and got her longitude. She had been passing and repassing us for a couple of days, wondering doubtless at the irrelevancy of our movements, shortening sail, and stopping every now and then in mid ocean with a fine breeze in our favour. On Monday morning we parted from our gay little companion. We stopped again to dredge, and she got far before us, and we saw with some regret first her green hull and then her white sails pass down over the edge of the world.

The sounding on Monday the 10th gave 2,675 fathoms, with a bottom of the same red clay with very little calcareous matter. The bottom temperature was 1°·6 C., that of the surface being 23°·3 C. We had been struck for some time past with the singular absence of the higher forms of life. Not a bird was to be seen from morning to night. A few kittiwakes (*Larus tridactylus*) followed the ship for the first few days after we left Teneriffe, but even these had disappeared. A single petrel (*Thalassidroma pelagica*) was seen one day from one of the boats on a towing-net excursion, but we had not seen one of the southern sea-birds. For the last day or two some of the larger sea-mammals and fishes had been visible. A large grampus (*Orca gladiator*) had been moving round the ship and apparently keeping up with it. Some sharks hung about, seeking what they might devour, but we had not yet succeeded in catching any of them. Lovely dolphins (*Coryphæna hippurus*) passed in their varying irridescent colouring from the shadow of the ship into the sunshine, and glided about like living patches of rainbow. Flying-fish became more abundant, evidently falling a prey to the dolphins, which are readily deceived by a rude imitation of one of them, a white spinning bait, when the ship is going rapidly through the water.

On Tuesday the 11th we pursued our course during the forenoon at the rate of from six to seven knots, with a light breeze, force 3 to 4. The dredge-line was veered to over 4,000 fathoms, nearly 5 statute miles. The dredge came up at about half-past five o'clock, full of red mud of the same character as that brought up by the sounding machine. Entangled about the mouth of the dredge and embedded in the mud were many long cases of a tube-building annelid, evidently formed out of the gritty matter which occurs, though sparingly, in the clay. The tubes with their contents were handed over to Dr. v. Willemoes-Suhm, who found the worms to belong to the family Ammocharidæ (Claparéde and Malmgren), closely allied to the Maldania or Clymenidæ, all of which build tubes of sand or mud. The largest specimens dredged are 120 mm. in length by 2 mm. in width. The head is rounded, with a lateral mouth. There is no trace of cephalic branchiæ. The segments are not divided from one another; but the *tori uncinigeri*, which are occupied by the hair-like setæ, and the elevations bearing small *uncini*, indicate the beginning of a new segment.

There is no doubt that this annelid is closely allied to the genus *Owenia*, but it differs from it in the absence of cephalic branchiæ. Malmgren, has, however, already proposed the name of *Myriochele* for a form in which this absence of branchiæ occurs. The description of the northern form on which Malmgren's genus is founded is not at hand, so that it is impossible in the meantime to determine whether the two forms are identical or specifically distinct.

As bearing upon some of the most important of the broad questions which it is our great object to solve, I do not see that any capture which we could have made could have been more important and more conclusive than that of this annelid. The depth was 2,975, practically 3,000, fathoms—a depth which does not appear to be greatly exceeded in any part of the ocean. The nature of the bottom, which consists of a smooth red clay with a few scattered sand grains and a very small number of foraminifera shells, was very unfavourable to higher animal life, and yet this creature, which is closely related to the Clymenidæ, a well-known shallow-water group of high organisation, is abundant and fully developed.[*] It is fortunate in possessing such attributes as to make it impossible even to suppose that it may have been taken during the passage of the dredge to the surface, or have entered the dredge-bag in any other illegitimate way; and its physiognomy and habits are the same as those of allied forms from moderate depths. It affords, in fact, conclusive proof that the conditions of the bottom of the sea to all depths are not only such as to admit of the existence of animal life, but are such as to allow of the unlimited extension of the distribution of animals high in the zoological series, and closely in relation with the characteristic faunæ of shallower zones.

On Thursday the 13th our position at noon was lat. 18° 54′ N., long. 61° 28′ W.

On the forenoon of the 14th we were still 35 miles from land, and we sounded in 1,420 fathoms. The bottom had altered greatly in character: it now consisted chiefly of calcareous foraminifera of many species, mixed with a considerable portion of the broken spicules of siliceous sponges. The bottom temperature registered was 3° C. The water-bottle was accidentally broken in taking in, so that that observation was lost. As we were now within sight of land, and all our results were evidently modified by its immediate proximity, we regarded our first deep-sea section as completed.

WYVILLE THOMSON

A MODERN STERNBERGIA

At a time when botanists of some repute are not ashamed to confess their inability to deduce satisfactory characters for the determination of plants from their internal anatomy, old workers in this field may well turn back to refresh their memories on such points, and to inquire whether their eyes may not have deceived them in the investigations of former years when microscopes were not what they now are. In doing this a few days ago in connection with the examination of a carboniferous conifer, I was surprised to find that I had overlooked or omitted to note the fact that the Balsam Fir of Canada (*Abies balsamea*), which affords the well-known Canada-balsam, has that curious structure of pith well known in Palæozoic Conifers, and which has been named *Sternbergia*. It is well seen in young twigs one or two years old, and though on a smaller scale, is very similar to that of *Dadoxylon materiarium* of the upper coal-formation of Nova Scotia and Prince Edward Island, as I have figured this in my recent report on the geology of the latter province.

This modern Sternbergia is not produced by the mere breaking of the cellular tissue transversely by elongation of the fibre; but, as I pointed out many years ago in the case of the coal-formation Sternbergiæ,[*] is a true organic partitioning of the pith by diaphragms of denser cells opposite the nodes, as in *Cecropia pellata*, and some species of *Ficus*, &c. The pith of the Balsam Fir is, like that of many other conifers, composed of dotted or transversely marked cells elongated vertically, and reminding one of the pseudo-vascular pith of some Lepidodendroid trees. The transverse diaphragms are composed of denser cells flattened horizontally, and they are, as in Sternbergia, accompanied by constrictions of the medullary cylinder. As in some fossil conifers, the diaphragms are not perfectly continuous.

The plan of growth of the modern fir does not permit its pith to increase in diameter. This was different in the Palæozoic conifers, in which the Sternbergia pith is sometimes nearly two inches in diameter.

In Palæozoic, as in modern times, Sternbergia piths were not confined to one family of trees. Corda has shown this structure in *Lomatophloios*, which is equivalent to *Lepidophloios* or *Ulodendron*. I have shown that it exists in several species of Lepidodendroid and Sigillaroid trees and in *Leptophleum*.[†] Williamson, who first established it in the Conifers, has also found it in *Dictyoxylon*. Still I have nowhere found these remarkable fossils so abundant as in the upper coal-formation, and either in the interior of calcified or silicified trunks of pine or with fragments of wood attached to them sufficient to indicate their coniferous character.

I may add, that the microscopic structure of young twigs of modern conifers presents many interesting points for comparison with fossil trees, and that in making longitudinal slices of the pith of recent specimens care should be taken not to be misled by the mere crumpling of the cellular tissue sometimes caused by the pressure of the knife.

J. W. DAWSON

NOTES

PROFESSOR CARUS, the well-known naturalist of Leipsic University, who is to fill Professor Wyville Thomson's chair during the absence of the latter with the *Challenger*, commenced his duties on May 2 last, by an able and eloquent address on the study of zoology. He is fully convinced that "the final form of our (zoological) system will be a pedigree."

THE *Challenger* arrived at Halifax on May 9, all well. She had a successful passage from Bermuda, the dredgings and soundings being very satisfactory. On the 18th inst. she will leave this port on a return voyage to Bermuda.

WITH great regret we record the death of Mr. John Stuart Mill, at the age of 67 years, on May 8, at Avignon, from a sudden attack of erysipelas, which cut him off in four days. He has been buried beside his wife at Avignon. A meeting of the friends of Mr. Mill has been convened, at Willis's Rooms, for Tuesday, 20th inst., to consider in what manner the national respect for his memory may be most fittingly testified.

A COMMITTEE for the erection of a monument to Liebig has been constituted at Munich. Councillor von Niethammer is the chairman, Prof. Von Bischoff the vice-chairman, and Professors

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[*] Canadian Naturalist and Geologist, 1857.

[†] Journal of the Geological Society, May 187

Vollhard and Von Jolly are the secretaries. The King of Bavaria has subscribed 1,000 florins.

THE purchase for the National collection, by the Trustees of the British Museum, of Mr. A. R. Wallace's splendid collection of birds from the Malay Archipelago, will be gratifying to all who are interested in science. Mr. Wallace being so thoroughly acquainted with ornithology, and having obtained so many of the specimens himself from localities recorded by himself at the time, makes the collection much more valuable than the skins alone would have been, if they had been accumulated by a less thorough master of the subject. That such is the case, is proved by the great value of Mr. Wallace's paper on the Parrots of the Malay Archipelago, which appeared in the Proceedings of the Zoological Society, nearly ten years ago; and another on the Pigeons of the same region, published in the *Ibis*, at about the same time. It is also not to be forgotten, that the discovery of one of the most important of recent points in physical geography, namely, the situation of the line which separates Asia from Australasia, in other words, *Wallace's line*, was made in great measure from the observations by the author,—whose name is thus deservedly immortalised,—of the differences in the avifaunas of Bali and Lombock.

THOSE of our readers who are interested in University science teaching will be glad to learn that Dr. Michael Foster's course of Elementary Biology at Cambridge, which commenced last week, is attended by more than 30 students. This unexpectedly large attendance has taxed to the utmost the space at disposal. However, such arrangements have been made as will enable every student to have a fair though not large amount of space at his disposal, each set of reagents, &c., being used in common by two or three men. Nothing could illustrate more strongly the urgent need for further provision of working-room for biological students at Cambridge; as scarcely any space is now available for advanced histological, embryological, or physiological research. Dr. Foster's course this term is very similar to that given to science teachers in the summer at South Kensington, and is the first that has been held in term-time at Cambridge, a few students having gone through a like course last long vacation. It is probable that there may be a still larger attendance at future courses of this kind, as Dr. Foster announced that he should require students to have received this or similar teaching before admission to the winter courses of practical physiology. Dr. Foster is assisted in the work of practical demonstration by Mr. H. N. Martin, D.Sc., M.B. of Christ's College, Mr. C. Yule, B.A. of St. John's College, and Mr. T. W. Bridge, of Trinity College, the newly-appointed Demonstrator of Comparative Anatomy.

MR. JOHN ARROWSMITH, the well-known geographer, died on May 2, at the age of eighty-three years.

A GENTLEMAN writes us that he was invited by the Royal Commissioners to act as a juror at the Vienna Exhibition, but was at the same time coolly told that our Philistian Government had placed no funds at the disposal of the Commissioners wherewith to defray the necessary expenses of those who are willing to devote their valuable time and experience to the service of their country. Our readers will not be surprised at this. Other Governments have discovered that even in the most commercial, as well as in the highest light, the encouragement of science "pays." The British Government, with five millions on the right side of their account, still regard science as a beggarly Lazarus, to whom, for mere shame's sake, they are compelled to throw an occasional crumb. As our correspondent says, poor little Switzerland has devoted two and a half times the pittance our Government have allowed to defray the expenses of the Vienna Commission; while the amount expended by Austria in their department of former exhibitions was at least four times as much as we have devoted to theirs.

CAPT. F. J. OWEN EVANS, R.N., F.R.S. Chief Naval Assistant in the Hydrographic Department of the Admiralty, and in charge of the Magnetic Department, has been appointed Companion of the Most Honourable Order of the Bath.

THE publication of the eighth volume of the *Zoological Record* which, as we announced some time since, has been so long delayed through the unfortunate indisposition of one of the contributors may now be shortly expected. The ninth volume containing the zoological literature of 1872 is now in hand, the recorders being the same as in the eighth volume, with the exception of Prof. Traquair, whose place is supplied by Prof. Lutken of Copenhagen. The Editor will be glad to receive separate copies of papers published in journals (especially those which have not a very wide circulation) addressed to the care of the publisher, Mr. Van Voorst, 1, Paternoster Row, London. Such separate copies, however, to be of use, should have the original pagination indicated.

THE Society of Antiquaries of Scotland has just come into the enjoyment of an estate in Caithness, of which the reversionary interest was bequeathed to it for the purpose of founding a Lectureship of Archæology.

MR. BESSEMER intends to found a gold medal, to be given annually to any member of the Iron and Steel Institute who may have displayed literary capacity, or promoted the progress of metallurgical science by original research.

PROFESSOR NEWCOMB'S "New Tables of the Motions of Uranus," are announced as already in the press, and may be expected to be published during the approaching summer. They have been prepared and will be printed at the expense of the Smithsonian Institution. Prof. Newcomb has already, by using all known observations of Neptune, compiled the very accurate tables for computing the motions of that planet that have been used in the "American Nautical Almanac." Having thus provided for the most distant member of our system, he has now returned to Uranus, and finds that his present tables (which will complete the survey of the solar system) represent quite completely the hitherto inexplicable movements of that body.

THE Cincinnati Observatory, founded by Prof. Mitchell, is, we learn, to be removed, and established in a manner worthy of the wealth of Cincinnati. From the drawings it may be judged that the dome of the new building will be thirty-five feet in diameter in the inside. The new site was highly approved of by Prof. Abbe, who continued until lately to be the director of the observatory at Cincinnati, and was presented by John Kilgour, Esq., who also added thereto the sum of ten thousand dollars to provide for the new building.

AMONG the resolutions adopted by Congress at its last session was one authorising the President to invite the International Statistical Congress to hold its next, or ninth, session in the United States. The invitation is to be formal and cordial, and it is provided that should this be accepted the President is authorised to appoint the usual organisation commission, and to take the other preliminary and necessary steps for the meeting of this body, and for holding its session at such time as may be deemed expedient by the Statistical Congress.

A TELEGRAM announces that some of the crew of the Arctic exploring ship *Polaris*, which left New York under the command of Captain Hall in 1871, have been landed in Newfoundland. They were picked up in an open boat 40 miles from the coast of Labrador. It seems, by their statements, that in August 1872, the ship, being beset with ice, commenced landing provisions, Suddenly the ice broke, and the men who were upon it were carried away. They drifted southward for 196 days—more than six months—and the ice, which originally was five miles in circumference, was gradually reduced to a few feet. They then took to the only remaining boat. Captain Hall, they report, died of apoplexy in November 1871. These statements have been received with distrust.

MR. LAMONT'S beautiful steam yacht *Diana*, which has been chartered by Mr. Benjamin Smith, of London, for a voyage of exploration in the Northern Seas, left Dundee on Saturday. The yacht is manned by a crew of twenty, and although there is a sailing master, Mr. Smith will have complete control. The first point of rendezvous will be Cobbe's Bay, on the north-west of Spitzbergen, where Mr. Smith expects to meet his own sailing yacht, the *Samson*, which was despatched from Hull with stores on May 1 under the command of Captain Walker, for many years connected with the Dundee whaling fleet. Every effort will subsequently be made to push as far northward as possible. During the voyage marine and land plants will be gathered and observations of the tides and currents made. The *Diana* is provisioned for a year, but the object contemplated is expected to be realised in about six months.

AT the recent meeting of the Delegates of the French Learned Societies, gold medals were awarded to the following:—M. Leymerie, for his geological studies in the Pyrenees; M. Blecher, military surgeon, for his interesting geological observations on the central plateau of France and the environs of Montpelier; M. Guillier, for his researches on the geology and industrial products of the department of Sarthe; M. Pomel, for his investigations on the geology of the Sahara; M. Sirodot, for his work on the algæ (*Lemanea*), which grow in fresh running water. Silver medals were awarded to M. Cauvet for various observations on vegetable anatomy and physiology; to M. Verlot, for his catalogue of the vascular plants of Dauphiny; M. Gassies, for his investigations on the terrestrial and river shells of New Caledonia; to M. Villot, for his observations on the curious metamorphoses and strange migrations of certain worms found in wells and in standing water.

IN 1859, an attempt was made to start a Zoological Garden in Philadelphia, which fell to the ground during the subsequent war. A fresh company is now being formed to carry out the original intention, though on a larger scale. A site has been secured in Fairmount Park, and capital is to be obtained in the following manner:—Certificates of stock are to be issued of not less than fifty dollars each. All receipts derived from the Gardens and collections of the Society, are to be applied annually—first, to the maintenance of the establishment; secondly, to the payment of six per cent. on the stock; and third, any balance remaining to go to the gradual extention of the collection of the Society and the improvement of its grounds. Many influential citizens are supporting the project.

THE Annual Report of the Visitors of the Royal Institution shows a considerable increase in the number of members, and is otherwise very satisfactory.

THE Rev. Thomas Fowler, M.A., Fellow, Sub-Rector, and Tutor of Lincoln College, has been elected to the Professorship of Logic at Oxford, vacant by the death of Prof. Wall.

MR. HYDE CLARKE will on Tuesday, the 20th instant, read a paper at the Anthropological Institute, on "The Egyptian Colony and Language in the Caucasus."

THE Royal Cornwall Polytechnic Society, has published its list of prizes for 1873. The largest sums, varying from ten guineas to one guinea, are offered for improvements in mine ventilation, mining, boring machinery, and similar departments. Small premiums are offered for essays, local observations, and collections of Natural History, especially such as illustrate the Natural History of the county.

WE have received the "Report on the Condition of the Sea Fisheries of the South Coast of New England in 1871-2," by Prof. S. F. Baird. As the result of a thorough investigation, Prof. Baird comes to the conclusion that during the last few years there has been a decided decrease in the number of food-fishes in these waters: the decrease being mainly due to the combined action of the fish-ponds or weirs and the blue-fish, the former destroying a large percentage of the spawning fish before they have deposited their eggs, and the latter devouring immense numbers of young fish after they have passed the ordinary perils of immaturity.

FROM the "Report of the Commissioners of Fisheries of the State of New York," we learn that the rivers of that State are being plentifully stocked with useful fish, especially shad; and the Commissioners are confident that the people of the United States will in a short time rely upon restocking their waters, and not upon game laws, to keep up a full supply of fish for their markets.

WE have received the first two parts of Mr. Tegetmeier's magnificent work on "Pheasants for the Covert and the Aviary" We shall notice it fully when completed.

THE much-vexed question as to whether seals are fish or not, as regards the oil to be obtained from them, has recently come up in a practical shape between the governments of the United States and Newfoundland. The fishery treaty lately entered into between the United States and Great Britain, and about to go into actual operation in the course of the present summer, provides that *fish* oil shall be admitted free, but that other oils shall pay a duty of ten per cent. This question is one that would be very easy of solution if it were purely zoological in its character, since, as every one does or should know, the seal and porpoise, as well as the whale, are warm-blooded mammalia, having nothing in common with the fish any more than has the man who, for the time being, goes into the water for the purpose of bathing. It appears, however, to be the general practice with commercial nations to class all oils obtained from marine objects, whether cetaceans, birds, or fishes, as fish oil, and on this ground it is probable that the claim of the Newfoundland authorities to have seals recognised as fish will be accepted.

THE following additions to the Brighton Aquarium have been made during the past week:—Two young Seals (*Phoca vitulina*) from Jan-Mayen Island, Arctic Sea, presented by Mr. John Clark; two Porpoises (*Phocæna communis*), from Rye Bay, purchased; one Angler (*Lophius piscatorius*), from Cornwall; Bass (*Labrax lupus*); Gurnards (*Trigla lineata*); Grey Mullet (*Mugil capito*); Conger-eels (*Conger vulgaris*); Sand Smelts (*Atherina Presbyter*); Pollack Whiting (*Gadus pollachius*); Rocklings (*Motella mustela*); Sand Launce (*Ammodytes lancea*); one Octopus (*Octopus vulgaris*), from the French coast; two cuttle-fish (*Sepia officinalis*); Prawns (*Palæmon serratus*); Foliaceous Coralline (*Eschara foliacea*); Anemones numerous.

THE additions to the Zoological Society's Gardens during the past week include a Chinese Water Deer (*Hydropotes inermis*), from China, presented by Mr. R. Swinhoe; four Peafowl (*Pavo cristatus*), from India, presented by Mrs. Stern; two Koodoos (*Strepsiceros kudu*), and a Pluto Monkey (*Cercopithecus pluto*), from Africa; a Weeper Capuchin (*Cebus capucinus*), from South America, deposited; three Cole Tits (*Parus ater*), from the British Isles; four Spix's Cavies (*Cavia spixii*), from Brazil; a tawny Eagle (*Aquila nævioides*), from Africa, purchased; a Markhoor (*Capra megaceros*); five Peacock Pheasants (*Polyplectron chinquis*); and five Chilian Pintails (*Dafila spinicauda*), born in the Gardens.

THE BIRTH OF CHEMISTRY

X.

The Theory of Phlogiston—Comparison with Hooke's Theory of Combustion.—Early Ideas regarding Calcination.—Stephen Hales—His Pneumatic Experiments.—Boerhaave.—Conclusion.

ABOUT the year 1669 we find the first dawnings of a theory which was proposed in order to connect together various chemical phenomena, and notably for the explanation of combustion, the common and most obvious of all chemical actions. This theory, known as the "Theory of Phlogiston," powerfully influenced chemistry for a century; indeed upon its ruins the structure of modern chemistry was raised by the labours of Lavoisier, Priestley, and Scheele. The proposers of this theory—John Joachim Becher (b. 1625, d. 1682) and George Ernest Stahl (b. 1660, d. 1734) endeavoured to trace the cause of various phenomena of chemical change to the assimilation or rejection of what they called "*materia aut principium ignis, non ipse ignis*"—not actual fire, but the principle of fire; a something not much unlike the pure, elemental, celestial fire which a few ancient and many Middle Age writers had feigned to exist. Stahl believed this *materia ignis* to be a very subtle, invisible, substance, which neither burns nor glows; its particles penetrate the most dense substances, and are agitated by a very rapid motion. When a body is burned it loses phlogiston; when a body is un-burned, if we may use such an expression, or de-oxidised, it assimilates phlogiston (φλογιστὸς, burnt). Thus if lead is heated for some length of time it is converted into a powdery substance which they called *calx of lead*, and we, *lead oxide*; the lead has lost Phlogiston, said Stahl. On the other hand, if this same calx of lead is heated with red-hot charcoal, it is deoxidised and becomes lead again. It has now assimilated the Phlogiston, which it had before lost.

But here arose a difficulty. A metal was found to be heavier after calcination than before; thus loss of Phlogiston lead to gain of weight, which was altogether anomalous, and apparently incapable of explanation. But the Phlogistians were equal to the occasion; the supporters of a pet theory will create any number of the most vague and impossible hypotheses, rather than yield up their darling to destruction: so, said they, Phlogiston is a principle of levity; it confers negative weight; it makes bodies lighter, just as bladders attached to a swimmer lighten him.

The theory was applied as generally as possible:—thus sulphuric acid is produced by burning sulphur under certain conditions of oxidation; the sulphur loses Phlogiston, and becomes heavier like the metallic calx; hence sulphuric acid is sulphur *minus* Phlogiston, while sulphur is consequently sulphuric acid *plus* Phlogiston. In fact *loss of phlogiston* was synonymous with what we call *oxidation*; and *gain of phlogiston* with *deoxidation*. The existence of Phlogiston was so utterly unsupported by experimental proof that the theory could scarcely exist without many opponents. The endurance of the most false chimerical theory is often really wonderful. The Phlogistians were attacked first in one direction, then in another, yet the theory continued to find supporters. At last, as a last resource, hydrogen gas—recently investigated by Cavendish—was said to be Phlogiston, but this was so entirely different from the Phlogiston of Stahl that the theory was now seen on all sides to be fast giving way. At length Lavoisier, a century ago, conclusively disproved the theory by means which cannot be discussed here, because they belong to the more advanced history of the science.

How the crude, unscientific, illogical theory of Phlogiston could have arisen in the face of Hooke's admirable theory of combustion, and Mayow's experiments in support of it, must always remain a mystery. It is probable that if Mayow had not died a young man, or if Hooke had found leisure to prosecute his views, the theory of Phlogiston would never have been propounded. The theory has been much over-praised. The only service which it rendered to the science was that it introduced a certain amount of order and system, which was hitherto wanting. It led to the grouping together of certain classes of facts, and, to a slight extent, to the application of similar modes of reasoning to similar chemical phenomena. And although that reasoning was altogether wrong, it seemed to indicate the means by which, with a more perfect and advanced system, chemistry might become an exact science subject to definite modes of treatment.

We have more than once spoken of calcination, which was indeed one of the most prominent operations of old chemistry. Since the examination of the process led to the proposal of just ideas concerning the materiality of the air—most often denied by ancient and middle-age writers—it may be well to glance at the early ideas regarding calcination. Here then was the dominant experiment in this direction: I take a bright lustrous metal, tin or lead, melt it, keep it in a molten state for awhile, and it is converted into powder, which weighs more than the original metal. Again I heat this same powder with charcoal, and it becomes metal again; yet nothing that can be seen has been added to the metal, or taken away from its calx. Geber defines calcination as "the pulverisation of a thing by fire, by depriving it of the humidity which consolidates its parts." He observed that the metal increases in weight during the operation, although "deprived of its humidity." Cardanus asserted that the increase of weight in the case of lead amounted to one-thirteenth the weight of the metal calcined; and he accounted for it on the supposition that all things possess a certain kind of life, a *celestial heat*, which is destroyed during calcination; hence they become heavier for the same reason that animals are heavier after death, for the celestial heat tends upwards. This idea was almost similar to that of the Phlogistians, although published more than a century before Becher wrote his *Physica Subterranea*. In 1629 Jean Rey, a physician of Bergerac, attempted to discover the cause of increase, and attributed it to the absorption of "thickened air" (*l'air espessi*) by the metal during calcination. Lemery, as we have seen, attributed the gain to the absorption of *corpuscules de feu*. Afterwards came the nitre-air of Mayow, then a century later the increase was proved to be due to the union of the body with a constituent of the air which Lavoisier named oxygen gas; and this gas was first discovered by heating one of the calces (calx of mercury), about which so much speculation had been wasted, and so little experiment bestowed, by earlier writers.

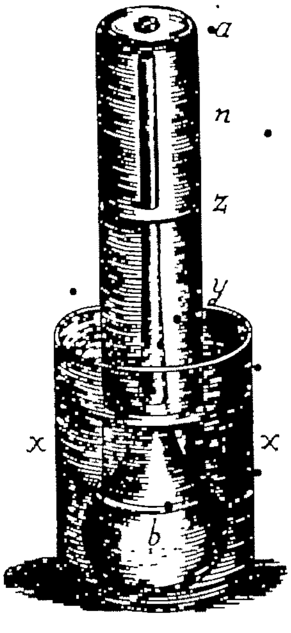


FIG. 21.

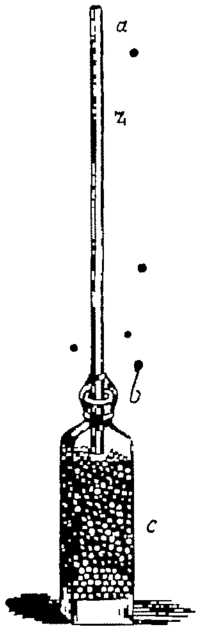


FIG. 22.

FIG. 21.—Hales' method of measuring a gas. FIG. 22.—Measurement of the elastic force of the gas produced by fermenting peas.

We are drawing towards the end of our subject, but we think any account of the earlier history of chemistry would be very incomplete without a notice of the work of Dr. Stephen Hales (born 1677, died 1761). In a number of papers communicated to the Royal Society, and afterwards published in a work entitled *Statical Essays*, we find a variety of experiments by Hales, chiefly relating to pneumatic chemistry. Herein we find an account of "a specimen of an attempt to analyse the air by a great variety of chymico-statical experiments, which show in how great a proportion air is wrought into the composition of animal, vegetable, and mineral substances, and withal how readily it resumes its former elastick state, when in the dissolution of those substances it is disengaged from them." In order to determine the quantity of air disengaged from any substance during distillation or fusion, Hales placed the substance in a retort, and luted the retort to a large receiver with a small hole, at the bottom; water was caused to occupy a known space in the receiver, and the amount of air expelled was estimated by noting the amount of water remaining in the receiver at the conclusion of the experiment, after cooling. Hales employed the following apparatus (Fig. 21) to measure the volume of air generated by any kind of fermentation, also by the reaction of one body upon another.

The substances undergoing fermentation were placed in *b*, and over the whole a vessel, *a y*, was inverted, closed below by the vessel *x x*, and containing above a certain amount of air, to the level *y*. If air were generated, the water in *a* sank (say to *y*); while if air were absorbed by the bodies in *b*, the water rose (say to *n*). Sometimes he placed different substances on pedestals in a jar of air, and ignited them, as Mayow had done, by a burning-glass, and noted the alteration in the bulk of air. He did this with phosphorus, brown paper dipped in nitre, sulphur, and other substances. If he required to act upon substances by means of a strong acid, he would place the substance in a suitable vessel on a pedestal in a known volume of air, standing over water, and would suspend over it a phial which could be emptied by pulling a string. These devices were closely copied by Priestley and Lavoisier in their experiments upon gaseous bodies. If a substance required to be heated violently, it was placed in a bent gun-barrel, *r r* (Fig. 23), one end of which was placed in a furnace, while the other was placed under a bell jar, *a b*, full of water, inserted in the pail of water *x x*. He distilled a number of substances, apparently taken at random, and determined the amount of gas evolved, but he appears to have been at no pains to determine the nature of the gas, assuming it to be ordinary atmospheric air. Thus he distilled 1 cubic inch of lard, and collected thirty-three cubic inches of gas as the products of decomposition. Tallow, horn, sal ammoniac, oyster shells, peas, amber, camphire, and many other substances, were similarly treated.

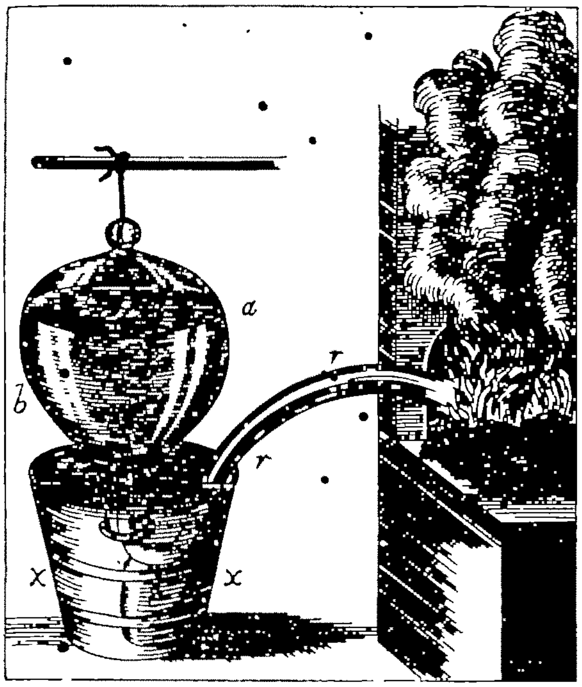


Fig. 23.—Hales' pneumatic experiments.

Two grains of phosphorus ignited in a closed vessel of air, were found to absorb 28 cubic inches of air. 211 grains of nitre mixed with bone-ash yielded 90 cubic inches of gas; 54 cubic inches of water on boiling yielded 1 cubic inch of air. In order to measure the elastic force of the gas produced by fermenting peas, Hales filled a small, strong bottle, *c* (Fig. 22) with peas, filling up the interstices with water; mercury to a depth of half an inch was then poured in, and of course remained at the bottom of the vessel *c*. A long tube, *a s*, the lower end of which dipped beneath the mercury, was securely fastened into the mouth of the bottle *b*, and fixed air-tight. In a few days' time the peas were in a state of fermentation, and the generated gas had forced the mercury to ascend in the tube *a s* to a height of 80 inches, hence the gas in *c* was existing under a pressure of about 35 lbs. on the square inch.

Hales also produced gases by various reactions. Thus he poured a cubic inch of sulphuric acid on half a cubic inch of iron filings: no effect took place until he had diluted the acid with water, when forty-three cubic inches of *air* (as he calls it—in reality hydrogen gas) came off. Iron filings mixed with nitric acid, or with ammonia, or sulphur, were found to absorb air. A cubic inch of chalk treated with dilute sulphuric acid produced thirty-one cubic inches of *air* (in reality carbonic anhydride gas). If space permitted, we could say much more of Hales' works. His experiments on respiration, and on various principles of vegetation, are exceedingly ingenious, and often accurate. It has often been said that Lavoisier created modern chemistry by the introduction of the balance into chemical experiments, but here we find Hales weighing his substances, and measuring his gases, years before Lavoisier was born. Hales did not sufficiently investigate the nature of the various gases which he produced in the course of his experiments, but he assuredly paved the way for many of the after discoveries of Priestley, Cavendish, and Lavoisier.

Dr Hermann Boerhaave, of Leyden (b. 1668, d. 1738), was a contemporary of Hales. He was the author of the first comprehensive system of chemistry:—a bulky quarto in two volumes, entitled *Elementa Chemiæ*, which appeared in 1732, and which for many years was the chemical text-book of Europe. In it he defines chemistry as "an art which teaches the manner of performing certain physical operations, whereby bodies cognizable to the senses, or capable of being rendered cognizable, and of being contained in vessels, are so changed by means of proper instruments, as to produce certain determinate effects, and at the same time discover the causes thereof for the service of various arts."

But hold! our task was to give some account of the *birth* of chemistry, while a science with such a ponderous definition as the above, is no longer infantile. The babe has grown up about us until it has assumed a tremendous individuality. The great discoveries of the fathers of modern chemistry, Lavoisier, Scheele, Priestley, Cavendish, Davy, need not be told here; they belong to the later history of chemistry. We have traced the science from its commencement in the crude metallurgical and other operations of the ancients, to the time when a comprehensive system of the science appeared. And when we think of the vast dimensions of the science of to-day, the numberless text-books in every language, the great laboratories springing up in every country, the immense amount of original research, we are carried back in spirit to those mistaken—but often grandly energetic men—who said to the disciples of their art:—

Ora!
Lege, Lege, Lege, Relege, Labora!
Et Invenies.

G. F. Rodwell

SCIENTIFIC SERIALS

Bulletin Mensuel de la Société d'Acclimatation de Paris. The April number contains much interesting information as to the work done by the Society, which besides gratuitously distributing specimens of various useful animals or plants wherever they are likely to thrive, also lends or lets to those persons, whose tastes or knowledge fit them for the charge, some of the rarer species of animal or vegetable life, thus sowing the seeds of miniature *jardins d'acclimatation* throughout the country. During the last 12 months 3 monkeys have been born at the Paris Gardens, one of them in March last. In that month 75 mammalia and 1,669 birds of various sorts were received, while the Society was able to distribute 62 mammalia and 1,731 birds. The Society aims at encouraging the reproduction of all sorts of useful animals, not merely confining its efforts to the maintenance of a stock for exhibition. An interesting account is given of an oyster breeding establishment and aquarium at Biarritz, and of the cultivation of silkworms in France generally. Our French neighbours have set us the example of cultivating our oysters; we may learn some day to follow in their steps and turn our attention, so far as our climate will allow of it, to the "education" of silkworms. This art is becoming quite a recognised industry in France, and the success that has attended its adoption is very gratifying. Bamboos, Spanish broom (*Stipa tenacissima*), China grass or China nettles, Californian pines (*Pinus sabiniana*), are among the plants which are referred to as proper to be introduced into France.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, May 8.—Dr. Hirst, F.R.S., in the chair. Prof Cayley communicated an extract from a letter he had received from M. Hermite "On an application of the theory of unicursal curves," and then gave accounts of the two following papers, "Plan of a curve-tracing apparatus," and "On a rational quintic correspondence of two points in a plane:" another paper entitled "Bicursal curves" (*i.e* curves with a deficiency *one*) by the same gentleman, was taken as read.—Mr. S Roberts read a short "Note on the Pluckerian characteristics of epi- and hypo-trochoids," &c., showing that the curves were unicursal: he gave also the order and class. In connection with these curves Mr. J. L Glaisher advocated the use of Mr. Perigal's term "bicircloids." Amongst the presents received were twenty-two memoirs, &c., by the late Prof. de Morgan, presented by Mrs. de Morgan.

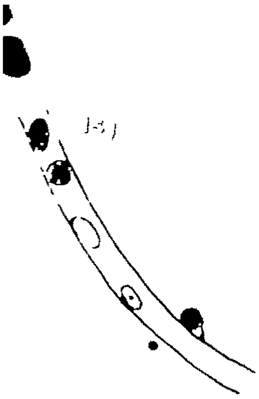
Geological Society, April 30.—Joseph Prestwich, F.R.S., vice-president, in the chair.—On the Permian Breccias and Boulder-beds of Armagh, by Prof. Edward Hull, F.R.S., Director of the Geological Survey of Ireland. In this paper the author described certain breccias occurring in the vicinity of Armagh, which he referred, both on stratigraphical and physical grounds, to the Lower Permian series, considering them to be identical with the "brockram" of Cumberland, and the Breccias of Worcestershire and Shropshire. The author further referred to the extensive denudation which the Carboniferous beds have undergone in Armagh, and also alluded to the occurrence of beds of Permian age near Benburb, between Armagh and Dungannon.—Geological Notes on Griqualand West, by G. W. Stow. The geological results of a journey made by Mr G. W. Stow and Mr. F. H. S. Orpen from the Orange Free State into Griqualand West are communicated by Mr. Stow in this paper, with numerous carefully executed sections and a geological map based on the survey map prepared by Mr. Orpen for the Government. From the junction of the Riet and Modder Rivers (south of the Panneveldt Diamond-fields) to Kheis and the Schurwe Bergen, the track traversed three degrees of longitude. The return route north-east to Mount Huxley and Daniel's Kuil, and eastward to Likatlong, on the Hart or Kolang River, was nearly as long. From the Modder, first south-westward and then westward, to the junction of the Vaal and Orange, the olive shales of the *Dicynodon*- or Karoo-series, traversed frequently by igneous rocks, form the country, and are seen in some places to lie unconformably on older rocks. The shales reach to the end of the Campbell Randt, on the other side of the Orange River, and have been, it seems, formed of the débris of those old hills to a great extent. The oldest rocks of the locality are seen cropping out here and there in the gorges at the foot of the Randt, and consist of metamorphic rocks, greatly denuded, on which the massive and extensive siliceo-calcareous strata of the Great Campbell Plateau lie unconformably. These latter and the breccias of their slopes are coated thickly with enormous travertine deposits. Igneous rock-masses occur around Ongeluk, west of the Jasper range, and then bright-red jasper rocks crop up near Matsáp, succeeded to the west by the parallel quartzite range of Matsáp, and again by other bedded jaspers, which seem to lie in a synclinal of the quartzite rocks, which come up again in the Langeberg. These are succeeded by lower rocks, consisting largely of sandstone, grit, and quartzite, with more or less pervading mica, as far as the journey extended in the Schurwe Bergen, also parallel to the former ranges. The maximum thickness of the successive strata is calculated by the author at 24,000 ft.; allowing for possible reduplications, the minimum is regarded as not less than 9,000 ft. —On some Bivalve Entomostraca, chiefly Cypridinidæ, of the Carboniferous formations, by Prof. T. R. Jones, F.R.S. The larger forms of bivalved Entomostraca are not rare in the Carboniferous Limestone, and some occur in certain shales of the Coal-measures.

Geologists' Association, May 2.—H. Woodward, F.R.S., president, in the chair.—On the valley of the Vézère (Perigord), its limestones, caves, and pre-historic remains, by Prof. T. Rupert Jones, F.R.S., F.G.S. The river Vézère, rising in the department of Corrèze, traversing the department Dordogne, and joining the river Dordogne near Larlat, runs from the old metamorphic rocks of the central plateau of France, through carboniferous, triassic, jurassic, and cretaceous strata. The last mentioned are chiefly limestones, nearly horizontal, presenting steep and often high cliffs, either washed by the river, or bordering its broader and older valley. The softer bands of limestone have been hollowed out along the valley by frost and water, and here and there present recesses and caves These in several instances have been artificially enlarged, and in very many cases have afforded shelter to pre-historic people, and still retain heaps of bones and hearth-stuff, with flint implements of numerous kinds, carved bones and antlers, and occasionally human bones. The most common bones and antlers are those of reindeer, which must have abounded in southern France, whether remaining all the year round or migrating from plain to mountain and back again in their season, for the cave-folk killed them of all ages in vast numbers. The cold climate necessary for the reindeer has long passed away; the musk-ox and the hairy mammoth disappeared also with the reindeer; and looking at the great changes in geographical outlines and contours that have taken place since the extinction of the European mammoth, the author thought that some eight or nine thousand years would not be too long for the bringing about of such changes That the Old cave folk of Périgord saw the living mammoth, a lively outline sketch of its peculiar and shaggy form, on a piece of ivory, found in the Madelaine Cave, is satisfactory evidence. The special geology of the district, the characters of the several caves and their contents, and the most striking of the implements of stone and bone were described in this paper; the human remains found at Cro-Magnon, a gigantic chief and his more ordinary companions, were specially treated of; and the high probability of their belonging to the same race of men as the older Cave-folk was discussed at some length. (For details on this subject see NATURE, vol. vii. p. 305 *et seq.*)

Anthropological Institute, May 6.—Col. A. Lane Fox, V.P in the chair A paper was read on "Eastern Coolie Labour," by Mr. W. L. Distant. The aim of the paper was to show the dissimilarity in the capacity and aptitude for certain work which exists among different peoples under the same conditions. The working of a large sugar estate by means of European capital, European appliances, and European superintendence, with the manual labour of some hundreds of Asiatics, including Klings, Chinese, Javanese, and Malays, was taken as an example. In describing the labours of these peoples, the differences were examined in their capacity for work in general, their aptitudes and dislikes for certain work, and also in their methods of working, viz. by task or otherwise, taken in conjunction with their social condition, and the terms under which they are engaged. In contact with the European the Chinaman seems to prosper; he bargains with him, whilst the Javanese sullenly works for him, and the Kling sinks to a crouching menial in his presence. The European seems affected in the same way; he can chat with the Chinese, tolerate the Javanese, but despises the Kling. European civilisation and prejudice are confronted with Eastern ignorance and prejudice. It is the need of money that has brought these different peoples together. English, Scotch, Portuguese, Klings, Javanese and Chinese are only attracted together in the hope of gain, and under this creed progress and civilisation generally remain in the hands of the strongest and richest party.—A paper by Mr. Howorth was read on "The westerly Drifting of Nomads from the fifth to the nineteenth century, Part x.: the Alans or Lesghs." Col. Lane Fox exhibited two beautifully chipped flint bracelets, four iron bracelets, and other articles found in a tomb in the valley leading to the tombs of the Kings of Thebes; also a large and finely worked flint knife from a tomb in the same neighbourhood. Lieut. S. C. Holland, R N, exhibited a series of photographs of Ainos, and various articles of Aino manufacture.—The Rev. Dunbar I. Heath has been elected Treasurer in the place of the late Mr. Flower.

Zoological Society, May 6.—Prof. Newton, F R.S, vice-president, in the chair. The secretary read a report on the additions that had been made to the Society's menagerie during the months of March and April, 1873, and called particular attention to an example of the Broad Banded Armadillo (*Xenu-*

Plate II.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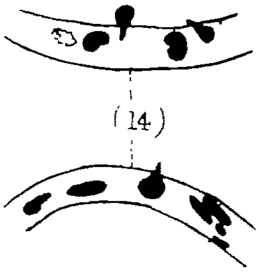
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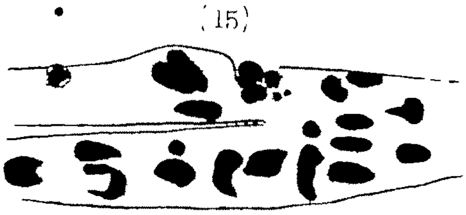
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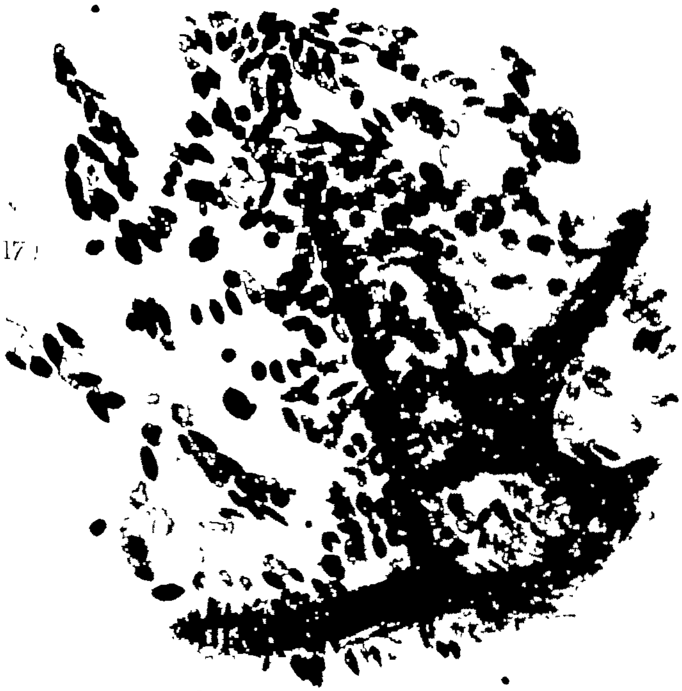
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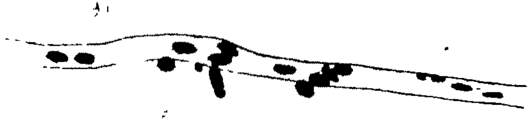
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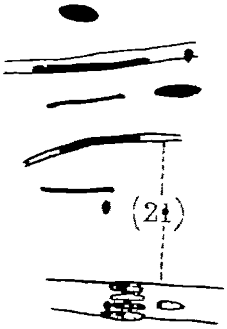
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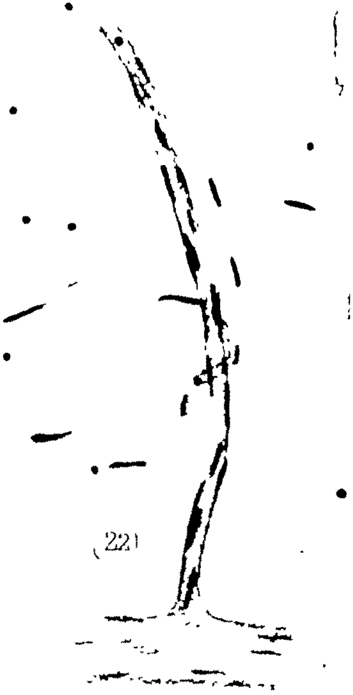
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rus sulcinctus), which was new to the Society's collection; also to a pair of White-necked Cranes (*Grus vipio*) from Japan. No example of this fine species, so far as was known, had previously been brought alive to Europe.—Mr. Sclater exhibited some photographs of, and made some remarks on, a young specimen of the Liberian Hippopotamus (*Hippopotamus liberiensis*) which had recently been received alive by the Zoological Society of Ireland, but had died shortly after its arrival.—A communication was read from the Rev. O. P. Cambridge on some new species of Araneidea, chiefly from Oriental Siberia.—A communication was read from Mr. G. B. Sowerby, jun., on three species of land shells from Madagascar, which he proposed to call *Cyclostoma suffusum*, *C. vexillum*, and *C. perspectivum*, sps. nov.—A communication by Messrs. P. L. Sclater and O. Salvin contained notes on the range of certain species of American *Limnocolæ* in the southern part of the New World. Two distinct species of Stilts (*Himantopus*) were shown to occur in the Neotropical region—namely, *H. nigricollis* Viell., and *H. brasiliensis* Brehm.—Mr. A. H. Garrod read a memoir on the variations of the carotid arteries of birds, in continuation of the labours of Bauer, Meckel, and Nitzsch upon this subject. Mr. Garrod's observations were based principally upon specimens that had died in the Society's gardens.

Entomological Society, May 5.—Mr. H. T. Stainton, vice-president, in the chair.—Mr. Higgins exhibited a specimen of *Langia zenzeroides* (one of the Sphingidæ), from the Himalaya, bred by Major Buckley. He also exhibited a female specimen (the first that he had ever seen) of *Goliathus albosignata*, from Limpopo.—Mr. McLachlan exhibited a coloured plate of butterflies as a sample of a work on the "Natural History of Turkestan," about to be published at the expense of the Government of that place, and founded on the entomological collections made by M. Alexis Fedtschenko during the years 1869–71. The work is to be published in the Russian language, with Latin diagnoses of the new species.—Mr. Bates pointed out a figure in the plate of *Cocandica*, a variety of *Colias nastes*, an insect belonging to Lapland, and remarked that it was an interesting fact that many species of insects belonging to Arctic regions were also found in mountainous districts much farther south, though not in the intervening plains. He mentioned also *Colias palæno*, which was found near the snow-line, in the Alps, and in Lapland.—Mr. Müller also remarked on the close connection between the Arctic and Alpine insect faunas, referring particularly to *Parnassius apollo*, which occurred in the north of Europe, but in Switzerland was confined to the Alps and the opposite Jurassian range, carefully avoiding the intervening alluvial plains, which in the glacial period had been covered with the glaciers of the Rhone, the Reuss, the Rhine, and minor tributaries. He added that if the actual stations of the species were mapped, they would all be found to exist outside, but along the moraines left by the ancient glaciers.—Dr. Sharp communicated a paper on the Staphylinidæ of Japan," principally from the collections formed by Mr. George Lewis.—A paper was read, entitled "Notes on the Ephemeridæ," by Dr. H. A. Hagen, compiled by the Rev. A. E. Eaton.

Royal Horticultural Society, May 7.—General Meeting.—Viscount Bury, M.P., having been nominated by the Council, pending the Queen's approval, to the office of president, took the chair.—The Rev. M. J. Berkeley commented upon the show. Prof. Thiselton Dyer called attention to the first appearance at the meetings of *Odontoglossum vexillarium*, a lovely orchid, with flat rose-coloured flowers, four inches across. It had flowered for the first time in the old world on April 19. The late Mr. Bowman discovered it in New Grenada, on the western slopes of the Andes. It was more nearly allied to *O. phalænopsis* than to the type generally prevailing in the genus.

Scientific Committee.—Dr. J. D. Hooker, C.B., F.R.S., in the chair.—The Rev. M. J. Berkeley exhibited a shoot of *Araucaria imbricata*, illustrating the injury suffered by this plant from the punctures of the young leaves by the prickly points of those on the other branches.—Dr. Masters exhibited a drawing of a flower of Mr. Ware's *Primula veris* var. *chlorantha*. It consisted of a mass of small leafy scales, the innermost of which were prolonged into styles and had ovules upon the edges.—Prof. Thiselton Dyer, adverting to some statements about the cultivation of fungi, stated that, according to Thore, cited by Duchartre, *Agaricus Palomet* and *Boletus edulis* are sown in the Landes by watering the soil with water in which these species had been boiled. The spores of various other species will, it is said, endure a temperature of 212° F., and those of *Peziza repanda* even, according to Schmitz, 230° F.—The Rev. M. J. Berkeley said there was no doubt that fungus spores would bear a high temperature. The development of a *Penicillium* in the interior of loaves of the *pain de munition* almost immediately after they were drawn from the oven to the temperature of which the spores must have been fully exposed, was a case in point. Specimens of *Cytinus hypocistis*, the only European species of *Rafflesiaceæ*, were shown. They had been sent from Cannes by the Hon R. Baillie Hamilton.

Institution of Civil Engineers, May 6.—Mr. T. Hawksley, president, in the chair.—The paper read was a history of the River Clyde, by Mr. James Deas, and gave an account of the various works carried out for improving it as a navigable river, and of the modes and cost of dredging and depositing followed in the deepening and widening of it. It was remarked that for no river in the kingdom had so much been done "by art and man's device" as for the Clyde above Port-Glasgow; that the river from Glasgow, for twelve miles seaward, was nearly as much an artificial navigation as the Suez Canal. One hundred years ago the river was fordable even on foot twelve miles below Glasgow. The engineering works carried out in the Clyde, combined with the mineral resources of the district, had raised Glasgow from an insignificant provincial town, with a population in 1771 of only 35,000, to be the second city in the empire, with a population (including suburbs) of 566,150, according to the census of 1871.

Royal Microscopical Society, May 7.—Dr. Millar, V.P., in the chair.—A paper by Dr. Maddox was read, "On a parasite (believed to be a species of *Tænia*) found encysted in the neck of a sheep." The general characteristics of the cyst and the appearance of sections of it under the microscope were fully described, as were also such portions of the parasite as could be separated from the general mass, and in which the presence of immature ova was particularly noted. The circumstance of finding ova during the encysted condition of the creature was believed to be unique.—A paper was also read by Mr. W. K. Parker "On the Development of the Facial Arches of the Sturgeon," in which the formation and development of the mouth was minutely described, and the relation which it bore to that of the osseous fishes and to mammals pointed out.

PHILADELPHIA

Academy of Natural Sciences, January 14.—Dr. Ruschenberger, president, in the chair.—Prof. Cope made some observations on the structure and systematic position of the genus *Eobasileus* Cope. *Uintatherium* Leidy and *Dinoceras* Marsh were names applied to allied mammals, so that the same would probably apply to them also. Until further evidence is presented, he adheres to his original position, that these animals are true *Proboscidia*, and cannot be referred to any other order.—"On the Forms of Artificial Oxide of Zinc," by George A. Koenig, Ph.D.—"On a Boiler Incrustation from New Jersey," by George A. Koenig, Ph.D.

January 21.—Dr. Bridges in the chair.—Notice of Fossil Vertebrates from the Miocene of Virginia. Prof. Leidy directed attention to some fossils, part of a small collection recently received. They were found imbedded in blue clay containing an abundance of fossil diatomes, among which Coscinodiscus is especially conspicuous. The fossil vertebrate remains consist mainly of vertebræ and teeth of cetaceans, vertebræ of bony fishes, teeth of sharks, and spines of rays. Among them also there is a portion of a humerus of a bird, and several worn teeth of a peccary. Besides these there are specimens which may be regarded as characteristic of the following undescribed species: *Protocamelus virginiensis*, *Tautoga* (*Protautoga*) *conidens*, *Acipenser ornatus*.—Mr. Thomas Meehan offered to the Academy some facts in regard to the fertilisation of flowers which confirmed the popular view that pollen of one variety had an immediate influence on the structure of the fruit of another variety, as well as on the progeny; and also furnished some entirely new facts in regard to the ability of a seed germ to receive impregnation from two distinct sources. Mr. Arnold of Paris, Canada, determined to observe the effect of cross fertilisation on Indian corn. He procured a very peculiar variety of which Mr. Meehan exhibited an ear, not known in the vicinity—a brown variety, with a circular dent at the apex—and raised one plant from it. The first set of flowers were permitted to be fertilised by their own pollen in order to test whether there was any reversionary tendency in the plant, or the pollen of any other variety in the vicinity. The ear now produced was the result

—every grain being like its parent. The corn plant produces two ears on each stalk. As soon as the "silk"—the pistils of this second ear—appeared, the pollen—in a "tassel"—of the common yellow flint corn was procured, set in a bottle of water tied near the developing ear, the plant's own tassel having been cut away sometime previous. After a short time this set of male flowers was removed, and a panicle of male flowers from a white variety was introduced to the same bottle in order to afford it the opportunity of operating on the same female flowers. The result was the ear now presented. The base of *each grain* was of the yellow flint corn, but the *upper half* of the white variety. The result was he thought no escape from the conclusion, not only that there was an immediate influence on the seed and the whole fruit structure by the application of strange pollen; but the still more important fact, hardly before more than suspected, that one ovule could receive and be affected by the pollen of two distinct parents, and this too after some time had elapsed between the first and second impregnation.

February 4.—Mr. Vaux, vice-president, in the chair.—The following papers were presented for publication:—"On the Lingual Dentition of certain Terrestrial Pulmonata from the United States, with remarks on their systematic value," by Thos. Bland and Wm. G. Binney; "Catalogue of the recent species of the Class Brachiopoda," by W. H. Dall, U.S.C.S.; "Descriptions of Mexican Ichneumonidæ," by E. T. Cresson. "Notices of Remains of Fishes in the Bridger Tertiary Formation of Wyoming." Prof. Leidy remarked that among the multitude of fossils which had been collected from the tertiary clays and sandstones of the Bridger Group of Wyoming, there were comparatively few pertaining to fishes. Nevertheless the remains of these are not unfrequent, but they are not so complete as one might have expected from the nature of the beds containing them. They usually occur as isolated bones, scales, and teeth, and mostly indicate fishes related with our living Gars (*Lepidosteus*) and Mud Fish (*Amia*). Many of the fragments appear to indicate the following extinct species previously undescribed:—*Lepidosteus atrox*; *L. simplex*; *L. notabilis*; *Amia* (*Protamia*) *uintaensis*; *A.* (*Protamia*) *media A.* (*Protamia*) *gracilis*); *Hypamia elegans*; *Pimelodus antiquus*; *Phareodus acutus*.

PARIS

Academy of Sciences, May 5.—M. de Quatrefages, president, in the chair.—The deaths of Baron Liebig, foreign associate of the Academy, and of M. Hausteen, correspondent, were announced.—The following papers were read:—On the heat produced by the reactions between water and ammonia, calcic, baric, and strontic oxides, by M. Berthelot. The author had estimated the heat produced by the solution of dry NH_3 in water, and also on the dilution of the former solution with more water; he has found that as regards the latter case the heat is in inverse ratio to the water already combined with the ammonia. The determinations of the heat in the case of calcic, baric, and strontic oxides, was made by dissolving them in HCl, and from the result obtained the heat for their combination with water was calculated.—On the separation of potash and soda in vegetables, 5th memoir, by M Eug. Peligot. The author finds that in those cases where plants growing near the sea contain sodium salts, this fact is to be attributed to their absorption of them, through their leaves, from the spray in the air, and not from the soil.—A report on M. Bertin's memoir on the resistance opposed to rolling by the keel of a vessel, by MM. Paris, Jurien de la Gravière, and Dupuy de Lôme.—On the conditions of the integrability of simultaneous equations, &c., by M. Collet.—On the use of the meat of tuberculous animals for food; can this meat cause the development of pulmonary phthisis? by M. G. Colin. The author, from the results of thirty experiments where as many animals were fed on every kind of tuberculous flesh, answers the question in the negative. Where other experimenters have obtained opposite results, he believes that they have either experimented on animals already diseased, or have allowed portions of the tuberculous matter to find admission to the lungs of the animals in the air they breathed. —On the action of ozone on absolute alcohol: on the combination of hydrogen and cyanogen under the influence of the silent electric discharge, by M. A. Boillot.—A new observation of comet II., 1867, by M. Stephan.—On the effects produced by electricity on mercury immersed in different solutions, by M. Th. du Moncel.—On the purification of hydrochloric acid by M. Engel.—On the estimation of sugar by Barreswil's method by M. E. Feltz.—Experiments on the respiration of fish, by M. Quinquand.—Contribution to the history of microzymes and Bacteria: physiological transformation of Bacteria into microzymes and of mycrozymes into Bacteria in the digestive tube of the same animal, by MM. Béchamp and Estor.—On the remains of *Elephas priscus* found in the quaternary formation of the environs of Paris, by M. J. Reboux.

DIARY

THURSDAY, May 15.

Royal Society, at 8.30.—On the Periodicity of Rainfall in Connection with the Sun-spot Periodicity: C. Meldrum.—On the Heating of a Disc by rapid Rotation in Vacuo: B. Stewart and P. G. Tait.—On Jeypoorite: Major Ross.—Determination of the number of Electrostatic Units in the Electromagnetic Unit made in the Physical Laboratory of Glasgow University: D. McKichan.

Society of Antiquaries, at 8.30.—Remarks on some Pictures by Quintin Matsys and Holbein, in the Collection of the Earl of Radnor, at Longford Castle, lately exhibited at the Royal Academy: J. G. Nichols.

Chemical Society, at 8.—On Isomerism: Dr. H. E. Armstrong.

Numismatic Society, at 7.

Royal Institution, at 3.—Light: Prof. Tyndall.

FRIDAY, May 16.

Royal Institution, at 9.—Limits of Certainty in Taste: Sidney Colvin.

Horticultural Society, at 3.—Lecture.

SATURDAY, May 17.

Royal Institution, at 3.—Ozone: Prof. Odling.

MONDAY, May 19.

London Institution, at 4.—Elementary Botany: Prof. Bentley.

Asiatic Society, at 3.—Anniversary.

Victoria Institute, at 8.—Anniversary.

Society of Arts, at 8.—Cantor Lectures. On Wines; their Production, Treatment, and Use? J. L. W. Thudichum, M.D.

TUESDAY, May 20.

Royal Institution, at 3.—Early Roman History and Architecture.

Institution of Civil Engineers, at 8.

Statistical Society, at 7.45

Anthropological Institute, at 8.—On the Egyptian Colony and Language in the Caucasus: Hyde Clarke

Zoological Society, at 8.30.—On African Buffaloes: Sir Victor Brooke, Bart.—Remarks on varieties of the Carp: Lord Arthur Russell.—On *Lepilemur cheirogaleus*, and on the Zoological rank of the *Lemuroidea*: St. George Mivart.

WEDNESDAY, May 21.

Meteorological Society, at 7.—Discussion on Proceedings of Meteorological Conference at Leipzig.—On Land and Sea Breezes: J. K. Laughton. —Notes on a Double Rainbow observed at Kirkwall: R. H. Scott.—On some Results of Temperature Observations at Durham: J. J. Plummer.

Horticultural Society.—Exhibition of Ericas, Pelargoniums, &c.

Society of Arts, at 8.—Recent Processes for the Production of Gas for Illuminating Purposes: Thomas Wells.

London Institution, at 7.—Fourth Musical Lecture.

THURSDAY, May 22.

Royal Institution, at 3.—Light: Prof. Tyndall.

Society of Antiquaries, at 8.30.

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THURSDAY, MAY 22, 1873

THE FUTURE OF THE ENGLISH UNIVERSITIES

AN ECHO FROM OXFORD

THE Association for the Organisation of Academical Study has inaugurated a good work, which must in the end have an important result. But in the expressions of policy as yet put out by that body we notice an omission which perhaps is intentional, but in any case a very serious, indeed a fundamental one. It is well enough to declare that the collegiate and other revenues of Oxford and Cambridge should be devoted to the encouragement of research, and to placing the highest kind of teaching in all subjects within the reach of the people of this country. It is most true that to this end prize-fellowships and non-resident sinecures must be abolished, and in their place we must have carefully-chosen professors, assistant-professors, and lecturers, teaching and carrying on original research in all departments of knowledge. With such a programme in hand the members of this association can very plausibly demand for the old Universities that they be not despoiled of their excessive wealth, but that this wealth be made operative and productive within the limits of the Universities themselves. Nevertheless there is a question which necessarily arises—whenever the future of the English Universities is mentioned—which the Association has not discussed, and which we think it ought boldly to meet, even though it should lead to a split in the ranks. That question is this—Are Oxford and Cambridge to remain as institutions exclusively for the elegant education—the "culture"—of the upper classes who may choose and can afford to allow their sons to while away certain years there? or are they to be made engines of national education where a poor man may go with as much reason as a rich one; and profitably spend his time in acquiring knowledge and training which have a real value in the world and place their possessor in the position to earn his bread and his standing among men?

It is a fact that at this moment a youth entering a college at either Oxford or Cambridge and taking his degree after four years of a very pleasant life, having spent during the process at least 800*l.* (*i.e.* 200*l.* a-year) comes away, not a whit further on in the battle of life than he was on entering. He has acquired some good habits, many very bad ones, but has received no training nor instruction which will render him useful to other men, excepting—the exception is a very significant one—as a clergyman or as a schoolmaster.

The state of things is neither more nor less than this—that a young man cannot study at the English Universities and associate with even the most steady-going of his fellow-students at a less expense than that named above; and that the University cannot, at any rate does not, teach him anything in a practical or professional way. It is useless for Cambridge and Oxford to open their classes to non-collegiate students, as long as those classes are not something more systematised and practically-pointed than they can be, when exclusively designed as parts of a so-called elegant or "liberal" education. Men who are intending to work hard in life cannot afford to pass through such a course after leaving school; and hence our University students are, with a few exceptions, drawn from the richer classes; hence, too, the amount of luxury and rarity of earnest study amongst them, which reacts on many of their teachers. The present position of the Universities with regard to education for the business of life is merely that of a preparatory school. The same limitations of subjects—the same books are in force here, with some small additions for the few "honour men," as in our public schools, such as Eton, Harrow, and Rugby. The B.A. degree—the ordinary examinations for which any average boy on leaving school at sixteen or eighteen years of age could easily pass—absorbs nearly all the activity; is, in fact, almost the highest effort of each University. Almost all the teaching, certainly all the college work, is directed and governed by the requirements of this preparatory course which prepares for nothing. Whilst the intellectual standard thus held up is childish enough, it is necessarily accompanied by a system of tutorial superintendence and direction as wearisome as it is injurious.

In fact, the best effort in Oxford and Cambridge—the most striking movement in recent times—as compared with the dead calm of some fifty years since, has been rather a retrogression than an advance; we are less of Universities now than then, and have become more like—and are daily becoming more like—the great public schools, such as Eton and Harrow. The greater part of all the college-teaching staff is employed in doing the very same work as that done in the schools, which ought never to be required at a University at all. As the arrangements and innovations of the various college-bodies are watched, it becomes obvious that the schoolmaster is abroad in a very ambitious spirit with the avowed object of making the University a great Seventh Form, similar in discipline and character of instruction to his own pedagogic institution.

This state of things is defended by a large number of persons—among them members of the Association—with two words chiefly in their mouths—"culture" and "technical." It is maintained that "technical education" (an expression which is used for the purpose of suggesting the less intellectual side of what it is better to term "professional education") is not the function of the Universities, that it cannot be conveniently undertaken in them, that it is better carried out in the great cities such as London, Manchester, Edinburgh, whilst the Universities in their academic seclusion can administer that smattering of omniscience, dilettantism, and good manners which it is so important for persons of a certain income to possess. To obtain this a youth must be prepared to sacrifice time and money; and in offering this the University is, according to the opinion of many resident fellows, doing its work in the world. The selfishness of this view of University functions is patent enough. Clearly it is an easier matter to undertake this ornamental work, and to leave to others the business of life. It appears to be overlooked by its advocates that the Universities thus may, or rather have, lost all influence, all share in the life of the country. In renouncing technical or professional education, the University renounces all those who must have such education at the age when she might receive them. Those who really value, as we do above most things, breadth of intellectual interests—who have intense repugnance to narrow "specialism"—cannot, upon due consideration, defend the separation of "ornamental" and "technical" education, as likely to conduce to increase of culture among our fellow-countrymen. It is by undertaking most fully the charge of the higher education—of those for whom without distinction such education is necessary—that the Universities can really do most for the cause of culture. When Oxford and Cambridge succeed in getting hold of all such students then only can thoroughly satisfactory results be expected by those who are anxious for the progress of the higher education. What we desire more earnestly is, that Oxford and Cambridge may be the means of giving breadth of view and interest to as large a number of young Englishmen as possible, for it is this that we understand by "culture:" not the mere ease of manner due to luxury and the select association of leisured men. Oxford and Cambridge can spread true culture, and can have pretensions to such an office only when acting up to their trust and fully providing for the very best and fullest professional study in all departments. There are some to whom it appears important that the Association should plainly declare itself on this matter, before proceeding to the question of the foundation of institutions for scientific and literary research within the University. If on the one hand the Association were to declare for the exclusion of professional study, and at the same time to advocate the foundation of increased means and material of research within the University, we should feel at once that the policy of the Association would not be accepted by all. There is a great deal of human nature in the men who occupy distinguished positions in our Universities, and in the select atmosphere of non-professional students and cultured ecclesiastics there is an inevitable languor and repose of the mind which are infectious. The most vigorous body becomes limp before the sirocco, and in this atmosphere of luxurious culture it may be doubted whether even Faraday could have carried out his investigations: probably only by investing himself in a kind of mental diver's costume. On the other hand, the presence of an active body of those who for want of a better word we may call professional students—of men who, having neither time nor money for self-indulgence, determinedly work round their professor—the presence of a whole lot of such professors each so surrounded, and the association thus established between the Universities and the progress of the body of the country in the arts and sciences, would bring about a gigantic change. Professors so surrounded might with advantage be largely increased; the purely ornamental students would be by no means dislodged—they would remain in numbers then as now—but beneficially influenced by the example of the career-seeking and professional student. These in their turn would be benefited by a duly proportioned infusion of those students seeking exclusively "culture"—the amateurs and patrons of serious pursuits.

It is, then, only on the basis of professional training in the widest sense of the term, that we should care to see a reorganisation of Oxford and Cambridge. Let the colleges be taxed, say, to the extent of fifty per cent. of their revenue in order to support the professoriate and the appliances which each faculty may deem adequate, not only for direct "student teaching," but for progressive research. Then we may hope to see our Universities elevated from the condition of mere finishing schools for young gentlemen. If such a plan cannot be carried out, it would seem useless to simply create sinecures within the old places, larger and probably less productive than those which at present exist. Sharp and painful though the measure might be—we should in that case have to yield to the removal of means which have so long lain idle. The colleges would be relieved of their excessive income to support more practical institutions elsewhere, and Oxford and Cambridge would collapse into the condition of mere theological seminaries. When the Association meets on Saturday next, it would be well that this point should be raised, lest by the silence of the leaders of the movement, any one should be lukewarm in its support.

FRICK'S PHYSIKALISCHE TECHNIK

Oder Anleitung zur Anstellung von physikalischen Versuchen und zur Herstellung von physikalischen Apparaten mit möglichst einfachen Mitteln. Von Dr. J. Frick. (Braunschweig, 1872)

THIS most useful book has now reached the fourth edition, and has swelled to 700 pages, illustrated by 986 wood engravings. To some British physicists and teachers the work has already proved itself serviceable, but there are doubtless many to whom it is at present unknown who would find much valuable information therein.

Dr. Frick's work is not in any sense a manual of experimental physics; it is rather an elaborate treatise upon physical apparatus and the methods of physical research. Its object, we learn from the preface, is to give an introduction to the methods of conducting physical inquiry, to enumerate the precautions which it is necessary to adopt in order to ensure success, and to give ample directions with reference to the construction of apparatus and its management. This field is, comparatively speaking, untrodden before, and we have no hesitation in saying how thoroughly successful Dr. Frick's attempt to guide us over it has proved. We shall briefly indicate the contents of the book, and then point out the few matters in which we think the execution of the task has fallen short of what might have been fairly expected.

The first part contains a sketch of the arrangements necessary for the physical laboratory, and a detailed account of the methods of manipulating glass, metals, and other materials which are required for the apparatus described in the second part. This portion of the book is very interesting and useful. We find here numerous hints on turning, glass-blowing, and similar processes with which it is well for the physicist to be acquainted. In the second part we have in Chap. I. a description of the apparatus necessary for the study of the equilibrium of forces applied to solids, liquids, and gases; Chap. II. describes the apparatus used for experiments on motion

Chap. III. is on acoustics; Chap. IV. on light; Chap. V. on magnetism; Chap. VI. on electricity; Chap. VII. on heat. It may be remarked that the figures are drawn to scale, and further illustrations of the details are added whenever necessary.

As a fair specimen of the illustrations and descriptions we may refer to Article 121, wherein is described Muller's apparatus for studying experimentally the free falling of a body. This beautiful contrivance is for the purpose of causing a point vibrating horizontally to trace a curve up on a board descending vertically. From the form of the curve the law of falling bodies is deduced. In Chap. IV. we meet with many interesting contrivances: for example, Fig. 433 represents an arrangement for showing the principle of the rainbow experimentally by the aid of spheres of glass. This chapter is concluded by a practical lesson in photography. Many of the figures in Chap. VI. will be found to represent electric instruments which are manifestly great improvements on forms in ordinary use. As an example we refer to the Rheostat, Fig. 775.

Considering the book has already reached such portly dimensions we can hardly complain of omissions. We are, however, of opinion that the space at the disposal of the author might have been more judiciously employed if some of the apparatus which he has described were omitted and some instruments which he has passed over were inserted instead. To illustrate this remark we may refer to the chapters on mechanics. We there find a number of ingenious contrivances generally pretty well known, but we also meet with toys like those described in articles 66 and 67 which could, we think, have been very well dispensed with. On the other hand we seek in vain in the same chapter for a full account of Willis's system of mechanical apparatus. To say that this ingenious system would, with trifling additions, enable all the mechanical experiments described by Dr. Frick to be performed is to give a very inadequate idea of its resources. In the hands of a competent experimenter Willis's apparatus will be found to provide in a substantial form the principal parts necessary for nearly every conceivable experiment in mechanical philosophy. The framework of this apparatus is so useful in almost any physical research that we cannot conceive how it could have been omitted from "Physikalische Technik," had the author of that work been acquainted with the writings of Prof. Willis. We think also that some of the host of merely qualitative experiments described for the purpose of illustrating centrifugal tendency (Article 124) might very well be omitted. On the other hand, we miss Smeaton's machine, which, admitting as it does of exact quantitative results being determined. is perhaps, next to Atwood's machine, the most useful instrument we have for illustrating the truths of dynamics.

We are tempted to think that Dr. Frick is not adequately acquainted with English scientific literature. This opinion receives some confirmation when, on turning over 238 closely-printed pages which describe electrical apparatus, we fail to see Sir William Thomson's beautiful instruments described; nor on turning to the Index do we even find the name of that philosopher mentioned.

Although we decidedly think this book might have been better, yet we decidedly think that it is very good, and we cordially recommend it to the notice of physicists and lecturers, who will certainly find it useful.

OUR BOOK SHELF

Electricity. By R. M. Ferguson, Ph.D., F.R.S.E. (W. and R. Chambers.)

We regret that the Elementary Treatise on Electricity has not been revised by its author since its first appearance. For example, useful as is the chapter on the absolute measurement of an electric current, its usefulness to students would be increased by a fuller and more detailed explanation. At the foot of p. 159 it is stated that "the heating effect (of the current) depends on the strength of the current and the resistance." It should be the *square* of the strength of the current into the resistance, as is correctly stated in a preceding paragraph. On p. 153 there is a mistake in the calculation of the quantity of water decomposed by a current; 60 c.c. × tan. $51\frac{1}{3} = 75$ c.c., and not 80 c.c., as is stated, and afterwards assumed. A description of the sine-galvanometer ought hardly to have been omitted, and a fuller explanation, together with an engraving of Thomson's reflecting galvanometer, ought surely to be given. There is also but a meagre account of the induction coil, and the function of the condenser is not explained: the term *rheotom* instead of contact-breaker, looks pedantic, and may puzzle some readers. But the most faulty part of the book in our estimation is the singularly obscure and misleading manner in which the terms Electric Quantity and Tension are defined on p. 64. Tension is spoken of as synonymous with electric depth, or as the French say, electric thickness; whereas the tension, pressure, or power of discharge possessed by any electrified point, varies as the *square* of the electric depth at that point.

The first part of this text-book relates to magnetism and more evident care has been bestowed on this portion. The charts of isogonic and isoclinic lines are most useful, and so also are the chronological appendices, in which a brief scientific history of each subject is given. But why could not the dip and declination be given for a later year than 1865? It is said on page 16 that two magnetic needles are absolutely necessary to show "the power of the earth in determining the position of the needle," and that "if it were possible to hang a needle in the air so as to leave it perfectly free to take any position, it would show us fully the directive action of the earth." Is it not possible to buoy a magnetic needle in water, or sink it in mercury, so that the action of gravity may be neutralised, and the directive influence of the earth wholly come into play? Moreover, many dipping needles are made with a swivel pivot, by means of which the declination and dip are roughly shown at the same time. Two other blunders we notice in the part on magnetism. On page 4, speaking of a "small magnetic bar or needle," Dr. Ferguson says that "if both poles of the needle are attracted indifferently by any end of it [a bit of iron], it is not magnetic." This is as slipshod in its science as it is in its English, for it is precisely the test of a magnetic body that it does attract either end of the needle; magnetic should of course read magnetised, and so again a few lines lower down. The other blunder is on page 14, where it is said that "cobalt is attracted by the magnet at the highest temperatures." It is well known, and can easily be shown as a class experiment, that cobalt loses its magnetic character at a white heat. But in spite of these errors, Dr. Ferguson's "Electricity" is a book that has been of much use to both teachers and students of science. Its obvious merits lead us to hope that a revised edition may find it free from the defects to which we have drawn attention.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

Forbes and Tyndall

AT p. 387 of the recently published "Life and Letters" of the late Principal Forbes, the following passage occurs :—

"I believe that the effect of the struggle—though unsuccessful in its immediate object—will be to render Tyndall and Huxley and their friends more cautious in their further proceedings. For instance, Tyndall's book, again withdrawn from Murray's 'immediate' list, will probably be infinitely more carefully worded relative to Rendu than he at first intended."[*]

This passage has been selected, among others, by Principal Shairp, the editor of this portion of the "Life," from a letter addressed to A. Wills, Esq., under date of November 14, 1859 : the "struggle" to which it refers arose out of an attempt on the part of some influential friends of Principal Forbes, who were at that time members of the Council of the Royal Society, to obtain the Copley medal for him ; and it took place at the Council meetings which were held on October 27 and November 3, 1859.

I was not a member of the Council at this time, and therefore, I could take no direct part in the "struggle" in question. But, for some years before 1859, glaciers had interested me very much ; I had done my best to inform myself in the history of glacier research ; I had followed with close attention the controversy which had been carried on between Prof. Tyndall and his friends, on the one hand, and Principal Forbes and his supporters on the other ; and, finally, I had arrived at a very clear conviction that the claims made for Principal Forbes's work, could not be justified.

Under these circumstances I thought it would be a most unfortunate occurrence if the Council of the Royal Society, containing as it did, not a single person who had made the glacier question his especial study, should practically intervene in the controversy then raging, and throw its weight upon the side of one of the combatants, without due consideration of what was to be said on the other side.

A friend of mine, who was a member of the Council, shared these views ; and, in order to enable him to enforce them, I undertook to furnish him with a statement which he could lay before the Council when the award of the Copley medal came up for discussion.

It is not necessary to state what took place at the meetings of the Council—suffice it to say that the Copley medal was not awarded to Principal Forbes.

So far, therefore, as my statement may have contributed to this result, my efforts were completely successful. Principal Forbes's very influential champions in the Council were left, as I am informed, in a hopeless minority ; and instead of tending to make me more cautious in my "future proceedings," what occurred on this occasion should have emboldened me.

The notion expressed by Principal Forbes that I and Prof. Tyndall's other friends were in any way discouraged by the results of our battle is therefore strangely erroneous ; however, I do not know that the error would have been worth correction, if Prof. Tyndall had not been referred to as one of those who took part in the fray. But, in justice to Prof. Tyndall, I am bound to say that he knew nothing about the battle until after it was over. My ally in the Council and I, agreed, for reasons which will be obvious to any honourable man, that Prof. Tyndall, though an intimate friend of ours (and largely because he was so), ought not to have any knowledge of the action we took ; and, in a note dated November 4, 1859, I find myself suggesting to my friend in the Council, that Tyndall ought to be kept in his then ignorance "until his book is out." I have every reason to believe that this suggestion was carried into effect ; at any rate, Prof. Tyndall did not see the drift of my statement till a year ago, when (on May 13, 1872) I sent it to him accompanied by some other documents and the following note :—

"Routing among my papers yesterday I came upon the inclosed cinders of an old fire, which I always told you you should see some day. They will be better in your keeping than mine."

I am informed that there was not even an attempt to controvert the leading points of my statement on the part of the advocates of Principal Forbes's claims ; and therefore the assertion that Prof. Tyndall was led to word "infinitely more carefully" what he had already written about Rendu, by anything which occurred in the Council, is simply preposterous.

[*] In making these remarks I have no intention of throwing the slightest blame upon the late Principal Forbes ; who surely had a perfect right to express to an intimate friend whatever impression was left upon his mind, by such reports as reached him of the occurrences to which he refers. But I confess I find it difficult to discover any excuse for the biographer, who deliberately picks the expressions I have quoted out of a private letter, and gives them to the public, without taking the trouble to learn whether they are, or are not, in accordance with easily ascertainable facts.

T. H. HUXLEY

May 17

Forbes and Agassiz

IN the review of Dr. Tyndall's book on the "Forms of Water" which appeared in NATURE, vol. vii. p. 400, the following words occur :—"But surely it was not unnecessary to rake up again the Forbes-Rendu controversy, nor to renew the claims of Agassiz and Guyot." Mr. Alexander Agassiz takes exception to this (see NATURE, vol. viii. p. 24) and makes the following assertions :—That when a guest of Agassiz on the glacier of the Aar in 1841, Forbes returned the hospitality of Agassiz "by appropriating what he could" from the work of the latter, and "misrepresenting the nature of his intercourse with Agassiz." This refers to a matter of facts and may be proved or disproved by the facts. It refers to an attack made upon Forbes in 1842, which was immediately answered by him in a manner that left no room for further discussion. I must necessarily be brief in stating the facts. They may be found fully detailed in the *Edin. New Phil. Journal*, 1843, or in the "Life and Letters of James David Forbes, 1873." They are as follows :—In 1841 Forbes enjoyed the pleasure of a visit to Agassiz on the Unteraar Glacier. On the first day of their sojourn (August 9), their only companion was Mr. Heath, of Cambridge. They were afterwards joined by friends of Agassiz. On this first day Forbes pointed out to Agassiz the veined structure of the ice. Agassiz had spent five summers studying the glaciers (see Mr. Alexander Agassiz' letter in NATURE), but he replied "that it must be a superficial phenomenon, that he had on a previous occasion noticed such markings, and that they were caused by the sand of the moraines causing channels of water to run." Forbes showed him that the structure was general, even in the body of the glacier. Agassiz expressed a doubt "whether the structure had not been superinduced since the previous year." Forbes afterwards showed him that in a crevasse three or four years old the markings extended across the crevasse and were visible in continuation from one side to the other. Further, Forbes insisted upon its intimate connection with the theory of glaciers. When in the ensuing winter M. Desor wrote to Prof. Forbes denying his claims to the discovery, the latter sent him a statement of the above facts, begging that M. Agassiz should state whether they were correct or not. M. Agassiz wrote an answer to this letter. He does not deny a single one of the facts supplied by Forbes in connection with the observations of August 9. This letter was printed and circulated by M. Agassiz. Furthermore, when these facts were published by Forbes, even then M. Agassiz did not deny any of them. Moreover, Mr. Heath, the only other witness, gives his evidence in support of the accuracy of the above facts (see "Life of Forbes," Appendix B, Extract I.). Other friends of Agassiz, who joined them afterwards, wrote to Forbes stating their belief that to him alone belonged the discovery. After leaving the Aar glacier Forbes extended his observations. He showed (1), that the structure was common to most, if not all, glaciers (see "Forbes' Life," p. 550, note) ; (2), that this was the cause of the sand lying in lines ("Life," p. 548) ; (3), that this was also the cause of the supposed horizontal stratification of the terminal face of some glaciers (Royal Soc. Edin., 1841, Dec. 6) ; (4), he showed that these blue markings were the outcroppings of blue ice that formed lamellar surfaces in the interior of the glacier ; (5), he actually determined the shape of these surfaces in the case of the Rhone glacier (R. S. E., 1841, Dec. 6) ; (6), he remarked that "the whole phenomenon has a good deal the air of being a structure induced perpendicular to the lines of greatest pressure," though he did not assert the statement to be general. This was in 1841. In later years he extended these observations. I have said enough to prove (1), that although Agassiz carried with him "a geologist, a microscopic observer, a secretary, a draughtsman, and many workmen," and though he had spent five summers studying the glaciers, he did not see these markings (or at any rate recognise them as a structure of the ice) until Forbes showed them to him ; and (2), that Forbes recognised this structure as an important "indication of an unknown cause" ("Occasional Papers," p. 4), and worked out the subject thoroughly.

In the *Comptes Rendus* for Oct. 18, 1841, a portion of a letter from Agassiz to Humboldt was published. Here he lays claim to the discovery without mentioning the name of Forbes. He speaks of it as "le fait le plus nouveau que *j'ai* remarqué." Forbes felt deeply annoyed at this conduct of his friend, but contented himself with publishing his own discovery. A rupture between the two friends now commenced. About this time M. Guyot recollected that he had described this appearance in 1838 to the Geological Society of France, at Porrentruy ("Agassiz Etudes," p. 207). Several people had seen the same thing previously. Among others, Sir David Brewster writes as follows:—"The Mer de Glace is like the waves of the sea, as if they had been fixed by sudden congelation; when the ice is most perfect, which is on the sides of the deep crevices, the colour is a fine blue. There is an appearance of a vertical stratification in the icy masses stretching in the direction of the valley in which the glacier lies. . . . The surface of the glacier exhibits also the appearance of veins exactly like blocks [?] of stone" (*Journal*, 1814). In 1820, M. Zummstein saw it ("Bibliothèque Universelle," 1843). Col. Sabine and M. Elie de Beaumont had also seen it ("Travels in the Alps," p. 29). But though seen it had not been studied, nor did any printed description of it exist. M. Guyot did not even print an abstract of his communication. It remained an isolated, unprinted, forgotten fact until Forbes appeared upon the scene. Professor Tyndall has most justly said that neither Forbes nor Agassiz knew of it in 1841 ("Forms of Water," p. 187). Yet though, as has just been proved, Forbes pointed it out to Agassiz in 1841, the latter tried to show that he had known of Guyot's observation (letter from Agassiz to Forbes, "Life of Forbes," Appendix B), and endeavoured to give the credit to Guyot rather than to Forbes (his own claims having been now disproved). If it be true that he knew what Guyot had done, then (1) why did he not mention it to Forbes and Heath, both of whom affirm (in contradiction to the statements of Agassiz) that Guyot's name was not mentioned? (2) Why did he not perceive the importance of the structure? (3) Why did he say that it was superficial? (4) Lastly, how could he reconcile it with his conscience to describe it to Humboldt as "le fait *le plus nouveau* que *j'ai* remarqué?"

The facts show (1) that Forbes was seriously wronged by the conduct of Agassiz; (2), that he discovered independently the veined structure; (3) that he was the first to study the subject and give it its true place in reference to glacier theories. I have limited myself to the accusation contained in the letter of Mr. Alex. Agassiz. Whether he is correct in his appreciation of the estimate put upon Forbes' labours, in Dr. Tyndall's last popular work, I need not at present discuss. I know so well to what conclusion a comparison of that book with the writings of Forbes and other workers on glacier theories would lead, that I leave it confidently to the judgment of those "fair-minded investigators" of whom Mr. Alex. Agassiz speaks.

GEORGE FORBES

P.S.—Mr. Heath's testimony, to which I have referred, is given in the following extract from a letter dated Trinity College, Cambridge, Feb. 25, 1842:—"I will witness—1st, that he (Agassiz) knew nothing about it; 2nd, when he did see it he said it was superficial *sand*; 3rd, that he was the last to believe that it went to any depth. I think your account very true, and not claiming one jot more than fully belongs to you."

Cambridge, May 20 G. F.

Perception and Instinct in the Lower Animals

THE suggestion made by me in your issue of February 20, that animals which had been deprived of the use of their eyes during a journey might retrace their way by means of smell, had the effect of letting loose a flood of illustration, fact, and argument bearing more or less directly on the question; and as the stream now seems to have run nearly dry, I ask permission briefly to review the evidence adduced, so far as it affects the particular issue I brought forward. Several of the writers argue as if I had maintained that in all cases dogs, &c., find their way, wholly or mainly, by smell; whereas I strictly limited it to the case in which their other senses could not be used. The cases of this kind adduced by your correspondents are but few. The first, and perhaps the most curious, is that of Mr. Darwin's horse; but, unfortunately, the whole of the facts are not known. As Mr. Darwin himself pointed out, the horse may have lived in the Isle of Wight, and been accustomed to go home along that very road. I would suggest also that the country might resemble some tract in the neighbourhood of his own home; or that the horse, having been brought from home by a route and to a distance of which it had no means of judging, thought its master was riding home on the occasion in question, and therefore objected to turning back. Anyhow, the case is too imperfect to be of much value as evidence in so difficult a matter. "J. T." (March 26) quotes the case of the hound sent "from Newbridge, county Dublin, to Moynalty, county Meath," thence long afterwards to Dublin, where it broke loose, and the same morning made its way back to its old kennel at Newbridge. I can find no "Newbridge, county Dublin," although there is a Newbridge, county Kildare, which is 26 miles from Dublin, on a pretty direct high road. That the dog never attempted to return during its "long stay" at Moynalty seems to show that some special facilities existed for the return from Newbridge. What they may have been we cannot guess at in the total absence of information as to the antecedents of the dog, the route by which he returned, and the manner in which he conducted himself on first escaping in Dublin.

The next case, of the two dogs returning from Liverpool to near Derby, is vague, and also without necessary details. It happened 50 years ago, and the only evidence offered as to the mode of the dogs' return is that "*it is said* they were seen swimming the Mersey." "N. Y.'s" case (April 24) of the dog who "did not make haste back," and therefore could not have returned by smell, is also most inconclusive. The distance was only 20 miles, and we know nothing of the route the dog followed, or the time it took. How do we know the dog did not wait the three weeks till it saw someone it knew living at or near its former house, and followed that person? This appears to me to be an exceedingly probable way of accounting for many of these returns where the distance is not very great. This brings me to the case of Mr. Geo. R. Jebb, who seems to have gone to the trouble of making an experiment which, with a little more trouble, might have been very complete and satisfactory. The dog was taken by rail very circuitously from Chester to a place 10 miles from Chester. It "hung about the station for about an hour and a half," and in three hours more arrived at its home. But we are still left totally in the dark, both as to the route it took or the process by which it decided on that route. What is required in such experiments is, that a person not known to the dog should be ready to watch and follow it (on horseback), noting carefully on the spot its every action. We should then perhaps know why it "hung about the station" an hour and a half before commencing its journey home, and afterwards, whether it showed any hesitation as to its route, and whether it followed the road or went straight across country. A few experiments carefully made in this way, at distances varying from 10 to 30 miles, and with a thorough knowledge in each case of the animal's antecedents, would, I venture to say, throw more light on this interesting question than all the facts that have been yet recorded. The only experiment of this kind I have met with is in the work of Houzeau ("Etudes sur les Facultés Mentales des Animaux"), and it is so curious that I give the passage literally. He says (vol. i. p. 156): "I have succeeded in making young dogs of five or six months lose themselves on first going out with me. They would begin by seeking for my trace by smell; but not succeeding in this, they would decide to return home. If there was a path, they followed the route by which they had come. If it was an untrodden virgin country, they shortened the circuits they had made in coming, but did not altogether depart from them. One would say that memory furnished a certain number of points which divided the route, and they went towards these by memory of directions. Thus inscribing chords to the curve by which they had come, they returned to the house." M. Houzeau's general conclusion from a considerable body of observations made with this point in view is, that animals find their way by exactly the same means as man does under similar circumstances, that is, by the use of all their faculties in observation of locality, but especially by a memory of directions and by a ready recognition of places once visited, which serve as guide-posts when they are again met with. This seems to me a very sound theory, and quite in accordance with all that is known of the manner in which savages find their way.

The more general objections to my little theory which are made in your leading article appear to depend on the denial, to such animals as dogs and horses, of that amount of common sense and reasoning power which I believe them to possess, and also to the assumption that in the case supposed they would recollect merely the odours, not the objects the presence of which these odours had indicated. I imagine that animals know, just as well as we do, that some sights, sounds, and smells are caused by permanent, others by evanescent or changeable causes. The smell or sound of a flock of sheep would indicate to a dog the presence of an actual flock of sheep, just as surely as the sight of them would do, and he would no more lose his way because those sheep were not in the same place the next day or the next week, than he would had he travelled the road on foot with his eyes open. The smell of a wood, of a farmyard, of a ditch, a village, or a blacksmith's shop, with the more or less characteristic sounds accompanying these, would tell the dog that corresponding objects were there just as surely as the sight of them would do. On his return he would recognise the objects, not the smells and sounds only, and he would be no more puzzled by the absence of certain moveable objects he had recognised by smell than he would be had he seen them. I quite believe that mistakes would often be made owing to the discontinuousness of sufficiently characteristic odours; but the process of "trial and error," suggested by F.R.S., would be constantly used, and this is in accordance with the length of time usually taken in these journeys, often very much longer than would be required for a return by the shortest route and at moderate speed.

A friend has communicated to me a most remarkable fact, of a different character from any which have been referred to during the course of this discussion; and as I have it at first hand and took the exact particulars down as narrated to me, I think it will be of value. Many years ago, my friend lost a favourite little dog. He was then living in Long Acre. Three months after, he removed to a house in another street about half a mile off, a place he had not contemplated going to or even seen before the loss of the dog. Two months after this (five months after the dog was lost) a scratching was one day heard at the door, and on opening it the lost dog rushed in, having found out its master in the new house. My friend was so astonished that he went next day to Long Acre to an acquaintance who lived nearly opposite the old house (then empty) and told him his little dog had come back. "Oh," said this person, "I saw the dog myself yesterday. He scratched at your door, barked a good deal, then went to the middle of the street, turned round several times, and started off towards where you now live." My friend cannot tell, unfortunately, what time elapsed between the dog's leaving the old and arriving at the new house. If every movement of this dog could have been watched from one door to the other, much might have been learnt. Could it have obtained information from other dogs (and that dogs can communicate information is well shown by Mr. A. P. Smith's anecdote in your issue of three weeks back)? Could the odour of persons and furniture linger two months in the streets? These are almost the only conceivable sources of information, for the most thorough-going advocates for a "sense of direction" will hardly maintain that it could enable a dog to go straight to its master, wherever he might happen to be.

Not to trespass further on your space, I would venture to hope that some persons, having means and leisure, would experiment on this subject in the same careful and thorough way that Mr. Spalding experimented on his fowls. The animals' previous history must be known and recorded; a sufficient number of experiments, at various distances and under different conditions, must be made, and a person of intelligence and activity must keep the animal in sight, and note down its every action till it arrives home. If this is done I feel sure that a satisfactory theory will soon be arrived at, and much, if not all the mystery that now attaches to this class of facts be removed.

ALFRED R. WALLACE

The Origin of Volcanic Products

I HAVE not yet had the advantage of seeing Mr. Mallet's translation of Palmieri's late work on Vesuvius, but have read with interest Mr. Forbes's review thereof and Mr. Mallet's reply in NATURE of Feb. 6 and March 20. I have no desire to enter into a controversy, but as I have for the past fifteen years taught and defended a theory of the origin of volcanic products identical with that now maintained by Mr. Mallet, I may be permitted to say a few words. That the source of all such matters was to be found not in the earth's nucleus but in sedimentary strata, was taught by Referstein in his *Naturgeschichte des Erdkörpers*, in 1834; and again, doubtless independently, by Sir J. F. W. Herschel in 1837; while, for my own part, I was led to the same conclusion before I became aware of the views of either of my predecessors, solely from a consideration of the varying composition of plutonic rocks and of the stony and vaporous products of volcanic action. To the views of Herschel I first called attention in the *Canadian Journal* for March 1858, and again in the *Quar. Geol. Journ.* for November 1859, pp. 488–496, § vii.).

In the first of these I have said: "If we admit that all igneous rocks, ancient plutonic masses, as well as modern lavas, have their origin in the liquefaction of sedimentary strata, we can at once explain the diversities in their composition. We can also understand why the products of volcanoes in different regions are so unlike, and why the lavas of the same volcano vary at different periods. We find an explanation of the water and carbonic acid, which are such constant accompaniments of volcanic action, as well as the hydrochloric acid, sulphuretted hydrogen, &c." The nature of the reactions between silicious, calcareous, and aluminous strata, holding carbonaceous matter, gypsum, sea-salt, &c., was then discussed, and the products of their transformations under the influence of water at an elevated temperature considered. In both of these papers referred to, the inadequacy of the views of Phillips, Durocher, and Bunsen, to explain the origin of these various products, was maintained.

In the *Geological Magazine* for June 1869, I returned to this subject in a paper on "The Probable Seat of Volcanic Action," where, after repeating and enforcing the above views, I said: "Two things become apparent from a study of the chemical nature of rocks; first, that their composition presents such variations as are irreconcileable with the simple origin generally assigned to them; and second, that it is similar to that of the sedimentary rocks whose history and origin it is, in most cases, not difficult to trace." In what follows I endeavour to show in the latter the source of such "eruptive rocks as peridolite, phonolite, leucitophyre, and similar rocks, which are so many exceptions in the basic group of Bunsen."

Mr. Mallet has, however, made a very important advance in this theory of volcanic action by pointing out a source of heat independent of the cooling nucleus. Referstein had supposed heat to be generated by chemical action in the sediments, and his view has lately been brought forward, in a modified form, by Leconte; but this I have always rejected as untenable. The chemical actions supposed to be involved in the processes would consume rather than generate heat. I have hitherto followed Herschel and Babbage in regarding the heat as directly derived by conduction from an incandescent nucleus, but Mr. Mallet has now shown that the work expended in the crushing of the strata which takes place in certain regions of the globe where the contraction which attends the slow refrigeration of the globe is displayed in corrugations of the crust, is more than adequate to explain volcanic heat. To this it must be added that, inasmuch as the crushing process takes place in strata which, from their depth, are already at an elevated temperature, the heat developed by the mechanical process comes in to supplement that derived by conduction from the igneous centre. Vose had already, in a general manner, pointed out the same thing, suggesting in terms which are, it is true, wanting in scientific precision, the notion that the mechanical force at work in the crushing of the strata was the source of heat. This, however, in no way detracts from the great merit of Mr. Mallet, who may rightly claim "to have been the first to apply weight, measure, and number to volcanic theory," and we await with great interest the publication of his quantitative results. Apart from his thermo-dynamic theory, however, his views of volcanic action are apparently identical with those of Referstein and Herschel, to which I have for many years been endeavouring to give form and consistency. I may here call attention to a paper, "On some Points of Dynamical Geology," published in the *American Journal of Science* for this month (April 1873), in which I have already alluded to the foregoing questions, and to the endeavours which I have for fifteen years been making "to reconstruct the theory of the earth on the basis of a solid nucleus." I have there rehearsed the views which I have all this time maintained as to the causes which determine the process of corrugation of the earth's crust, the accumulation of sediments, and the development of volcanic activity in certain regions of the earth; thus giving a theory of the geological and geographical distribution of past and present volcanoes.

T. STERRY HUNT

Institute of Technology, Boston, Mass., April 25

Kinetic Theory of Gases

ON page 300 of the second edition of Maxwell's excellent little text-book on the "Theory of Heat," it is stated, as a result of the kinetic theory of gases therein set forth, that "gravity produces no effect in making the bottom of the column" (of gas) "hotter or colder than the top."

I cannot see how this result follows from the kinetic theory of gases. On the contrary, it seems obvious that thermal equilibrium can only subsist according to the kinetic theory, where the molecules encounter each other with equal average amounts of *work* or *vis viva*, and in order that this may be the case, the velocity of the molecules (and consequent temperature) of any upper layer must be less than that of the molecules in the layer next below; since, in order to encounter each other, the former must descend, and acquire velocity, while the latter must ascend and lose it. This would establish a diminution of temperature from the bottom to the top of a column of air at the rate (in the absence of any counteracting cause) of 1° F. for 113 ft. of height, as can easily be verified from the fact that on account of the specific heat of air 1 lb. requires 183 foot-pounds to raise its temperature 1° F. Radiation may diminish this and tend to produce equilibrium, but nevertheless it seems obvious from these two opposing tendencies a residual inequality of thermal condition would result, and that the top of a column would be cooler than the bottom. That this would be the case if the air were in general motion in the form of upward and downward currents, will not, I presume, be disputed; and surely molecular is on the same footing. If the particles of air are moving in every direction with great absolute velocity, in what respect does this differ from air currents? In fact, all the particles which at any epoch of time are moving in any given direction constitute an air-current in that direction, mingled, it is true, with currents in other directions, but moving with accelerated velocity if descending, and with retarded velocity if ascending, and thus always tending to produce a diminution of temperature with height as a condition of gaseous thermal equilibrium.

J. GUTHRIE

Graaf Reinet, Cape Colony, April 2

Kerguelen Cabbage

I WOULD like to know, through your paper, whether the naturalists of the *Challenger* have orders to attempt to collect the seeds of the Kerguelen Land cabbage (*Pringlea antiscorbutica*). It has often occurred to me that the attempt ought to be made to introduce this plant on the seashores of Northern Europe and America.

JOHN R. JONES

Milwaukee, Wisconsin, U.S. April 14

Yorkshire Terrier Story

THE anecdote of the instinct of dogs given in the number of NATURE, May 1, p. 6, is identical with one to be found in Bewick's "History of Quadrupeds," p. 367, 1800, which he calls the well-known story of the "Dog at St. Alban's."

The same story precisely, with some dramatic embellishments and names, occurs in "Bingley's Animal Biography," vol. 1, p. 223.

A.

Dorking

BICHROMATE PHOTOGRAPHS

A SINGULAR discovery has recently been made touching the action of light upon substances rendered sensitive by the bichromates of potash and ammonia, which threatens to revolutionise photographic printing altogether, at any rate so far as the production of permanent prints is concerned. The printing by means of silver salts in the ordinary way, which is still in vogue with nearly all portrait photographers, will always find application, by reason of the simplicity of the manipulations and the delicate and pleasing nature of the results, albeit all silver photographs enjoy the unenviable notoriety of being perishable. First of all, they lose their pristine brilliancy and freshness, then a sickly yellowness gives place to the glossy whites of the picture, and finally the deep bronze shadows become of a flat brownish tint, which grows weaker and weaker as time goes on. To secure permanent photographs, which shall possess all the beauty and detail exhibited by silver prints, has been for many years the aim of photographic experimenters, and it was not until Swan and Johnson had contributed their well-known improvements that the production of a delicate photograph in permanent pigments became at all possible. Mechanical photographic processes, where the pictures are printed off in a press, are still beset with many difficulties of a practical nature, the most perfect of them—Woodburytype—requiring further elaboration before perfect prints of large dimensions can be secured.

Pigment photographs, or carbon prints, as they are generally termed, require three elements for their production—a pigment (such as Indian-ink, lamp-black, or some such substance), gelatine, and bichromate of potash, or ammonia. A compound of these three substances is spread upon paper, and termed pigment or carbon tissue. This tissue is printed under a transparent negative in the sun, the light acting more or less energetically upon the sensitive pigment, and rendering it insoluble in parts, so that when it is immersed subsequently in warm water certain portions refuse to wash away, and these form the image; during the exposure of the tissue to light, these parts have in fact become fixed by its action. This, as we all know, is what takes place in the formation of a carbon print.

It has been found that the action of light upon a bichromate film is very different in its nature to the result produced by the sun upon iodide of silver. A film of pure iodide of silver, as Dr. Reissig and Mr. Carey Lea have abundantly shown, may be impressed with an image which will fade out altogether if the film is afterwards preserved for a sufficient time screened from light. Indeed it is possible to impress iodide of silver with an image, allow the same to fade away in darkness, and then impress the film with a second and different picture. The photographic image, therefore, on iodide of silver is of an evanescent nature, becoming weaker and weaker, and, if preserved for any time, ultimately fading away altogether. Now, with a photograph upon a bichromate film, the reverse is the case. If an impression of the slightest kind is produced upon a film of gelatine sensitised with bichromate, and put away in the dark, the action of the light still goes on, and progresses until the image has become a perfect and vigorous one. This continuation of the solar action has been turned to good account by carbon printers, who in winter time and busy moments have printed their photographs in darkness instead of light; that is to say, in lieu of exposing their sensitive tissue in the sun under a negative for hours and hours, they merely do so for a few minutes, the slight image thus impressed being allowed to gain in vigour subsequently by preservation for some time—half-a-day or so—in darkness, before development in warm water. In the ordinary way only half-a-dozen copies can be obtained from one negative during the day, if all of them are fully printed in the sun, whilst if only incipient prints are produced, a score of impressions may easily be secured.

Within the last few days we have progressed a step further in carbon printing. M. Marion of Paris has discovered that if you take a bichromate image printed in the sun, and put it into contact with another bichromate surface, you produce upon the latter a similar impression. You can in fact take a carbon picture fresh from the frame and employ it as a printing block, from which any number of impressions are procurable. It is a most singular fact that a solarised surface should be capable of setting up an action upon another sensitive surface placed in contact with it. But so it is. The impression made by light upon a bichromate film is capable of transmission to another surface of like nature merely pressed against it. We have, as it were, stored up in the original print a quantity of sunlight which has been absorbed and may afterwards be communicated to other surfaces.

The importance of this discovery can scarcely be overrated, and there is no doubt but that it will work an era in the matter of carbon printing. We need secure but one single photograph printed in the sun in order to obtain a large number of copies, all of which shall be as delicate and vigorous as if they had been printed by sunlight. A sheet of gelatine sensitised with bichromate of potash is put under a negative and printed; it is withdrawn from the printing frame and immersed in a weak solution of bichromate of potash which swells up those portions of the surface that have not been attacked by light, and thus produces a picture in relief. The sheet of gelatine is then put into a press and impressions from it taken on sensitive carbon tissue, the block being moistened from time to time with bichromate solution. The copies thus produced upon the tissue are not fully printed and cannot be developed at once; they are simply incipient, or nascent, pictures, it must be mentioned, and they require preservation in the dark for some hours to allow the action of the light to continue, exactly in the same way as if the carbon tissue had been exposed to sun-light for a few minutes. When the prints have been kept sufficiently they are developed in warm water, and fine vigorous copies are the result. Naturally enough if the tissue is kept too long after, the mordant action of the light continues rendering the film insoluble, and then the development of the image in warm water obviously becomes impossible.

Another application of the same principle has been made by M. Marion, in which carbon printing is assimilated to silver printing, to such a degree, that those accustomed to the ordinary method of printing photographs on albumenised paper, would find no difficulty in adopting it.

H. BADEN PRITCHARD

ON THE METHODS OF COLLECTING AND PRESERVING ENTOMOSTRACA AND OTHER MICROZOA

CONSIDERING the varied interest which attaches to the Entomostraca, it has long seemed to me that they attract a remarkably small share of attention from microscopists. In the case of so widely distributed and numerous a group, this cannot arise from any real difficulty in procuring materials for study; but I believe it does arise in great measure from a want of information as to the best means of capturing and preserving specimens. I propose, therefore, briefly to point out some of the methods which in my own hands have best answered these ends.

Classification.—The Entomostraca constitute, as all microscopists know, a division of the class Crustacea, and for the purposes of the present paper we may with sufficient approach to accuracy consider them as forming four groups—*Cladocera*, of which the common *Daphnia*, or water-flea, is the type; *Ostracoda*, typified by the little hard-shelled, bivalve, mollusc-like *Cypris*; *Copepoda*, represented by the well-known *Cyclops*; and the parasitic species, *Pœcilopoda*, commonly known under the name "fish-lice."

Respecting the last-named group, I shall have nothing to say here; the mere knowledge of their mode of life indicates the method of capture.

Habitat.—All collections of still-water, large and small, from the mere road-side pool to the mountain lake and the ocean, support, with scarcely an exception, their quota of entomostracan inhabitants; nor is purity an essential condition of their existence, for sometimes they are found in great numbers when one would think the foulness of the medium too much for animal existence of so high a grade. Doubtless, however, a moderate purity of water is necessary to the presence of any great variety of species; a luxuriant aquatic vegetation is also very favourable to the growth of most Entomostraca, affording them probably not only food, but shelter. For this reason the weedy margins of lakes are as a rule much more prolific than the clear central portions, where, indeed, but little microscopic life usually exists. Rapidly flowing water is of course unfavourable to the existence of these organisms, but the sea, both between tide-marks and in the open, abounds with them. Ostracoda, except the fresh-water Cyprides, live for the most part on the bottom, and are therefore to be obtained chiefly by dredging. The brackish water of salt-marshes and estuaries supports its own peculiar species, some of which often occur in prodigious numbers; and even the highly saline waters of brine springs and salt lakes have been found to contain Entomostraca.

Methods of Collecting

1. *Freshwater.*—An ordinary "ring-net," made of "hard muslin," or "crinoline," from six to twelve inches in diameter, and fitted to the end of a walking-stick, will be found the most convenient apparatus for the capture of such swimming species as haunt the weedy margins of ponds and lakes. For such shallows as are matted with a growth of *Littorella*, *Lobelia*, or other dwarf ground-plants a "horse-shoe" net, with a frame made after the fashion of a Dutch hoe, is very serviceable; while in working from a boat in the centre of a lake the ordinary ring-net on a stick will be quite sufficient. In this way the net will, after working for a few minutes, usually be partially filled with fragments of weed and other *débris*, amongst which there will also be found a fair sample of the Microzoa inhabiting the locality. The coarsest fragments, such as stems of rushes and portions of water weeds, may conveniently be picked out with the fingers, and thrown away, while the rest of the contents of the net must be transferred to a bottle of clear water, an eight-ounce being a convenient size for the purpose. The Microzoa may then be readily separated by filtering into another bottle through a net of sufficiently wide mesh to allow of their passage through it: "mosquito-netting" I have found to answer well for this purpose. Having thus obtained our Entomostraca in a condition tolerably free from admixture with extraneous matter, they may easily be collected in a patch on the centre of a piece of fine muslin by passing the whole through a piece of that material, arranged over a funnel. They should then be transferred at once (if it be not wished to keep them alive) to a small phial of some preservative fluid. This may be effected easily by a penknife, but a very convenient instrument for the purpose is an ordinary quill toothpick. This process, which appears somewhat cumbrous in writing, is in reality very easily performed, but it may be still further simplified, according to the fancy of the collector, by fitting an outside funnel with a muslin net, and having a small inner one of perforated zinc, so as to do all the filtering at one operation. The collecting net may also be protected from the entrance of very coarse rubbish by a light, moveable wire grating. The species obained by these means will often include numerous representatives of all three orders, Cladocera, Ostracoda, and Copepoda. For the capture of such Ostracoda as haunt the bottom in parts too deep to be reached by a walking-stick, a small hand-dredge is required: this will be more particularly noticed in the marine section.

2. *The Sea.*—The free-swimming species, the great majority of which belong to the order Copepoda, may be most conveniently captured by the walking-stick net held over the side of a row-boat in gentle motion. Care should be taken that the lower end of the net is as wide or wider than its mouth, and that the material, while close enough to retain the Entomostraca, is yet open enough to allow a free current of water through it: if those points be not attended to the result will be a back-wash, carrying back out of the net much which should have been retained.

A towing-net dragged by means of a line from the side or stern of the boat may be used, but is not so much under control, and seldom produces so much spoil: such a net, however, attached in a tide-way during the night to some stationary object, and made with the precautions mentioned above, will often do good work, especially if its specific gravity be adjusted so as to sink very slightly below the surface. As a rule, indeed, the hours from dusk to midnight seem to be the best for capturing pelagic species near the surface. In tidal pools on the shore the same appliances are required as for fresh-water ponds.

Ostracoda and other deep-dwelling species require, of course, the use of the dredge; and where Microzoa only are the objects sought, the dredge may conveniently be made of a size much smaller than those in ordinary use. The mouth need not be more than 6 in. in its largest diameter, the bag being made of coarse canvas or "cheese cloth," and from 18 in. to 2 ft. long. The material so dredged up, after having been passed through suitable sieves, so as to separate the coarser portions, should be washed in a muslin bag for the purpose of removing all the impalpable mud, which often constitutes a very considerable proportion of the bulk: this operation may most easily be performed over the side of the boat in the sea, or in some large vessel of sea-water. The washed material is then to be put up in canvas bags, duly labelled, and hung up in a warm position to dry; the more rapidly this part of the process is conducted the better chance will there be of preserving the internal parts, as well as the valves of the Ostracoda, in good condition. But should it be wished to secure the animals actually alive, the best plan will be, after washing the mud as above explained, to immerse a quantity of it in a basin of sea water, allowing it to stand for an hour or more, when many of its inhabitants will have made their way to the surface of the water. They will, indeed, continue to come to the surface for many hours, but the later ones will probably be sickly or dead.

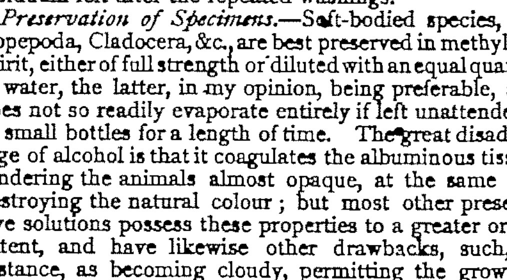
But besides Ostracoda, there are often great numbers of Copepoda in or on the ooze and sand of the sea-bed. These require for their separation a different method of procedure; the following, so far as I know, being the most convenient. After the process of sieving described in the preceding paragraph, all the minute swimming animals will be found in the water in which that operation has been conducted; all that is necessary, therefore, is to pour the water off through a muslin net in which the Microzoa will be retained—in a dirty state, however, which will render careful washing desirable, or still better, the transference of the whole to a bottle of clean sea water for an hour or two; in this way the little creatures will clear themselves of adherent dirt better than we can do by any amount of washing.

A very rich field for the collecting of Copepoda is found in the groves of Fuci and Laminariæ so common on rocky shores at and beyond low-water mark. The fronds of these weeds having been dragged up in any convenient way, are to be washed, a handful or two at a time, by brisk agitation in a tub of sea water, after which the water is to be filtered as directed above. It is best not to macerate weeds in the water for any great length of time, because much mucus exudes from the Laminariæ, enveloping the Entomostraca, and rendering it an extremely difficult and tedious matter to examine the gathering properly. It should be mentioned that, although all weeds harbour numbers of Entomostraca, *Laminaria saccharina* is, as a rule, by far the most productive, apparently on account of the rugosities of the frond affording more efficient shelter to their minute inhabitants: sheltered pieces of coast and land-locked bays are much the most productive hunting grounds.

Treatment of Dredged Material.—The separation of Ostracoda, Foraminifera, and other Microzoa from dredged sand or mud, is best accomplished by the process of "floating." For this purpose the material should be thoroughly well dried and sifted, so as to insure the fine division of the whole mass, then placed in a vessel of water and thoroughly stirred. By this means all the lighter organised particles—chiefly Ostracoda, Foraminifera, minute Mollusca, fragments of Polyzoa, &c.—will, owing to their contained air, be brought to the surface, and may be removed in any convenient way, but best, perhaps, by pouring off the supernatant water through a very fine gauze sieve. Some of the larger and heavier species will, however, sometimes remain at the bottom, and must be picked out with the help of a hand lens.

Fossiliferous Clays and Shales.—These, after repeated maceration in water, should be passed, time after time, through fine sieves, so as to wash out the impalpable suspended mud; at last drying the residuum and floating out the organic particles, as previously directed. When much fossilised, however, the Microzoa will not float. In this case they must be picked out one by one from the residuum left after the repeated washings.

Preservation of Specimens.—Soft-bodied species, *e.g.*, Copepoda, Cladocera, &c., are best preserved in methylated spirit, either of full strength or diluted with an equal quantity of water, the latter, in my opinion, being preferable, as it does not so readily evaporate entirely if left unattended to in small bottles for a length of time. The great disadvantage of alcohol is that it coagulates the albuminous tissues, rendering the animals almost opaque, at the same time destroying the natural colour; but most other preservative solutions possess these properties to a greater or less extent, and have likewise other drawbacks, such, for instance, as becoming cloudy, permitting the growth of fungi, &c. When, however, it is especially wished to preserve the colours, a mixture of equal parts of glycerine and distilled water answers admirably. Indeed, the only hindrances to its general use as a preservative for Microzoa are its strongly solvent action on calcareous tissues and its inconvenient stickiness. For microscopic mountings (of non-calcareous objects) some kind of "glycerine jelly" answers admirably; especially that described by Dr. Carpenter in his book on the microscope, which preparation is, however, improved by saturating with arsenious acid the water used in its manufacture. Ostracoda and other dry specimens require, of course, no preparation beyond mounting on slides of wood or cardboard. An excellent plan of mounting, so as to show at one view all the Ostracoda or Foraminifera obtained in any locality, is shown in the accompanying diagram, the slides being made of the ordinary size, of stout cardboard or millboard. The central part of the slide is cut out, and the marginal portion mounted on another slide having a dull black ground. The slide is ruled transversely, so as to divide it into any convenient number of spaces, and if needful, ruled also with one line lengthwise down the middle. Each space is marked with a figure or letter of the alphabet referring to the species mounted within it, and an index to the whole kept in a book of reference. The diagram is a facsimile of a mounting so prepared in my collection.


OFF MARSDEN, 10 FATHOMS.
V W X Y Z 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16
B.
3.
a b c d e f g h i j k l m n o p q r s t u
MUDDY SAND.

GEORGE S. BRADY

ON THE ORIGIN AND METAMORPHOSES OF INSECTS*

IV.

ON THE NATURE OF METAMORPHOSES

IN the preceding articles we have considered the life history of insects after they have quitted the egg. It is obvious, however, that to treat the subject in a satisfactory manner we must take the development as a whole, from the commencement of the changes in the egg, up to the maturity of the animal, and not suffer ourselves to be confused by the fact that all insects do not leave the egg in the same stage of embryonal development. For although all young insects when they quit the egg are termed "larvæ," whatever their form may be (the case of the so-called Pupipara not constituting a true exception), still it must be remembered that some of these larvæ are much more advanced than others. It is evident that the larva of a fly, as regards its stage of development, corresponds in reality neither with that of a moth nor with that of a grasshopper. In fact, insects quit the egg in very different stages. The maggots of flies, in which the appendages of the head are rudimentary, belong to a lower grade than the grubs of bees, &c., which have antennæ, mandibles, maxilla, labrum, labium, and, in fact, all the mouth parts of a perfect insect. The caterpillars of Lepidoptera are generally classed with the vermiform larvæ of Diptera and Hymenoptera, and placed in opposition to those of Orthoptera, Hemiptera, &c. But, in truth, the possession of thoracic legs places them, as well as the similar larvæ of the Tenthredinidæ, on a decidedly higher level, while in the development of the cephalic appendages there is, as already mentioned, a marked difference between the maggots of flies and the grubs of bees. Thus, then, the period of growth (that in which the animal eats and increases in size) occupies sometimes one stage in the development, sometimes another; sometimes, as for instance in the case of *Chloëon*, it continues through more than one, or, in other words, growth is accompanied by development. But, in fact, the question is even more complicated than this. It is not only that the larvæ of insects at their birth offer the most various grades of development, from the grub of a fly to the young of a grasshopper or a cricket; if we were to classify larvæ according to their development, we should have to deal not with a simple case of gradations only, but with a series of gradations, which would be different according to the organ which we took as our test.

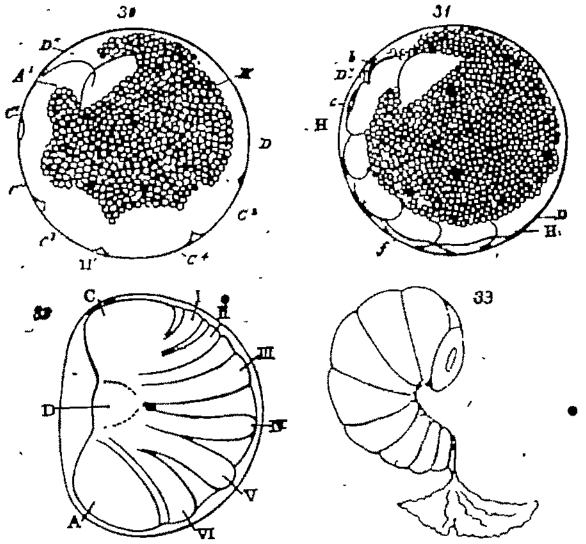


FIG. 30.—Egg of Phryganea (Mystacides). A^1, mandibular segment; C^1 to C^5, maxillary, labial, and three thoracic segments; *D*, abdomen. (after Zaddach). 31, Egg of Phryganea somewhat more advanced *b*, mandibles; *c*, maxillæ; *c f s*, rudiments of the three pairs of legs 32, Egg of Pholcus opilionides (after Claparède). 33, Embryo of Julus after Newport).

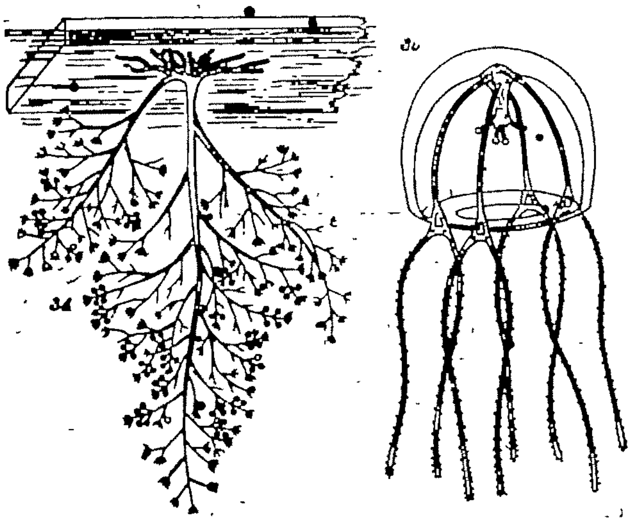


FIG. 34.—Colony of Bougainvillea fruticosa, natural size, attached to the underside of a piece of floating timber (after Allman). 36, The medusa from the same species.

FIG. 35.—Portion of Colony of Bougainvillea fruticosa, more magnified.

Apart, however, from the adaptive changes to which special reference was made in a previous article, the differences are those of gradation, not of direction. The development of a grasshopper does not pursue a different course from that of a bee or wasp, but the embryo attains a higher state before quitting the egg in the former than in the latter; while in most Hymenoptera the body-walls and internal organs are formed before the thoracic appendages; in the Orthoptera, on the contrary, the legs make their appearance before the body-walls have completely closed round the yolk.

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* Continued from p. 31.

Prof. Owen,* indeed, goes so far as to say that the Orthoptera and other Homomorphous insects are, "at one stage of their development, apodal and acephalous larvæ, like the maggot of the fly; but, instead of quitting the egg in this stage, they are quickly transformed into another, in which the head and rudimental thoracic feet are developed to the degree which characterises the hexapod larvæ of the *Carabi* and *Petalocera*."

I quite believe that this was originally true of such larvæ, but from the tendency which large and important organs have, to appear at an early stage of embryonal development, the fact now appears to be, so far at least as can be judged from the observations yet recorded, that the legs of those larvæ which commence life with these appendages, generally make their appearance before the body-walls have closed, or the internal organs have approached to completion. Indeed when the legs first appear they are merely short projections, which it is not always easy to distinguish from the segments themselves. It must, however, be admitted, that the observations are neither so numerous, nor in most cases so full, as could be wished.

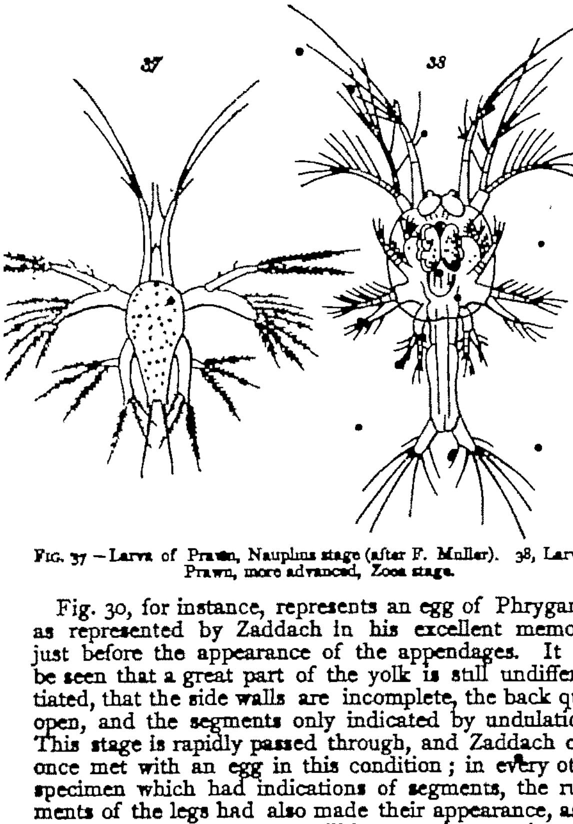

FIG. 37—Larva of Prawn, Nauplius stage (after F. Muller). 38, Larva of Prawn, more advanced, Zoea stage.

Fig. 30, for instance, represents an egg of Phryganea, as represented by Zaddach in his excellent memoir,† just before the appearance of the appendages. It will be seen that a great part of the yolk is still undifferentiated, that the side walls are incomplete, the back quite open, and the segments only indicated by undulations. This stage is rapidly passed through, and Zaddach only once met with an egg in this condition; in every other specimen which had indications of segments, the rudiments of the legs had also made their appearance, as in Fig. 31, which, however, as will be seen, does not in other respects show much advance on Fig. 30.

Again in Aphis, the embryology of which has been so well worked out by Huxley,‡ the case is very similar, although the legs are somewhat later in making their appearance. "In embryos," he says, "$\frac{1}{17}$th of an inch in length (Pl. xxxvii. Fig. 6), I have found the cephalic portion of the blastoderm beginning to extend upwards again over the anterior face of the germ, so as to constitute its anterior and a small part of its superior wall. This portion is divided by a median fissure into two lobes, which play an important part in the development of the head, and will be termed the "procephalic lobes." I have already made use of this term for the corresponding parts in the embryos of *Crustacea*. The rudimentary thorax presents traces of a division into three segments; and the dorso-lateral margins of the cephalic blastoderm, behind the procephalic lobes, have a sinuous margin. It is in embryos between this and $\frac{1}{100}$th of an inch in length, that the rudiments of the appendages make their appearance, and by the growth of the cephalic, thoracic, and abdominal blastoderm, curious changes are effected in the relative position of those regions."

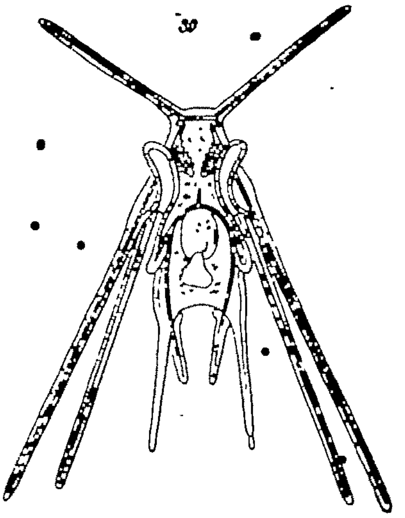

FIG. 39—Larva of Echinocidaris, seen from above × $\frac{1}{15}$ (after J. Muller).

In *Chrysopa oculata*, one of the Hemerobiidæ, Packard has described* and figured a stage in which the body segments have made their appearance, but in which "there are no indications of limbs. The primitive band," he says, "is fully formed, the protozonites being distinctly marked, the transverse impressed lines indicating the primitive segments being distinct, and the median furrow easily discerned." Here also, again, the dorsal walls are incomplete, and the internal organs as yet unformed.

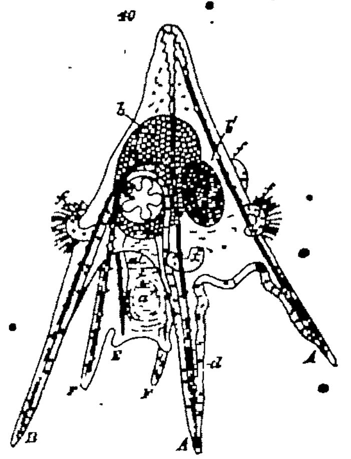

FIG. 40.—Larva of Echinus, × 100 *A*, anus, *F*, mouth process, *B*, posterior side arm; *F*$_D$ accessory arm of the mouth process; *a*, mouth; *a*^{1}, oesophagus, *b*, stomach; *b*^{1}, intestine; *e*, posterior orifice; *d*, ciliated bands, *f*, ciliated epaulets; *c*, disc of future Echinus (after J. Muller).

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* "Lectures on the Anatomy, &c. of the Invertebrate Animals."

† "Untersuchungen uber die Entwickelung und den Bau der Gliederthiere." 1854.

‡ "Linnean Transactions," v. xxii 1858.

* "Embryological Studies on Hexapodous Insects." Peabody Academy Science. Third memoir.

In certain Dragonflies (Calypteryx), and Hemiptera (Hydrometra), the legs, according to Brandt,* appear at a still earlier stage.

According to the observations of Kolliker† it would appear that in Donacia the segments and appendages appear simultaneously. Kolliker himself, however, admits that "meæ de hoc insecto observationes satis sunt mancæ," and it is possible that he may never have met with an embryo in the state immediately preceding the appearance of the legs.

On the whole, as far as we can judge from the observations as yet recorded, it seems that in Homomorphous insects the ventral wall is developed and divided into segments before the appearance of the legs, but that the latter are formed simultaneously, or almost simultaneously, with the cephalic appendages, and before either the dorsal walls or the internal organs.

As it may be interesting from this point of view to compare the development of other Articulata with that of insects, I give a figure (Fig. 32) representing one of the early stages in the development of a spider (Pholcus) after Claparede,‡ who says, "C'est à ce moment qu'a lieu la formation des *protozonites* ou segments primordiaux du corps de l'embryon. Le rudiment ventral s'épaissit suivant six zônes disposées transversalement entre le capuchon anal et le capuchon céphalique. L'œuf considéré par sa face ventrale offre alors un contour à peu près circulaire et on peut le croire sphérique. Les zônes se montrent alors comme six circles d'un blanc plus éclatant, tracés sur la sphère."

Among Centipedes the development of Julus has been described by Newport.§ The first period, from the deposition of the egg to the gradual bursting of the shell, and exposure of the embryo within it, which, however, remains for some time longer in connection with the shell by a distinct funis, lasts for twenty-five days. The segments of the body, originally six in number, make their appearance on the twentieth day after the deposition of the egg, at which time there were no traces of legs. The larva when it leaves the egg is a soft, white, legless grub (Fig 33), consisting of a head and seven segments, the head being somewhat firmer in texture than the rest of the body. It exhibits rudimentary antennæ, but the legs are still only represented by very slight papilliform processes on the undersides of the segments to which they belong.

As already mentioned, I believe that at one time the vermiform state of the Homomorphous insects, which, as we have seen, is now so short, and passed through at so early a stage of development, was more important, more prolonged, and accompanied by a more complete condition of the internal organs. The compression, and even disappearance, of embryonal stages which are no longer adapted to the mode of life, which do not benefit the animal, is a phenomenon not without a parallel in other parts of the animal and even of the vegetable kingdom. Just as in language long compound words have a tendency to concision, and single letters sometimes linger on, indicating the history of a word, like the "l" in "alms," or the "b" in "debt," long after they have ceased to influence the sound; so in embryology useless stages, interesting as illustrations of past history, but without direct advantage under present conditions, are rapidly passed through, and even, as it would appear, in some cases altogether omitted.

For instance, among the Hydroida, in the great majority of cases, the egg produces a body more or less resembling the common Hydra of our ponds, and known technically as the "trophosome," which develops into the well-known Medusæ or jelly-fishes. The group, however, for which Prof. Allman has proposed the term Monopsea,* and of which the genus Ægina may be taken as the type, is, as he says, "distinguished by the absence of a hydriform trophosome, the ovum becoming developed through direct metamorphosis into a medusiform body, just as in the other orders it is developed into a hydriform body." Figure 34 represents, after Allman, a colony of *Bougainvillea fruticosa* of the natural size. It is a British species, which is found growing on buoys, floating timber, &c., and, says Allman,† when in health and vigour, "offers a spectacle unsurpassed in interest by any other species—every branchlet crowned by its graceful hydranth, and budding with Medusæ in all stages of development (Fig. 35), some still in the condition of minute buds, in which no trace of the definite Medusa-form can yet be detected; others, in which the outlines of the Medusa can be distinctly traced within the transparent ectotheque; others, again, just casting off this thin outer pellicle, and others completely freed from it, struggling with convulsive efforts to break loose from the colony, and finally launched forth in the full enjoyment of their freedom into the surrounding water. I know of no form in which so many of the characteristic features of a typical hydroid are more finely expressed than in this beautiful species."

Figure 36 represents the Medusa form of this species, and the development thus described may be regarded as typical of the Hydroida; yet, as already mentioned, the Æginidæ do not present us with any stage corresponding to the fixed condition of Bougainvillea, but on the contrary are developed direct from the egg.

But on the other hand there are groups in which the Medusiform stage becomes less and less important.

Among the higher Crustacea again the great majority go through well-marked metamorphoses. Figs. 37 and 38 represent two stages in the development of the prawn. In the first (Fig. 37), representing the young animal as it quits the egg, the body is more or less oval and unsegmented, there is a median frontal eye, and three pairs of natatory feet, the first pair simple, the two posterior biramose. Very similar larvæ occur in various other groups of Crustacea.

They were at first regarded as mature forms, and O. F. Muller gave them the name of Nauplius. So, also, the second or Zoea form (Fig. 38) was at first regarded as a mature animal, until its true nature was discovered by Vaughan Thompson.

The Zoea form of larva differs from the perfect prawn or crab in the absence of the middle portion of the body and its appendages. The mandibles have no palpi, the maxillipeds or foot-jaws are used as feet, whereas in the mature form they serve as jaws. Branchiæ are either wanting or rudimentary, respiration being principally effected through the walls of the carapace. The abdomen and tail are destitute of appendages. The development of Zoea into the perfect animal has been well described by Mr. Spence Bate‡ in the case of the common crab (*Carcinus mœnas*).

All crabs, so far as we know, with the exception of a species of land crab (*Gegarcinus*), described by Westwood, pass through a stage more or less resembling that shown in Fig. 38. On the other hand the great group of Edriopthalma, comprising Amphipoda (shorehoppers, &c.) and Isopoda (woodlice, &c.), pass through no such metamorphoses; the development is direct, as in the Orthoptera. It is true that one species, *Tanais Dulongii*, though a typical Isopod in form and general character, is said to retain in some points, and especially in the mode of respiration, some peculiarities of the Zoea type; but this is quite an exceptional case. In Mysis, says F. Muller,§ "there is still a trace of the Nauplius-stage; being transferred back to a period when it had not to provide for itself, the Nauplius has become degraded into a mere skin; in *Ligia* this larva-skin has lost the traces of limbs, and in *Philoscia* it is scarcely demonstrable."

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* Mem. de l'Acad. Impé. des Sci. de St. Petersburg." 1869.
† Observationes de Prima Insectorum Genesi, p. 14.
‡ Recherches sur l'Evolution des Araignées.
§ Philosophical Transactions, 1841.

* Monog. of the Gymnoblastic or Tubularian Hydroida. By G. J. Allman, F.R.S., &c., Roy. Society. † l.c., p. 315.
‡ Philosophical Transactions, 1859, p. 589.
§ "Facts for Darwin," Eng. Trans., p. 127.

Once more, the Echinodermata in most cases "go through a very well-marked metamorphosis, which often has more than one larval stage. The distinctive character of the metamorphosis appears to be the possession by the larvæ of at least a mouth and pharynx, which, whether absorbed or cast off, is never converted into the corresponding organs of the perfect Echinoderm developed inside of the provisional organism. The mass of more or less differentiated sarcode, of which the larva, or pseudembryo, as opposed to the Echinoderm within it, is made up, always carries upon its exterior certain bilaterally-arranged ciliated bands, by the action of which the whole organism is moved from place to place, and it may be strengthened by the superaddition to it of a framework of calcareous rods."[*]

Thus Fig. 39 represents a larva of *Echino-cidaris*, after Muller;[†] The body is transparent, $\frac{1}{10}$ in length, shaped somewhat like a double easel, but with two long horns in front, which, as well as the posterior processes, are supported by calcareous rods. These larvæ swim by means of minute vibratile hairs, or ciliæ. They have a mouth, stomach, and in fact, a well-defined alimentary canal, but no nerves or other organs have yet been discovered in them. After swimming about in this condition for awhile, they begin to show signs of change. An involution of the integument takes place on one side of the back, so as to form a pit or tube, which continues to deepen till it reaches a mass or store of what is called blastema, or, as we may say, the raw material of the animal body. This blastema then begins to grow, and gradually assumes the form of the perfect Echinoderm. In doing so it surrounds and adopts the stomach of the larva, but forms for itself a new mouth or gullet, throwing off the old mouth, together with the intestine, the calcareous rods, and in fact all the rest of the body of the larva.

Fig. 40 represents a larva probably of *Echinus lividus*, from the Mediterranean, and shows the commencement of the sea egg within the body of the larva. The capital letters denote the different arms, a is the mouth, a' the œsophagus, b the stomach, b' the intestine, f the ciliated lobes or epaulets, c the young sea-egg.

JOHN LUBBOCK

(*To be continued.*)

EXTIRPATION BY COLLECTORS OF RARE PLANTS AND ANIMALS

THE Legislature, having very properly provided for the preservation of small birds, might extend its protection to other animals and to plants; for although it would be inexpedient to prevent individuals from taking rare insects and botanical specimens, it is surely expedient to deter persons or societies from offering premiums which are leading to the extirpation of such species.

Some years ago a judicious and formal protest against this culpable practice was published by many of the most eminent British botanists, and it has constantly been deplored by all true lovers of natural science. The respected president (the Rev. Dr. Mitchinson) of our East Kent Natural History Society, in his address at the last annual meeting thereof at Canterbury, made such strong observations on the subject as might raise the question whether local societies may not do as much harm by promoting the extirpation of rare plants and animals as good in other respects; and I have always been insisting, at the meetings of the same society and elsewhere, that it is our duty to cherish, and not destroy the precious plants and animals of the district. Whenever a rare plant or animal is exhibited at those meetings, we have always a wail about its having been "not long since often seen, though now fast disappearing." A chief cause of this is the deplorable rapacity of collectors of and traffickers in specimens; since the preposterous notion prevails that botany and entomology consist in a recognition of the mere physiognomy, without the least regard to the physiology, of species, and being able to call them by their scientific names.

And so it will be while local societies continue to encourage such errors, instead of promulgating the essential principles of botanical or entomological science, and obstructing the injurious operations of mere collectors or pretenders. And this desirable end, so far as regards taxonomy, might be easily attained without the least harm to rare species. Prizes for the best display, illustrated by microscopic drawings and preparations of the generic and specific characters of sections or the whole of many natural orders would afford really good tests of the industry and attainments of the candidates. For example, why not try for this purpose the Willows, Grasses, or Sedges? Two of these orders have the further recommendation of being of great economic value. Again, as specific distinctions seem to be the ultimate aim of these societies, certain cells or tissues, such as the pollen, epidermis, hairs, and stomata, would afford good subjects for investigation in this point of view, as would also raphides and other plant-crystals, and very likely disclose valuable characters not yet recognised in the books of systematic botany.

I have been led to these remarks by the increasing frequency of the practice now deplored. As the "West Kent Natural History, Microscopical, and Photographic Society" is much and deservedly respected, and exercises justly considerable influence in its department, an extract from its last "Council's Report," p. 19, will suffice as a sample of the mischief.—"With a view to promote the study of Entomology and Botany among the members of the Society and their families, the Council, in the early part of the year, announced their intention of giving two prizes of 5*l.* 5*s.* each, one for the best Botanical collection, the other for the best collection of Lepidopterous Insects; all specimens to be gathered or taken within the West Kent district." This quotation is by no means intended for blame to any particular society, but merely as an example taken from one of the printed "Reports" that has lately reached me of what is still being sown broadcast generally throughout the country.

And here we have plainly not only a reward of money for the best collection of plants and Lepidoptera in a given district, but a temptation or inducement to unscrupulous collectors, in their anxiety to win the prize and defeat their competitors, to destroy such rare specimens as they may not take away. Such nefarious conduct is not meant to be insinuated of the West Kent Society; but my object is simply to assert that which I know has too often been the effect of such prizes, and to invoke the aid of NATURE in suppressing the evil.

GEORGE GULLIVER

A FRENCH PHYSICAL SOCIETY

THE scientific movement increases in France; it began about the end of the Empire, under the ministry of Duruy, and has since taken greater proportions, especially after the last war. The new French Association for the Advancement of Science,[*] it is well known, is modelled after the British Association, the success of which has surpassed expectation.

The physicists of Paris have assembled for several years in the laboratories of the Superior Normal School, placed at their disposal by M. Berlin, the director of the scientific studies of this school. They conversed about physics recent theories were set forth, the new or little known instruments were shown and explained. Thus Sir Wm. Thomson's electrometer, and several experiments of Prof. Tyndall called forth the curiosity and attention of the assistants. But those amicable meetings are no longer sufficient; the necessity of a more formal gathering was felt, as well as of writing and publishing Transactions, that the notes and observations might not be completely lost. The members of the Institute of the physical section encouraged the new society by their warm approval.

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[*] "Rolleston—" Forms of Animal Life," p. 146.

[†] Uber die Gattungen der Seeigellarven. Siebente Abhandlung. Kon. Akad. d. Wiss. zu Berlin. Von Joh. Müller, 1855, Pl. iii. fig. 3.

[*] See NATURE, vol. v. p. 357.

On the 17th of January of the present year, in the Salle Gerson, an *annexé* of the Faculté des Sciences of Paris (*Sorbonne*), a number of physicists met. They accepted provisional statutes and elected a board. The provisional statutes proposed by a committee composed of MM. d'Almeida, Alfred Cornu, Gernez, Lissajous, Mascart, expressed, in a few articles, the basis of the new association.

The purpose of the society is to promote physics; it will have two sittings a month alternately with the Chemical Society, and will publish transactions that will be sent to the members. The members are divided into resident, non-resident, and honorary members, the last chosen by election, from among the most eminent men in France and abroad. In the first year six will be elected, and two only in each following year.

The society will be glad to receive such gifts as will facilitate its work, and will inscribe in its Transactions the names of the givers.

The board is thus composed:—President, M. Fizeau, Member of the Institute; Vice-President, M. Bertin, Director of the Scientific Studies to the Superior Normal School; General Secretary, M. d'Almeida, Director of the new Journal of Physics; Secretary, M. Maurat, Professor of Physics to the Lycée St. Louis, of Paris; Vice-Secretary, M. Alfred Cornu, Professor of Physics to the Polytechnic School; Treasurer-Archivist, M. Philippon, Secretary of the Faculté des Sciences of Paris.

The venerable M. Becquerel, who, notwithstanding his 89 years, assisted at the meeting, in order to give by his presence a proof of his adhesion to the new society, has been designed, by acclamation, an honorary member.

MAXIME CORNU

NOTES

PROF. OWEN has been appointed to a Civil Companionship of the Bath. If this is intended as an acknowledgment of Prof. Owen's services to science, it is not to the credit of Government that the honour was not conferred years ago.

PROF. TAIT'S Rede Lecture on Thermo-dynamics will be delivered to-morrow.

HITHERTO the London "Companies," whose "fatness" is notorious, have done little or nothing for the promotion of scientific researches or education. It is therefore with the greatest pleasure we record that the Fishmongers' Company have handsomely presented to Mr. W. K. Parker, F.R.S., so well known for his valuable researches on the shoulder-girdle and skull in vertebrated animals, the sum of 50*l*., in addition to an allowance of 20*l*. a year for the next three years in order to enable him to pursue such parts of his work as relate to the Anatomy of Fish. This we certainly think a step in the right direction, and the Fishmongers' Company deserve all praise for having been so original and generous as to be the first to take it. We hope their award to Mr. Parker is only an earnest of what they will do in the future, and that their example will not be lost on the other notoriously wealthy companies of the City of London. A few thousands a year would never be missed out of their enormous revenues, and would not diminish by a single dainty the sumptuousness of their numerous feasts; whereas the amount of original and practically beneficial scientific work that could be done with the money, would yield them and the country generally a rich return. We daresay those who have the management of the funds of the various companies would be willing enough to divert a portion into scientific channels if they only knew how to go about it; the example of the Fishmongers' Company may afford them a hint. Moreover they need be at no loss, for there are plenty of eminent men of science competent and judicious enough to lend advice to the companies in this matter. Commerce, with which these companies are all more or less connected, owes much of its present gigantic dimensions and great prosperity to the discoveries and advances of science; gratitude and self-interest ought to urge our London merchants not to be indifferent to scientific progress. Let us also add, that their award to Mr. Parker is on a scale which shows a very slight acquaintance on the part of the City magnates with the value of time.

A FUSION has taken place between the local committee at Munich for erecting a statue to Justus von Liebig, and the committee appointed by the German Chemical Society at Berlin; the latter, in order to insure unity of action, giving way in the question as to where the statue should find its place. Notwithstanding the serious nature of the claims of Giessen, it was generally thought that the resting-place of the great chemist would unite the majority of votes of his admirers. A considerable number of leading German statesmen and foreign ambassadors have joined the committee, the full list of which will shortly be published.

FRESENIUS, who twenty-five years ago founded a school of chemistry at Wiesbaden, has celebrated the anniversary of its foundation amidst the festive concourse of his friends and pupils, and of the Government and learned societies of his country. A gloom was unfortunately cast over this event by the death of Mrs. Fresenius, which almost coincided with its celebration.

WE regret very much to announce the death of Emanuel Deutsch, at Alexandria. His premature death is a very great loss to Eastern scholarship.

THE Alexandra Palace, under new management, reopens on Saturday. We hope the managers will not neglect the interests of science.

WE recently announced that the French Society for the Encouragement of National Industry had awarded its grand medal to Sir Charles Wheatstone. The following is an extract from the report of the Committee on the Economic Arts:—While the kaleidophone of Sir Charles Wheatstone has been the point of departure of the method which permits sounds to be studied by the aid of the eye; while his researches on the qualities of sound, on the production of vowels, while the creation of his speaking machine, have elucidated many points in the theory of the voice; while his ingenious apparatus, illustrating the propagation and the combination of waves, has facilitated the understanding of these delicate phenomena, and contributed to throw light on the mechanism of the undulatory motions, his numerous researches on the applications of electricity, in which he has shown, at the same time, profound science and a genius marvellously inspired, occupy a great place in the history of the electric telegraph. It is he who first realised, under conditions really practicable, this admirable means of communication between men and between nations, and we ought not to forget that, more than once, he has come *personally* among us to prepare its organisation and stimulate success. The unanimous choice made by the committee of the economic arts and cordially ratified by the Council honours our society as much as him who is the object of it. We are happy to give, on this occasion, a testimony of sympathy to a nation in which science is held in such high esteem. Those among us who have had the good fortune to visit the scientific men of England in their own country have not forgotten that we have always received from them the most cordial and the most generous hospitality. In conferring on Sir Charles Wheatstone a reward rendered valuable by those who have already received it, the Council performs a pure act of justice, and acquits, at least for some among us, a debt of gratitude.

DR. VON DOLLINGER has been appointed President of the Bavarian Academy of Science and Conservator-General of Scientific Museums in Bavaria, which became vacant by the death of Baron Liebig. King Louis advised the doctor of his appointment by an autograph letter.

THE Institution of Civil Engineers hold a *conversazione* in the West Galleries of the International Exhibition, on Tuesday, the 27th inst.

MR. ARTHUR GAMGEE, M.D., F.R.S., Lecturer on Physiology at Surgeons Hall, Edinburgh, and Examiner in Forensic Medicine in the University of London, has been appointed Brackenbury Professor of Practical Physiology and Histology in Owens College, Manchester.

PROF. H. DE LACAZE-DUTHIERS, member of the French Institute, Professor of Zoology at the Faculté des Sciences of Paris, and Director of the Zoological Station of Roscoff, will accompany Commander Mouchez, in the *Narval*, that officer being engaged in completing the hydrographic map of the Algerian shores. The professor will make frequent soundings, and study the fauna of the Mediterranean. He will be assisted in the geological determinations by a distinguished young geologist, M. Vélain, Répétiteur of the Faculté of Sciences of Paris. The cruise will last five months. The ship left Lorient on May 1. M. de Lacaze-Duthiers will join them in July, at the termination of his lectures at the Faculté. Let us hope that these new explorations, under the guidance of an ardent, learned, and experienced man, will procure materials as valuable as those which were obtained by Agassiz, Wyville Thomson and others.

WE understand that there is a plan in hand for building a new museum at Vienna, to which the contents of the Imperial Zoological Cabinet, including the important collections of Natterer and other well-known naturalists, are to be transferred.

THE following telegram was received on Saturday at the Foreign-office from Colonel Stanton:—Alexandria, May 17, 1873.—The Egyptian Government has just received a despatch from the Governor-General of Southern Loudan, dated 15th March, reporting the arrival at Gondokoro of the reinforcements sent to Sir S. Baker, confirming the private intelligence recently forwarded to your lordship as to the safety of the party, and adding that in compliance with Sir S. Baker's demand, 200 soldiers, with a supply of salt and ammunition, had been sent on to him. Sir S. Baker had not reached the lake.

DR. PETERMANN has recently received a letter from Dr. Nachtigal, who in 1869 was sent out to Africa on a mission from the Emperor of Germany to the Sultan of Borneo. The letter is dated February 1872, and gives some brief details of Dr. Nachtigal's visits to the countries lying to the N.E. of Lake Tchad, the greater part of the region visited being new to European exploration. A most important discovery made by Dr. Nachtigal is that Bahr-el-Gazal, put down on some maps conjecturally as flowing into Lake Tchad, really flows out of that lake north-eastwards for about 300 miles. He has also discovered a range of mountains extending probably a distance of upwards of 800 miles from Tibesti to Darfur; one of the passes is at least 7,878 ft. above sea-level. At the date of the despatch of his letter, Dr. Nachtigal was about to undertake a journey into Bagirmi, the country lying to the south-east of Lake Tchad. It will thus be seen that this traveller is collecting materials which will add greatly to our knowledge of Central N. Africa.

A MESSAGE has been received by the *Daily Telegraph* from Mr. George Smith dated Mosul, May 19. "Since my last message," he says, "I have come upon numerous valuable inscriptions and fragments of all classes, including very curious syllabaries and bi-lingual records. Among them is a remarkable table of the penalties for neglect or infraction of the laws. But my most fortunate discovery is that of a broken tablet containing the very portion of the text which was missing from the Deluge tablet. Immense masses of earth and *débris* overlie whatever remains to be brought to light in this part of the great mound. Much time and large sums of money would be required to lay it open. I therefore await instructions from you and the Museum, as the season is closing." The *Daily Telegraph* and the British Museum have now an opportunity of showing that they have really at heart the advancement of historical research, and we are sure Mr. Smith's hint will be met by a hearty response. We feel confident that the liberality of the *Daily Telegraph* will be continued until Mr. Smith's researches are completed to his own satisfaction.

SOME time ago we were able to give authentic news of the safety of the Russian explorer of New Guinea, Dr. N. von Miklucho-Maclay, who had been reported dead in several newspapers. Dr. Maclay has himself sent a letter to Dr. Petermann, dated on board the Russian clipper, *Isumrud*, March 11, with a postscript dated Manilla, March 22, saying he is alive, though not very well, and was about to despatch to the St Petersburg Geographical Society an account of his exploration of New Guinea, his main object in visiting that country being to collect material for its ethnology. He intended to visit Luzon and the Sunda Islands, and then return to New Guinea.

AN important step has been taken in the carrying out of the decisions of the International Metric Commission which met at Paris in October last year. The form and mode of execution of the standard metre having been settled, the Commission entrusted to the French Section the manufacture and comparison of the new metres with the original standard in the Archives of France. We learn from *Les Mondes* that before proceeding to cast the definitive metres, the French Commission has thought it advisable to execute the first types, with which to test successively all the methods that will ultimately be applied to the definitive metres. This first experiment took place in the laboratory of M. H. Sainte-Claire Deville, who, with the assistance of M. Debray, has succeeded in obtaining the iridio-platinum alloy perfectly pure. The operation of casting this first international metre was considered of so much importance, that the President of the Republic and some of his Ministers, and other eminent Frenchmen, "assisted" at it. Nine kilogrammes of platinum, with one kilogramme of iridium, were melted under the action of an oxyhydrogen flame from a blow-pipe in three-quarters of an hour. The ingot was then cast, perfectly limpid, in a mould formed, like the furnace itself, of a block of carbonate of lime, whose interior walls alone were burned under the influence of the excessive temperature which was developed; consequently with this substance there is no risk of breakage. The metal was allowed to cool in the mould, and preserved its bright surface; in this condition it will be submitted to all the processes necessary to give it the definitive form which it ought to possess. The operation was considered, by all who witnessed it, as perfectly successful.

THE following further particulars with reference to the American Arctic exploring ship *Polaris*, Captain Hall, have been obtained by the correspondent of the *New York Herald*; they are dated Bay Roberts, *via* St. John's, N.F., into which the steamer *Tigress* had come, having on board nineteen survivors, including H. C. Tyson, assistant-navigator of Captain Hall's expedition. This party, which had been landed from the *Polaris*, were driven from her by a gale which burst her moorings on October 15, 1872, in latitude 72·35. When they last saw the *Polaris* she was under steam and canvas, making for a harbour on the east side of Northumberland Island. She had no boats left of the six which she brought with her from the port of New York. Two were lost in a northern expedition, two were landed on the ice with Captain Tyson's party, one was burnt as firewood to make water for the crew, and the other is on board the *Tigress*. The *Polaris* was in command of Captain Buddington, who had thirteen of a crew along with him, and a plentiful stock of provisions. She was making a good deal of water, but, as Captain Tyson informed the *Herald* correspondent, she was not more leaky than when he was on board all the previous fall and winter. Her bow was somewhat damaged, and it is the opinion of the survivors they will be unable to get clear until July, and even then, if the ship is unseaworthy, they would have to make new boats to effect an escape. On October 8, 1871, in latitude 81·38, longitude 61·44, Captain Hall died of apoplexy, and was buried on shore, where they erected a wooden cross to mark his grave. He had recently returned from a northern sledge expedition, in which he had attained the latitude of 82·16. In September 1871, the *Polaris* entered winter quarters, and left August 12, 1872. The ice was very heavy, and set in a southern direction. She was forced south, and so continued drifting till Captain Tyson and party were driven from her. The sledge party crossed Kane's Polar Sea, which they pronounced to be a strait about 15 miles wide. There was an appearance of open water to the north.

THE Education Department propose to send on loan, to local schools in which it will be useful, what they call Travelling Apparatus for illustrating Instruction in Naval Architecture. The following is the list of articles included under that title:—1. Model of a half-midship section of an iron-clad ship, showing the mode of forming and combining the keel, frames, beams, &c., &c. 2. Ditto of an ordinary wooden ship. 3. Block-model, showing the lines used in laying off the fore-body of a ship. 4. Ditto, after-body. 5. Diagram showing the lines used in laying-off. These models and diagram are intended to be placed in the school or class-room for reference during the hours of study, in order that the students may better understand the nature of the work under consideration, and also to aid the teachers in illustrating their ideas when imparting instruction to their classes.

PROF. MARSH, in the current number of the *American Journal of Science and Art*, describes several new species of mammalia from the tertiary deposits of the Rocky Mountains region. *Orohippus agilis* is a new species of a genus intermediate between *Anchitherium* and *Palæotherium*, which has four functional digits, the first premolar tooth nearly as large as the second, no antorbital fossa, and an incomplete bony orbit. *Colonoceras*, a new genus, nearly allied to *Hyrachyus* (Leidy) and *Helaletes* (Marsh), is peculiar in having a pair of rugosities on the nasal bones, to support dermal horns. It was about the size of a sheep. Prof. Marsh separates the genus *Dinoceras* from *Tinoceras*, on account of the maxillary horn-cores being more anteriorly situated, and the parietal crests more elevated in the former, at the same time that the canine tusks are more compressed and trenchant. A new species of *Oreodon*, and two others of *Rhinoceros*, are also described.

A *résumé* of our knowledge, strikingly incomplete as it is, on the subject of sneezing, is given by Dr. Seguin in the third number of the new and excellent *Archives of Scientific and Practical Medicine*. The author's attention was drawn to the subject from his observing a fact, previously well known, that sneezing may be frequently stopped by pressing the fingers on the lips or sides of the nose. No new theory is given to explain the physiology of the phenomenon, and it is stated that naturally most of the air expired during a sneeze escapes through the nose, but that custom has brought about the discharge of a part through the mouth. This we cannot agree with, as it is difficult to believe that custom has much influence on so abrupt an act.

WE learn from *Ocean Highways* that Major Branfill, of the great Indian Trigonometrical Survey, has discovered that a peak of the Anamully Range attains a height of 8,837 ft. above the sea, 500 ft. higher than Dodabetta, in the Nilgiri Hills, hitherto supposed to be the loftiest peak in Southern India.

A FEW of the members of the Anthropological Institute, who did not approve of the proceedings at the annual meeting, have formed themselves into a separate society, under the name of the London Anthropological Society, with Dr. Charnock as president, and Captain R. F. Burton and Mr. Staniland Wake as vice-presidents. "This society," the prospectus says, "has been formed for the study of the science of anthropology in all its branches. The society, while adhering to the usual practice of conducting its transactions at meetings attended only by Fellows and gentlemen introduced by Fellows, contemplates placing the results of its investigations before the non-scientific portion of the community, by holding from time to time special meetings, to which the general public will be admitted."

ADDITIONS to the Brighton Aquarium during the past week:—One Alligator (*Alligator mississipiensis*), 8 feet long, from South Carolina, purchased; one Australian Monitor (*Monitor gouldii*), purchased; 500 salmon, Great Lake trout, common trout, and hybrid fry (*Salmo salar, lacustris, et fario*), presented by Mr. Frank Buckland; larger and lesser Spotted Dog-fish (*Scyllium stellare et canicula*); Skate-toothed Shark (*Mustelus vulgaris*); Picked Dog-fish (*Acanthias vulgaris*); Monk-fish (*Rhina squatina*), one specimen 5 feet long; Sting Ray *Trygon pastinaca*); Common Skate (*Raia batis*); Spotted Ray (*R. maculata*); Thornback (*R. clavata*); Three-spined Sticklebacks (*Gasterosteus spinulosus*); Bass (*Labrax lupus*); Streaked Gurnards (*Trigla lineata*); the Piper (*Trigla lyra*); Greater Weever (*Trachinus draco*); Lesser do. (*T. vipera*); John Dorée (*Zeus faber*); Dragonets (*Callionymus lyra*); Sand Smelts (*Atherina presbyter*); Grey Mullet (*Mugil capito*); Carp (*Cyprinus carpio*); Roach (*Leuciscus rutilus*); Minnow (*L. phoxinus*); Loach (*Nemachilus barbatula*); Tench (*Tinca vulgaris*); Herring (*Clupea harengus*); Sharp-nosed Eel (*Anguilla vulgaris*); Greater Pipe-fish *Syngnathus acus*); Snake Pipe-fish (*Nerophis æquoreus*); Branched Seahorse (*Hippocampus ramulosus*), Mediterranean; Squids, (*Loligo media*); Masked crabs (*Corystes cassivelaunus*); Spider Crabs (*Maia squinado*).

THE additions to the Zoological Society's Gardens during the past week include a Cashmere Monkey (*Macacus pelops*), presented by Rear-Admiral Davies; a Savannah Deer (*Cervus savannarum*) from South America, presented by Capt. Bennett; a Suricate (*Suricata zenik*) from South Africa, presented by Mr. A. Benyon; a Palm Squirrel (*Sciurus palmarum*) from India, presented by Mr. W. Lovegrove; a Mocking Bird (*Mimus polyglottis*) from North America, presented by Mr. P. Frank; an Indian Eryx (*Eryx johnii*), presented by Dr. Anderson; two pied Crow Shrikes (*Strepera graculina*) from Australia; an Ursine Colobus (*Colobus polycomus*) from Sierra Leone; a Hocheur Monkey (*Cercopithecus nictitans*) from West Africa; a Wandering Tree-pie (*Dendrocitta vagabunda*), two pied Mynahs (*Sternopastor contra*), and two rose-coloured Pastors (*Pastor roseus*) from India, purchased; two Hoffmann's Sloths (*Cholopus hoffmanni*) from Panama; two black Vultures (*Cathartes atratus*) from South America; a black-handed Spider Monkey (*Ateles melanochir*) from Central America, and a Crocodile (*Crocodilus americanus*) from Mexico, deposited.

COMPARISON OF THE SPECTRA OF THE LIMB AND OF THE CENTRE OF THE SUN *

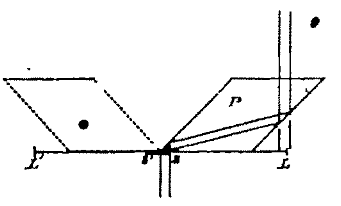
A COMPARISON of the spectrum of the edge of the sun with that of its centre is of great theoretical interest; but any comparison other than by direct juxtaposition must be very unsatisfactory, and the more so as the differences are less. In order to obtain spectra of two different portions of the sun side by side, where the slightest variations may be detected, I have constructed a small prism with four polished sides, its bases being parallelograms. This is so placed that one face rests upon the slit plate of the telespectroscope, and has its acute edge perpendicular to the slit at its middle point. The instrument may then be directed so that the image of the sun falls with its centre on the uncovered portion of the slit, while the light which forms the edge of the sun, falling perpendicularly upon the first surface of the prism, suffers two interior total reflections and a displacement depending upon the form of the prism. A glance at the figure, in which $s\,s'$ is the slit, L L' the diameter of the sun's image, and P the prism, shows that no light from the covered part of the slit will reach the collimating lens except that which has been reflected from the two sides of the prism. The relation of the acute angle (v) and the distance between the reflecting sides (t) to the focal length of the great telescope (F) and the width of the spectrum (a) is given by the formula,

$$2t \sin v = F \tan 16' - a.$$

The sides of the prism not fixed by the equation admit of considerable latitude, but should be made to approach the lower limit in order that the planes of the direct and transmitted images may be as little separated as possible. Of course t and v should be so proportioned that the reflections may be total.

The instruments with which the following observations have been made are those belonging to the observatory of the Sheffield (U.S.) Scientific School, consisting of an equatorial telescope of 9 in. aperture, and 118 in. focal length, by Clark, and a spectroscope of Young's form by the same maker. The spectroscope has a dispersive power of 12 prisms of 60°. In most of these observations an eye-piece of high power has been adapted to it, which gives a separation of the D lines equal to 64 minutes nearly. In the small prism placed before the slit, a is equal to ·04 in., a quarter of the length of the slit.

When the instrument is properly directed and in adjustment, we see a very narrow black line dividing the spectrum longitudinally into two parts of widely different intensity; the fainter, belonging to the limb of the sun, is marked on its edge by the bright chromosphere lines. Upon comparing these two spectra, certain differences are recognised besides that of intensity, by far the most marked of which are exhibited by the lines b_1 and b_2, which become sharper and less hazy near the limb. The line b_4 possesses the same characteristic, but to a less degree; C and F also become sharper in the same region. Excepting these and the D lines it requires very close examination to detect any variation. There is, however, a line in the red at 768·1 of Kirchhoff's scale which is strongly marked near the centre of the sun's disc, but disappears entirely, to my power at least, within 16″ to 20″ from the limb. Two other lines below F, at 1828·6 and 1830·9 of the same scale, exhibit nearly complementary phenomena, *i.e.*, they are strongly marked near the edge, but much fainter at the centre. These latter lines also become greatly strengthened over the penumbræ of spots. The line 768·1 is not thus affected. These are all the differences which I have invariably seen in repeated examinations since February 17.



Others have, however, been suspected. Certain lines, which are strengthened in a region of spots like those above mentioned, appear to be strengthened also near the edge, but do not undergo so marked a change. It is obvious that the differences should be most pronounced in the clearest sky, and such is the case. The closest examination has extended only from B to a short distance above F, as the plate glass of which the small prism is made has a decided yellow tint and absorbs the blue rays strongly.

Since the light from the border of the sun undergoes a general absorption, which reduces its intensity to much less than one-fourth that at the centre, according to Secchi's measurements, and yet the spectroscopic character is changed so slightly, it is impossible for me to escape the conviction that the seat of the selective absorption, which produces the Fraunhofer lines, is below the envelope which exerts the general absorption. But the phenomena of the faculæ prove not only that this envelope rests upon the photosphere, but also that it is very thin. The origin of the Fraunhofer lines, then, must be in the photosphere itself, which is in accordance with Lockyer's views.

Any effects which the chromosphere might produce, we would anticipate finding most evident in the lines of those gases which are readily detected there. A reference to the observations shows at once a compliance with this anticipation in the lines of hydrogen, magnesium, and sodium. The line 768·1 is not less strikingly in concordance, if it be regarded as 768·?* (the ? indicates doubt as to the tenths of the scale, and * absence of a corresponding black line) of Young's Catalogue of Chromosphere Lines. The lines 1828·6 and 1830·9, with others of the same class, probably have their origin in the medium which exerts the general absorption, and thus are allied to our telluric lines. It also seems probable that the chromosphere is too transparent to reverse many of its lines. That this is the case in the helium lines is tolerably certain.

In the apparatus described, two similar prisms were also placed over the slit in a symmetrical position. The spectra of two opposite edges of the sun were thus brought together, and the change in refrangibility due to the sun's rotation was very clearly shown.

CHAS H. HASTINGS

Newhaven, April 3

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* Made at the Sheffield (U.S.) Scientific School. Communicated by Prof. Newton.

THE "INSTINCT" QUESTION

FROM the many additional communications we have received on this subject, we make the following selection.

With regard to a sense of direction, Mr. George C. Merrill, of Topeka, Kansas, writes as follows:—

> I have learned from the hunters and guides who spend their lives on the plains and mountains west of us, that no matter how far or with what turns they may have been led in chasing the bison or other game, they on their return to camp always take a straight line. In explanation they say that unconsciously to themselves they have kept all the turns in their mind.

Mr. C. Bygrave Wharton, of Bushey, Herts, writes:—

> As a left-handed and left-legged man who has more than once been lost in the bush in New South Wales, my experience may possibly be of interest to Mr. George Darwin and others. Invariably I unintentionally bore to the left; and once, after wandering for about six hours, just as I was giving myself up for lost, I discovered that I was within a hundred yards of the place from which I had started having performed a large circle to the left. It will thus be seen that though my left leg and arm are the stronger, there is always a tendency to walk in a circle to the left.

Mr. William Earley, of the Gardens, Valentines, sends the following interesting observations on the habits of wild rabbits:—

> As is well known, the doe rabbit does not produce her young in any ordinary rabbit warren, or "run," but invariably selects a quiet out-of-the-way situation wherein to form a nursery for them. Now the reason for this peculiar practice has always been attributed to the fact that they leave their legitimate homes at this all-important period, simply because the male parents invariably destroy the offspring if an attempt be made to breed them in the permanent home or warren. I incline to believe we must look elsewhere for the explanation.
>
> Firstly, then, a close atmosphere seems all-important to their development, as the old doe rabbit not alone denudes her breast of its natural fur covering wherein to ensconce them warmly all

around, she also closes up the usual entrance to the nursery firmly, even patting the soil down to exclude the colder outer air. In due time, as the young increase in size, &c., she makes "air-holes," commencing with very minute ones, which are gradually enlarged as the inmates gain strength and size.

These are known facts, to which I add one not heretofore noticed, which seems important; it has reference to the formation of the subterranean nursery, in regard to its shape and the evident "end in view." These minor tunnels, or nursery "stops," are invariably formed by starting a downward curve, at an angle of about 45°, which is continued beyond any line of sight the eye can be guided by on the outer side. They subsequently curve abruptly upward, with almost double this initial acuteness, ending in a shelved enlargement, with the roof boundary nearly uniformly three inches from the surface of the ground above and without.

What I feel constrained to uphold in regard to these first facts is, that herein exists a most subtle sanitary arrangement; that by these means a subdued genial air is admitted, the only fresh air the nursery receives, and whereon the nurselings thrive, strengthen, and grow. The facts would seem to support the theory that the mother-parent continues what must be its hard work—doubly hard and severe in these finishing overhead excavations—until the very keen power of scent they possess assures them that the outer air is slightly admitted through innumerable interstices in the soil above.

My second proposition, or indeed belief, based upon distinct observation, is, that the parent doe rabbit does not visit its young, even nocturnally, at certain times oftener than once in each 72 hours! Certainly sometimes not more frequently than once during the 48 hours comprising two days and two nights. The latter fact I have ascertained by carefully marking and observing the neatly closed entrance to the stops, and also by marks beneath an iron garden-gate, in freshly laid gravel, which the rabbits had to scratch aside before they could enter. Furthermore, I have every reason to believe that the parent rabbit ceases to transmit the customary natural scent at the time she approaches or acts about the "stop;" if, indeed, as is the case with some kinds of game birds, during the period of incubation, she does not lose it altogether. Certain is it no appreciable amount of scent remains about the stop in the early morning after the parent rabbit has visited its nursery during the past night.

[On the question whether animals have the power of ceasing to emit a scent at certain times, see the article on Pheasants in this week's number.—ED.]

Mr. J. D. Bell, of the *World* office, New York, writes as follows on the consciousness of time in horses:—

My own experience will not allow me to speak positively as to smell, but horses that I have met and carefully observed, were not peculiarly gifted in this respect. It was a common saying on "the plains" and in the mining regions of California, that mules, by the way very sagacious animals, which would well repay observation, "scent the redskin a mile away." I have made some inquiry on this point, but have been unable to find that the olfactories of the mule are really thus acute. I can bear testimony to the extraordinary powers of sight in horses. And I am inclined to think that they take more notes by the way through their eyes than through the nose. As none of your correspondents have called attention to it, I desire to recall the fact that horses have ears as well as eyes and noses. Their hearing is very acute, and I am inclined to think that the explanation of the detection of red-skins by mules, will be found in the educated ear rather than in the educated nose. It used to be said in the cavalry service of the United States during the war that "horses were the best pickets." I have seen them again and again in the dead of night prick up their ears when the men on their backs heard nothing. I have never seen them sniff or smell first. Listening was invariably the first movement. Then came sight. Horses have scanned the woods and chapparall with a care that no man could surpass. If the moving thing first heard and then seen was an unfamiliar object—more especially if it was moving along the ground—then I have seen horses sniff, smell, and snort. In horses the snort is expressive of aversion rather than fear, or perhaps of a sentiment compounded of both.

Horses learn the notes of the bugle, and I have often seen a trained horse turn in a direction opposed to that indicated by the pressure of his less experienced rider's leg. I have known horses which, after detecting the presence of moving objects by hearing and then by sight, during which time they remained perfectly quiet, change feet, and even paw the ground if the rider did not by his movement show recognition of the presence of what might be an enemy.

And what, it will be asked, has this to do with the question at issue? Simply this—horses think, horses reason, horses classify, horses remember. But I desire to offer a few remarks on Darwin's letter about the blind mare that stopped at every public-house on the road. My own explanation of the fact, and there must be hundreds of similar instances—is that the mare, by long-continued custom, became conscious of the time which should elapse between the respective stopping places. Horses have a great memory for time. What is the interpretation of the existence and improvements of our racing and trotting horses but that these animals have the power of remembering time, and the power of transmitting this improved registering and transmitting cerebral apparatus to their progeny. I will close this letter by relating a couple of incidents. I was speaking of my belief in this equine memory for time to an enthusiastic horseman of my acquaintance, the other day, and at the same time showed him Mr. Darwin's letter. He said that in his youth he had driven a horse, sound in every respect, on a "bread" route. He always served his customers in a certain order. After a while his animal knew all the places, and stopped in front of the store or residence where bread was to be delivered, without a signal from his master. If the master remained in any place longer than usual, his horse started off, but instead of going to the next customer, returned to the stable. This, said he, occurred again and again, not at one place, but at many places.

I served, during the recent war, in a cavalry regiment in the United States' service. The horses knew the time for "the relief," and if the relief did not come they became restive. On one occasion we changed the time of remaining on post from two to four hours. For the first two hours the horses behaved admirably; after that they were in constant motion, and had to be constantly restrained. Horses recognised the time for stable call—not merely "hunger"-call, but the proper time-call.

A gentleman in the north of Ireland, who gives us his name and address, sends us the following story of a dog:—

He was a terrier—a cross upon the skye—very intelligent, like all of his kind. He was given to me by Mr. C——, a gentleman residing upon Lough Foyle near Moville. He was brought from that to Derry in a steamer up the Lough, and from Derry to Buncrana down Lough Swilly by train. He therefore travelled two sides of an acute-angled triangle, about thirty miles in all by conveyance. The third side being about fifteen miles, but a mountainous and unfrequented route. He appeared at first very happy and reconciled, but one fine morning he was seen taking the road parallel to the railway back to Derry, and after my searching for him for some days and making every inquiry, we found he had returned, tired and worn out, to his old master, Mr. C., near Moville. It was evidently hard work, and he was two or three days on the road. This I consider an interesting case.—Here the dog did not go by the third side of the triangle—which if he knew how to do he would have done instead of exhausting himself by the long route he took—following the direction along which he came by steamer and train.

My theory is that the dog does preserve a very distinct, or at least tolerably distinct, notion of the route he was brought from home by, and that it is forcibly impressed upon him; but the great aid to his return is the *direction* of the sun or light. He knows that if he travels in a certain direction—say E.—he is going towards the morning sun, and W., towards the evening sun.

A correspondent, Mr. R. A. Pryor, Hatfield, sends us the following extract from the Rev. A. l'Estrange's edition of Miss Mitford's "Life and Letters":—

Miss Mitford (Letter of October 16, 1829, vol. ii. p. 277), had been dining in company with the late Dr. Routh, president of Magdalen College, Oxford, who "had a spaniel of king Charles's breed, who, losing his mamma by accident when a pup, was brought up by a cat: well, he and his brother, for there were two pups, orphans of three days old, were nursed by this cat. But what I mention him to you for is to tell you the curious account which the doctor, a man of perfect veracity, gives of his habits—he is as afraid of rain as his foster mother, will never, if possible to avoid it, set his paw in a wet place; licks his feet two or three times a day, for the purpose of washing his face, which operation he performs in the true cattish position, sitting upon his tail; will watch a mouse-hole for hours together; and has in short all the ways, manners, habits, and dispositions of his wet nurse, the cat. Is not this very singular? But it's puzzling as well as amusing, and opens a new and strange view into that mysterious subject, the instincts of animals. Mrs. Routh, and Mrs. Blagrove (the mistress of the cat, who was present at dinner to-day), confirmed all the facts of the case. They say that one can hardly imagine how like a cat Romulus (the dog's name) is, unless one lived with him."

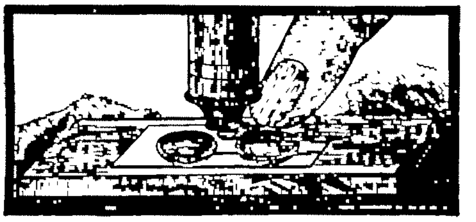
The following is from a letter of October 20, 1835:—

"Another characteristic of this hot dry summer (1835) has been the manner in which the large humble bees have forced open, torn apart the buds of my geraniums; an operation I never saw them perform before.

"Another novelty of this season has been that the splendid new annual, the *Salpiglossis picta*, has, after the first crop of blossoms, produced perfect seed without flower petals, a proof (if any were needed), that the petals which constitute the beauty of a flower, are not necessary to its propagation."

We may mention that Mr. C. H. Jeens has a cat and a dog, the latter now twenty months old, which, from the time the dog was a month old, have been in a relation similar to the cat and the pups in Miss Mitford's story, with a result somewhat similar. When the dog catches a mouse he treats it after the well-known manner of cats, pawing it, allowing it to run a distance, then pouncing upon it, and so on for many minutes.

SCIENTIFIC SERIALS



THE *Monthly Microscopical Journal* commences with the paper on "a new Callidina; with the results of experiments on the Desiccation of Rotifera," by Mr. H. Davis, which was read before the Royal Microscopical Society in April, and in which the author, by means of several carefully performed experiments, proves that Rotifera, which survive after being exposed to a temperature of 200° F., or in a vacuum for some time, do not get desiccated, but only covered with an impervious gelatinous covering which retains a certain amount of moisture in them. This Mr. Slack shows to have been previously proved. Mr. Parfitt describes a new form apparently related to the Rotifera and the Annelida, named by him *Agchistus plumosus*, with the oral aperture lateral and inferior. Dr. Braithwaite describes *Sphagnum papillosum* and *S. austini* in his paper on Bog Mosses; and Mr. F. Wenham has another valuable paper on "Binoculars for the highest powers." A new slide for the microscope, designed by Mr. D. S. Holman, is described. It is a current cell or moist chamber for studying the blood and other organic fluids. The accompanying illustration will assist in explaining it. Two shallow circular cavities are excavated in a very flat thick glass slide, not far from one another. They are united by two or three grooves, which are cut as triangles in order that they may be of unequal depth in different parts. When the apparatus is to be used, each of the shallow cavities and the intermediate grooves are partly filled with the fluid to be examined, after the slide has been warmed by the hand, and a glass cover is laid over the whole, which soon becomes fixed from the cooling of the slide and the consequent rarification of the enclosed air. The grooves between the cavities form the field for inspection, and any degree of movement may be produced in the fluid which they contain by approaching the warm finger to the top of one of the cavities, as the air inside is thus made to expand and drive some of the fluid into the other which is not heated. There is scarcely any limit to the degree of delicacy of movement which may be attained with this instrument; the slightest movement, not sufficient to remove a body from the field of vision, being produced without difficulty after some practice.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, May 15.—Dr. Odling, F.R.S., president, in the chair.—Dr. H. S. Armstrong delivered a most able and comprehensive lecture on "Isomerism," pointing out that the generally received position theory was incompetent to explain many reactions which took place in the formation of metameric and isomeric substances. He suggested that the investigation of the thermal properties of compounds would establish facts which might ultimately enable us to obtain some insight into the matter.

Anthropological Institute, May 20.—Prof. Busk, F.R.S., in the chair.—A paper was read by Mr. Hyde Clarke on the Egyptian Colony and Language in the Caucasus. This was devoted to a part of a series of investigations to ascertain the comparative chronology of prehistoric races by the correlation of comparative philology with the study of physical features, monuments, weapons, &c. It identified the Ude language of the Caucasus, that of an expiring population, with the Coptic, and still more closely with the Hieroglyphic in minute and numerous details of roots, grammar, and structure. The resemblance of the Bryb dialect of Ude with the Bashmuric Coptic illustrated the differences between Hieroglyphic and Coptic. The paper then proceeded to point out the conformity of strata in the linguistic topography of Caucasia and the Nile regions, particularly in the earlier epochs of Agaw and Abkhas, and of Furian and Akush. Hence the conclusion was drawn that the sources of Egyptian grammar were not in the late Semitic, but in the prior epochs, and that Egyptian grammar and civilisation belong to a remote period in the annals of civilisation, but still to a relatively modern period in the history of man. The author, accepting the history of Herodotus as to the conformity between the Colchians of Caucasia and the Egyptians, did not accept his theory that the Colchians were a colony of Lesodites. In the time of Herodotus and Pindar, the Colchians, now light, were as dark as the Egyptians.

GLASGOW

Geological Society, April 24.—Mr. John Young, vice-president, in the chair.—Mr. David Robertson, F.G.S., read a note on the "Precipitation of Clay in Fresh and Sea Water." He stated that in making some observations on the gradual deposition of particles of clay held in solution by water, he found that in fresh water these were held suspended for a long time before wholly subsiding, while salt water, or a mixture of salt and fresh, became comparatively clear in the course of a few hours. The results showed that water only slightly brackish had a great power in precipitating the clay, and from this he concluded that the great bulk of the clay carried down in solution by rivers must be deposited before it could reach any great distance from the sea shore. This might throw some light on the formation of deltas, and on the silting up of river courses within the influence of the tides. It might also assist in determining how far the glacial mud, for example, could be carried into the sea by tides and currents.—The chairman read a paper which he had prepared in conjunction with Mr. Robertson, "On the Composition of the Boulder and Laminated Brick Clays of the West of Scotland." The authors stated that their object was to ascertain, if possible, the conditions under which these clays had been deposited, and how far any of them were fossiliferous. For this purpose they had collected samples of clays from upwards of fifty localities. These, after being dried, were weighed, and then carefully washed. The results led them to regard as most probable the conclusion that the till or unstratified boulder clay was a deposit that had been laid down in water and formed from materials which land ice had carried seawards, the ice extending over the submerged tracts now covered by the boulder clay. This seemed to be borne out by the large percentage of fine glacier mud found in all the boulder clays, and which they thought, could not have been retained in the deposit had it been formed under a sheet of land ice above sea level, seeing that streams of muddy water continually issue from under all existing ice sheets. The laminated brick clays they viewed as having been formed, by rewashings out of the boulder clay, and from the flow seawards of the muddy water from under the melting ice-sheets that bound the shores; the sea, however, being then comparatively free of ice. In nearly all the brick clays of the maritime districts they had found organisms, chiefly marine; but a few indicated brackish and fresh water conditions. Only in one or two instances had they found organisms in the boulder clay.

BERLIN

German Chemical Society, May 12.—President, A. W. Hoffmann. C. Engler spoke on the simultaneous action o ammonia and phosphoric anhydride on ketones, especially on acetophenone, C_8H_8O. The results are two bodies, $NC_{24}H_{18}$ and a hydrocarbon. The former crystallines and a weak base is formed according to the formulae,

$$2C_8H_8O + NH_3 - 3U_2O = NC_{24}H_{21}$$
$$2NC_{24}H_{21} + {}_3O = 2NC_{24}H_{18}.$$

The hydrocarbon simultaneously formed is beautifully crystallised triphenylated benzol, $C_6H_3(C_6H_5)_3 = C_{24}H_{18}$. The reaction corresponds therefore to the formation of mesityline from acetone. Phosphoric anhydride and aniline seem to transform acetone into a base of the formula $CH_3(C = NC_6H_5) - CH_3$ a liquid boiling between 210° – 220°.—C. Rammelsberg has investigated a so-called ozone-water, an article of trade, much advertised and praised for its medical properties. He has found no trace of ozone, but a small proportion of chlorine in it. Pursuing his researches, he found water of ordinary temperatures unable to absorb ozone without the application of pressure. The ozone was produced by Siemen's tube. Referring to a popular error: he explains what is generally considered as the production of ozone by mixing potassic permanganate with sulphuric acid, by the unavoidable presence of potassicperchlorate.—C. Scheibler referred to a gum, $C_{12}H_{22}O_{11}$ (isomeric with arabine) which he found in beetroot, and which is identical with metapectic acid. It occurs in two modifications turning the plane of polarisation to the right or to the left. The latter is transformed by sulphuric acid into "arabin sugar" identical with the sugar he obtained by the same process from arabine. Both crystallise in identical rhombic prisms, turn the plane of polarisation to the right ($a =\quad + 116$), reduce solutions of copper, and do not ferment.—N. Michaelis has made the interesting discovery that a liquid phenylic phosphide is obtained by passing benzole and tercloride of phosphorus through red hot tubes. It corresponds to the formula, $C_6H_5 . P. Cl_2$, and boils at 222°.—A. Bruckner, comparing two various mononitrophenoles, has found erroneous the supposition that there are four bodies of this formula, which he has reduced to three.—N. Baumgarten refutes the generally adopted opinion that bromine replaces chlorine in chloric acid. His experiments corroborate the doubt expressed by J. Thomson, and founded on his thermochemical researches.—C. Schorlemmer continued his valuable communications on hephylic acids and alcohols, as derived from heptan and from oenanthol.—V. Meyer recommends, for analysing commercial chloral, to heat it with a certain quantity of potassa of known strength, and to determine volumetrically the quantity of potassa that remained uncombined with formic acid.

PARIS

Academy of Sciences, May 12.—M. de Quatrefages, president, in the chair.—The following papers were read:—On the portative force of magnets, by M. Jamin. The author thus denominates the carrying power of magnets. He exhibited two: an ordinary one weighing 6 and carrying 80 kilos, and one made on his principle, weighing 600 grammes and carrying 500 kilos; the paper described their construction.—On the causes which produce the tumefaction of obsidian at a high temperature, by MM. Boussingauld and Damour.—New researches on aldol, by M. Ad. Wurtz. The author thus names a condensed aldehyde partaking of the properties of that body and of an alcohol.—Hydrologic studies of the Seine, by M. Belgrand.—The Academy then proceeded to elect a member of the physical section in the place of the late M. Babinet. M. P. Desains obtained 32; M. Cornu, 13; M. Le Roux, 7; and MM. Bourget, Gaugain, and Lucas 1 vote each. M. Desains was therefore declared duly elected.—A report on MM. Troost and Hautefeuille's paper on isomeric and allotropic transformations was then read, and also one on a memoir on the proximate analysis of rocks, &c., by M. Fouqué.—On the water supply of Versailles during the first half of 1873, by M. E. Decaisne.—On the algebraic representation of bright lines in space, by Mr. W. Spottiswoode.—On the regulation of compasses, by M. Caspari.—On an electro-diapason of continuous movement, by M. E. Mercadier.—An answer to an observation of M. Raynaud on the conditions of maximum resistance in galvanometers, by M. Th. du Moncel.—Observations on the notes of MM. du Moncel and Thenard on the decomposition of carbonic anhydride by the silent electric discharge, by M. G. Jean.—Observations on a paper by M. du Moncel on the condensed induction spark, by M. Honreau.—On the action of sulphur on arsenic, by M. Angélis.—On the action of gaseous hydochloric acid on the compound ammonias, by M. Ch. Lauth.—On a modification of the optical saccharimeter, by M. Prazmowski.—On the action of the dissolved oxygen of water on reducing agents, by MM. Schutzenberger and Risler.—A new classification of the fresh-water algae of the genus *Batrachospermum*, by M. Siroclot.—On spring and winter frosts, by M. Martha-Beker.

DIARY

THURSDAY, MAY 22.

SOCIETY OF ANTIQUARIES, at 8.30.—Miscellaneous Antiquities.
ROYAL INSTITUTION, at 3.—Light: Prof. Tyndall.

FRIDAY, MAY 23.

ROYAL INSTITUTION, at 9.—Spectra of Polarised Light: Mr. Spottiswoode.
GEOLOGISTS' ASSOCIATION.—Excursion to Eastbourne and St. Leonards.

SATURDAY, MAY 24.

ROYAL INSTITUTION, at 3.—The Historical Method: John Morley.
LINNEAN SOCIETY, at 3.—Anniversary

MONDAY, MAY 26.

GEOGRAPHICAL SOCIETY, at 1.—Anniversary.

TUESDAY, MAY 27.

INSTITUTION OF CIVIL ENGINEERS, at 9.—Conversazione.
ROYAL INSTITUTION, at 3.—Archæology of Rome: J. H. Parker.

WEDNESDAY, MAY 28.

SOCIETY OF ARTS, at 8.
GEOLOGICAL SOCIETY, at 8.—On the Glaciation of the Northern part of the Lake District': J. Clifton Ward.—Alluvial and Lacustrine Deposits and Glacial Records of the Upper Indus Basin: Frederic Drew.—On the Nature and probable Origin of the superficial Deposits in the Valleys and Deserts of Central Persia: W. T Blanford.—On the Cephalopoda Bed and the Oolite Sands of Dorset and part of Somerset: James Buckman.
ARCHÆOLOGICAL ASSOCIATION, at 8.

PAMPHLETS RECEIVED

ENGLISH.—The Method of Quantitative Induction in Physical Science: Dr. G. Hinrichs.—Two Essays drawn up for the Official Record of the Exhibition held in Melbourne, 1872-3. 1. On Preserved Meats. 2. On Colonial Wines: Rev. J. I. Bleasdale, Melbourne.—Solar Radiation: an Account of the Experiments made at Harpenden, Herts, by the Rev. F. W. Stow, M.A.—Report of the Entomological Society of Ontario, 1872.—The Geological Survey of Canada, Report of Progress for 1871-2.—The Fourth Annual Report of the State Board of Health of Massachusetts, January, 1873.—Journal of the Royal Agricultural Society of England, Pt. 1, No. 17, Vol. x. (Murray).—Local Biology: Rev. L. Blomfield (Bath).—Report of the Committee on the Treatment and Utilisation of Sewage, 1871 (Taylor and Francis).

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THURSDAY, MAY 29, 1873

THE ZOOLOGICAL STATION AT NAPLES

ROME was not built in a day, says the proverb,—and so far, at least, the Zoological Station resembles the Eternal City,—for it is not yet quite finished.

The difficulties have been sufficient to explain this delay. The complexity of a building of this kind, which had to combine so many technical arrangements with scientific requirements without neglecting beauty of appearance and the comfort of a dwelling-house for the principal, assistant naturalists, and other officials, will easily be conceived by those who have ever attempted to carry out the plan of an establishment *sui generis*. Add to this, that the dimensions of the building were limited before a stone was laid, that the sums allotted for the construction were by no means unlimited, that all had to be done in so difficult a place as Naples, by a foreigner who never had experience in practical pursuits of this intricate nature, but is a naturalist, and not a business man.

At the same time, one must not believe that this delay has been altogether a misfortune. Though the Zoological Station had to pass through more than one "crisis," it has been particularly lucky: dangerous as the aspect of all these critical situations seemed, nevertheless it has always escaped, and now finds itself in better circumstances than it would have been without them. This seems principally due to the fact that in struggling against difficulties and enemies, one is forced to strengthen and augment one's auxiliary troops, and thus the army of supporters gets greater and greater, and triumph is more easily secured than before.

As the outlay had been considerably increased in consequence of greater dimensions, and some internal arrangements, it became necessary to find additional funds. I am happy to say, that on my application, the German Empire, after having consulted the Berlin Academy of Sciences, consented to contribute 1,500*l*. The Italian Government likewise promised, on my personal application to the Minister of Finances, Dr. Sella, to remit the not unimportant sums that had to be paid as duties on the importation of the machinery and the great glasses.

On the other hand, I formed a new scheme for keeping up the establishment. Some of the readers of NATURE may remember, perhaps, that the whole place was founded upon the income of the Aquarium, which is combined with the Zoological Station. The bulk of the capital being augmented, and the whole establishment in all its parts increased, the sums necessary for supporting it likewise must increase. Instead of ten places to be given to foreign naturalists, who come to work in the Zoological Station, there are now twenty. The number of officials, scientific and unscientific, will increase at the same rate, and everything else, too. Desirable as such an event must be for science' sake, much as it would increase the importance of the new Institution, there can be no doubt that it would also greatly increase its annual wants.

I pursued, therefore, as much as I could, the plan for letting the tables in the laboratories,—a plan which has been spoken of in NATURE (vol. vi. p. 362). I am happy to say that at present Italy as well as Prussia has consented to hire each two tables. Bavaria, too, is likely to take one table, and further applications have been made to Saxony, to Baden, and some other places, which at present cannot be indicated, as negociations are still impending.

The Library of the Station has made very important progress. The Zoological Society of London has generously granted the complete set of their Proceedings; the British Association the complete set of their Transactions. Dr. Engelmann, the Leipzig publisher, has again made a splendid gift of all that he has published since 1870; Viet and Co., of Leipzig, have given the eight last volumes of the *Archiv fur Anatomie und Physiologie*: Friedlander, of Berlin, has sent some of his most valuable books; and single naturalists constantly send in their publications. The Catalogue of the whole Library will soon appear, and be delivered to the scientific public as Appendix to the *Zeitschrift fur wissenschaftliche Zoologie*.

The Station has already made its presence felt in the world of Zoology, by sending to Universities and Laboratories collections of Mediterranean animals. What makes this especially valuable is, that by the careful way in which the required specimens have been prepared and preserved, they are always capable of being dissected and even studied in a histological way, which seldom is the case with museum specimens. Thus the Universities of Marburg, Gottingen, Munich, Strasburg, Jena, and others, have received such collections as were asked for by the Professors of Zoology; besides this, the zoologists that passed during the last winter to Naples or Messina, have been always assisted by the scientific staff of the Station.

We have also succeeded in sending animals alive to distant places. Thus it has become very generally known that a small parcel containing some specimens of Amphioxus has been received as a charged letter in the Crystal Palace Aquarium; and I hear from Mr. Lloyd that the small animals are still alive. We succeeded also in sending some large crabs over by steamer.

It is my intention to develop as much as may be this department of the activity of the Station, and I take this opportunity of stating that the Station will send Mediterranean animals of every kind and in any state of preparation to those who make application for them. The charges will be as moderate as possible, always in accordance with the self-supporting principle, so as to enable every part of the establishment to provide for its own wants.

ANTON DOHRN

Naples, May 8

GAUDIN'S "WORLD OF ATOMS"

L'Architecture du Monde des Atomes, devoilant la structure des composés chimiques et leur cristallogénie. Par Marc-Antoine Gaudin. (Paris: Gauthier-Villars, 1873.)

IT is now more than forty years since Ampère, in his lectures at the Collége de France, was discussing the evidence in favour of the existence of atoms, and the difficulties of any scientific investigation of their properties and relations. M. Gaudin, one of his hearers, was struck, as he tells us, with the importance of this investigation, and then and there devoted the efforts of his whole life to carry it out. Accordingly, in 1832 he presented a very extensive work to the Academy of Sciences, a report on which, by MM. Gay-Lussac and Becquerel, is annexed to the volume before us.

The ideas developed in this work were derived from two sources—crystallography and chemistry. Haüy had endeavoured to explain the regularity of the forms of crystals by regarding them as built up of molecules, the form of each molecule being similar to that of the simplest solid which can be obtained from the crystal by cleavage. The absolute size of these integrant molecules, as they were called, was left, of course, indeterminate.

Wollaston preferred to regard the arrangement of the ultimate molecules in a crystal as resulting, not from their accurately fitting one another as bricks do in a wall, but from their tendency to crowd together into the smallest possible volume as peas do in a bag. The form of the molecules, according to Wollaston, was not polygonal, but spherical or ellipsoidal.

At this point Ampère took up the theory. His atoms were no longer either closely fitted together, or even touching one another at isolated points, but were maintained by attractive and repulsive forces at distances exceedingly great compared with their own dimensions. The forms of the atoms themselves were therefore no longer considered as of any importance; the molecules, formed of groups of these atoms, were represented in diagrams as systems of points; and the explanation of the geometrical properties of the substance in the crystalline form was sought in the geometrical arrangement of these atoms.

The proportions in which the atoms of different kinds were to be represented in the molecules were determined in accordance with the atomic theory of chemistry, established by Dalton, and the absolute number of such atoms in the molecule was arranged so as to satisfy the law of gases, recently discovered by Gay Lussac, which asserts that the mass of every gaseous molecule is proportional to the specific gravity of the gas at the standard pressure and temperature.

The theory of M. Gaudin may be regarded as founded upon that of Ampère, with certain modifications. Instead of assuming with Ampère, that when two molecules combine, the form of the compound molecule is the resultant of the forms of its components, he supposes that the atoms of the combining molecules are all thrown into a common stock, to be arranged, according to some principle of equilibrium or of symmetry, in a form having no necessary relation to the forms of the combining molecules.

In the work before us M. Gaudin gives us, as the result of his long-continued meditation on compound molecules, actual diagrams of their supposed forms, showing not only their outward shape, but the arrangement of the molecules in each of the layers in which they are disposed. The ingenuity with which he has arranged in a symmetrical manner groups sometimes amounting to 279 atoms must be seen in order to be appreciated. But the merit of these arrangements as an explanation of facts must be tested, first by a careful comparison of those forms whose chemical relations are similar, and then by a comparison of each diagram with the crystallographic properties of the substance which it is supposed to represent. The author has, to the best of his ability, applied both these tests, and we shall not here pronounce sentence upon the result of such an examination.

We may remark, however, that M. Gaudin began his labours forty years ago, using the methods of investigation which we have briefly described. Since that time he has been patiently arranging his atoms by rows and groups, and representing them in models by means of pearls of various hues. He has shown no symptom of being attracted towards any of those newer paths which Joule, Clausius, and others have opened up into the higher regions of kinetic molecular science. Indeed we not only find no mention of the names of any of these men, but we look in vain for any indication of a desire to pass beyond mere geometrical arrangements of atoms, and to inquire into the forces with which they act on each other or the motions with which they are agitated. There is a chapter, indeed, entitled "Hémiédrie et pouvoir rotatoire," but though there is something about hemihedry, there is nothing there at all about the power of rotating the plane of polarisation of light. The only piece of dynamics in the book is the theory of capillary phenomena at p. 197, about which the less we say the better.

M. Gaudin is favourably known to science as an adept in the management of the blow-pipe. He has melted the most refractory bodies, and compounded the oriental ruby from its elements. He has not only established the chemical formula of silica and modelled its molecule, but he has fused quartz into beads, and drawn it into threads like spun glass.

His experimental researches have displayed great ingenuity and manipulative skill, but have often been brought to an untimely end for want of funds to carry them on. In his theoretical speculations he has been guided by geometry alone, without the powerful if not absolutely necessary aid of dynamics; and in the great work of his life he has met with very little encouragement, and has been sustained only by his conviction of the scientific value of the treasure of which he is in search.

OUR BOOK SHELF

A Manual of Photography. By George Dawson, M.A. Eighth edition. (J. and A. Churchill.)

THE new edition of this excellent manual of photography, which is founded on and incorporates as much of Hardwick's "Photographic Chemistry" as is valuable in the present further advanced stage of the art, retains its position as the best work on the subject for amateurs, as well as professionals. The many new methods and materials which are so frequently being introduced, make it essential that any book professing to keep up to the times must be frequently revised, and Dr. Dawson has in this work presented the subject in its most advanced position. The earlier chapters, after giving a short sketch of the history of photography, enter into a description of the most important experiments, the expansion of which make up the subject itself. This is followed by a review of the various lenses required for the many different purposes to which photography is applied, and their peculiarities are rendered more evident by the introduction of very clear diagrams of them in section. After a full description of the various points connected with the wet-plate

Clerk-Maxwell's Kinetic Theory of Gases

YOUR correspondent, Mr. Guthrie, has pointed out an, at first sight, very obvious and very serious objection to my kinetic theory of a vertical column of gas. According to that theory, a vertical column of gas acted on by gravity would be in thermal equilibrium if it were at a uniform temperature throughout, that is to say, if the mean energy of the molecules were the same at all heights. But if this were the case the molecules in their free paths would be gaining energy if descending, and losing energy if ascending. Hence, Mr. Guthrie argues, at any horizontal section of the column a descending molecule would carry more energy down with it than an ascending molecule would bring up, and since as many molecules descend as ascend through the section, there would on the whole be a transfer of energy, that is, of heat, downwards; and this would be the case unless the energy were so distributed that a molecule in any part of its course finds itself, on an average, among molecules of the same energy as its own. An argument of the same kind, which occurred to me in 1866, nearly upset my belief in calculation, and it was some time before I discovered the weak point in it.

The argument assumes that, of the molecules which have encounters in a given stratum, those projected upwards have the same mean energy as those projected downwards. This, however, is not the case, for since the density is greater below than above, a greater *number* of molecules come from below than from above to strike those in the stratum, and therefore a greater number are projected from the stratum downwards than upwards. Hence since the total momentum of the molecules temporarily occupying the stratum remains zero (because, as a whole, it is at rest), the smaller number of molecules projected upwards must have a greater initial velocity than the larger number projected downwards. This much we may gather from general reasoning. It is not quite so easy, without calculation, to show that this difference betwen the molecules projected upwards and downwards from the same stratum exactly counteracts the tendency to a downward transmission of energy pointed out by Mr. Guthrie. The difficulty lies chiefly in forming exact expressions for the state of the molecules which instantaneously occupy a given stratum in terms of their state when projected from the various strata in which they had their last encounters. In my paper in the *Philosophical Transactions*, for 1867, on the "Dynamical Theory of Gases," I have entirely avoided these difficulties by expressing everything in terms of what passes through the boundary of an element, and what exists or takes place inside it. By this method, which I have lately carefully verified and considerably simplified, Mr. Guthrie's argument is passed by without ever becoming visible. It is well, however, that he has directed attention to it, and challenged the defenders of the kinetic theory to clear up their ideas of the result of those encounters which take place in a given stratum.

J. CLERK MAXWELL

Additional Remarks on Abiogenesis

SINCE my communication in NATURE, March 20, a further investigation of the subject has shown me that the experiments there recorded do not yet fully prove the reality of abiogenesis. My argumentation based on those experiments is liable to the following objection:—

The principal experiment (water, potassium-nitrate, magnesium-sulphate, calcium-phosphate, glucose, and peptone) is conducted in a *neutral* solution. In the control-experiments neutral ammonium-tartrate is used as nutritious substance for the supposed germs. But this salt dissociates by boiling, loses ammonia, and the reaction becomes *acid*. When, therefore, Bacteria appear in the principal experiment and not in the control-experiments, this result can be explained by admitting that the germs resist a temperature of 100° in a neutral liquid, but are killed by the same temperature in an acid solution. This explanation agrees very satisfactorily with the fact proved by Pasteur, that an acid reaction is much more deleterious to living germs than a neutral reaction at the same temperature.

This objection is very rational, but it does not throw over my conclusion respecting the reality of abiogenesis, for the following reasons:—

It is now obvious that in the control-experiments ammonium-tartrate cannot be used, a nitrogenous body must be sought, not too complex, that remains neutral by 100°. For this end I have found urea to answer well. Pure urea is perfectly fit to furnish nitrogen to the Bacteria, but not to furnish them their carbon. Bacteria sown in a solution of urea and mineral salts do not develop themselves, but when sugar is added their growth goes forth rapidly. The following solution—100 c.c. water, 0·2 grms. potassium-nitrate, 0·2 grms. magnesium-sulphate, 0·04 grms. calcium-phosphate, 1 grm. glucose, 0·5 grm. urea, is eminently fit for the development of Bacteria. Also a solution that contains, instead of the sugar and the urea, 0·5 grm. peptone.

These solutions were now used in the control-experiments. For instance:

a. Principal experiment. 100 c.c. salt-solution,* 2 grms. glucose, 0·3 grms. peptone boiled and treated in the ordinary manner (See NATURE, vol. vii. p. 380). On the third day the liquid contains countless swarms of Bacteria.

b. Control experiment. 100 c.c. salt solution, 1 grm. glucose, 0·5 grm. urea, boiled exact. No Bacteria appear; on the eighth day the liquid is perfectly clear.

c. Control experiment. 100 c.c. salt solution, 0·5 grm. peptone, boiled, &c. On the eighth day complete absence of Bacteria.

In each of these experiments the reaction is neutral. They are therefore fully comparable. The experiments *b* and *c* prove, moreover, that the closing tiles exclude completely the atmospheric germs, a fact that was also proved by direct experiments, wherein the solutions *b* and *c* were used and dust strewn on the closing tile in the manner formerly described.

But is it not possible to generate Bacteria in a liquid which has been boiled when acid?

To elucidate this point, the above-named solution *a* was rendered acid (2 – 4 c.c. of a 1 per cent. solution to 100 c.c.) and treated as usual. No Bacteria appeared, whether the liquid was, after boiling, neutralised with soda or not.

But this negative result is easily conceivable; for the acid alters essentially the calcium-phosphate, changes $CaHPO_4$ into $Ca_2H_4P_2O_8$. And that this alteration is not without influence, is rendered probable by the fact, which I have recorded in the *Maandblad voor Natuurwetenschappen*, No. 7 (April 23, 1873), namely, when in the principal experiment instead of $CaHPO_4$ is used a mixture of $Ca_3P_2O_8$, and $Ca_2H_4P_2O_8$ the result (the genesis of Bacteria) is much less constant. The neutral calcium-phosphate by boiling with water breaks up in the basic and the acid salt, but this division must take place in the presence of sugar and peptone.

On the other hand, the acid modifies the peptone. This is easily demonstrated by comparing, in the polariscope, the rotating power of a neutral peptone-solution with the power of the same solution. After boiling with acid a notable difference is observed.

The acid can, nevertheless, be employed with the following modification:—In 100 c.c. water are dissolved 0·2 grm. potassium-nitrate, 0·2 grm. magnesium-sulphate, and 2 grms. glucose; 2 c.c. of a 1 per cent. solution of tartaric acid are added, so that the liquid has a strong acid reaction. It is then boiled for ten minutes. Then with a red hot platinum spatule a little soda is taken from a hot crucible and thrown in the flask. The quantity of soda required is approximately ascertained by a preliminary trial. Care should be taken not to render the liquid alkaline. Then 0·05 grm. calcium phosphate and 0·3 grm. peptone are added together, and the boiling continued for ten minutes. The flask is closed as usual, and deposited in the hatching-bath. Three days after, it swarms with Bacteria.

When, instead of calcium-phosphate and peptone, are added 0·05 grm. calcium-phosphate and 0·5 grm. urea, nothing appears; and the result is equally negative when the following solution is taken:—100 c.c. water, 0·2 grm. potassium-nitrate, 0·2 grm. magnesium-sulphate, 0·05 grm. calcium-phosphate, 1 grm. potassium-natrium-tartrate, 0·3 grm. peptone. In this latter case no acid is used. The addition of the tartrate is made to have a sufficient quantity of carbon in the liquid. These control experiments prove that none of the employed materials, neither the glucose, nor the calcium-phosphate, nor the peptone did introduce germs.

By these experiments the above-stated objection is, in my opinion, satisfactorily refuted.

In concluding these remarks, I must mention an important fact. For the above-described experiments, I employed mostly the ordinary glucose, an amorphous, yellowish white mass, not chemically pure. By crystallisation from strong alcohol, I purified this sugar. In three different preparations I obtained thus three samples of perfectly white more or less pure glucose. One of these samples yielded, with peptone, Bacteria; not so the other two. All three were prepared with the utmost caution respecting atmospheric dust, &c. That, moreover, the positive result could not be caused by an accidental admixture of germs was amply proved by the often repeated control-experiments. It appears, therefore, that, besides the glucose and the peptone, a third substance is needed for generating Bacteria, a body present in the ordinary glucose (starch-sugar), but removed by purification. The nature of this body I have not yet been able to ascertain. But however important, this matter has no direct bearing upon the question of abiogenesis. For that this third unknown body cannot be (as some will probably presume) a germ, my control-experiments and also the above-described experiment, wherein the sugar was boiled with acid, do sufficiently prove.

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* Composed of 1 grm. potassium-nitrate, 1 grm. magnesium sulphate, 0·1 grm. neutral calcium-phosphate in 500 c.c. water

D. HUIZINGA

Groningen, May 23

Flight of Birds

SOME time since I had occasion to ascend a mountain in the neighbourhood. The wind was blowing over the ridge-like crest of the mountain with a velocity of, I should say, ten or twelve miles an hour, sweeping with increased rapidity through certain transverse gorges cutting the ridge at right angles. In one of these I observed a hawk hovering in search of prey. In the midst of this rapid air current the bird remained apparently *fixed* in space, without fluttering a wing, for at least two minutes. After a time it gently changed its position a few feet with a slight motion of its wings, and then came to rest again as before, remaining apparently as motionless as the rocks around it. From my nearness to it a change of position of an inch would have been clearly visible, and yet except when it seemed to desire to change its point of observation no motion of any kind could be detected. How is this to be accounted for? Does a bird possess the power of giving an extremely rapid tremulous motion to its wings invisible even at a small distance, similar in its nature to the wing vibration of certain insects, which, as any one may have noticed, have a similar power of apparently fixing themselves in space over a flower, for example, notwithstanding a considerable amount of motion in the air in which they are suspended?

If any of your correspondents would kindly take the trouble to throw some light on these points they would greatly oblige one who is unfortunately placed out of reach of the ordinary means of reference.

J. GUTHRIE

Graaff Reinet, Cape Colony, April 2

THERMO-ELECTRICITY [*]

THE subject I have chosen is one intimately connected with the names of at least two well-known members of this University—the late Prof. Cumming and Sir William Thomson. It possesses at present peculiar interest for the physicist; for, though a great many general facts and laws connected with it are already experimentally, or otherwise, secured to science—the pioneers have done little more than map the rough outlines of some of the more prominent features of a comparatively new and almost unexplored region. Some of its experimental problems are extremely simple, others seem at present to present all but insuperable difficulties. And it does not appear that any further application of mathematical analysis can be safely, or at least usefully, made until some doubtful points are cleared up experimentally.

The grand idea of the conservation, or indestructibility, of energy:—pointed out by Newton in a short Scholium a couple of centuries ago, so far at least as the progress of experimental science in his time enabled him to extend his statements:—conclusively established for heat at the very end of last century by Rumford and Davy; and extended to all other forms of energy by the splendid researches of Joule:—forms the groundwork of modern physics.

Just as, in the eye of the chemist, every chemical change is merely a re-arrangement of indestructible and unalterable matter; so to the physicist, every physical change is merely a transformation of indestructible energy; and thus the whole aim of natural philosophy, so far at least as we yet know, may be described as the study of the possible transformations of energy, with their conditions and limitations; and of the present forms and distribution of energy in the universe, with their past and future.

It is found by experiment that some forms of energy are more easily or more completely transformable than others, and thus we speak of higher and lower forms, and are introduced to the enormously important consideration of the degradation, or, as it is more commonly called, the dissipation, of energy. The application of mathematical reasoning to the conservation of energy presented no special difficulties which had not, to some extent at least, been overcome in Newton's time: but it was altogether otherwise with the transformations of energy. And it is possible that, had it not been for the wonderfully original processes devised by Carnot in 1824, we might not now have secured more than a small fraction of the immense advances which science has taken during the last thirty years.

For a transformation of heat we must have bodies of different temperatures. Just as water has no "head" unless raised above the sea-level, so heat cannot do work except with the accompaniment of a transference from a hotter to a colder body. Carnot showed that to reason on this subject we must have *cycles* of operations, at the end of which the working substance is restored exactly to its initial state. And he also showed that the test of a *perfect* engine (*i.e.* the best which is, even theoretically, attainable) is simply that it must be *reversible*. By this term we do not mean mere backing, as in the popular use of the word, but something much higher—viz. that, whereas, when working directly, the engine does work during the letting down of heat from a hot to a cold body; when reversed, it shall spend the same amount of work while pumping up the same quantity of heat from the cold body to the hot one. As a reversible engine may be constructed (theoretically at least) with any working substance whatever, and as all reversible engines working under similar circumstances must be equivalent to one another (since each is as good as an engine can be) it is clear that the amount of work derivable from a given amount of heat under given circumstances (*i.e.* the amount of transformation possible) can depend only upon the temperatures of the hot and cold bodies employed. In this sense we speak of Carnot's Function of Temperature, which is as imperishably connected with his name as is the Dynamical Equivalent of Heat with that of Joule.

Building upon this work of Carnot, Sir W. Thomson gave the first *absolute* definition of temperature—that is a definition independent of the properties of any particular substance. Perhaps there is no term in the whole range of science whose meaning is correctly known to so few even of scientific men, as this common word temperature. It would not, I think, be an exaggeration to say that there are not six books yet published in which it is given with even an approach to accuracy. The form in which the definition ultimately came from the hands of Joule and Thomson enables us to state as follows the laws of transformation of energy from the heat form.

1. A given quantity of heat has a definite transformation equivalent.

2. But only a fraction of this heat can be transformed by means even of a perfect engine: and this fraction is DEFINED as the ratio of the range through which the heat actually falls to that through which it might fall—were it possible to obtain and employ bodies absolutely deprived of heat.

This definition has two great advantages. 1st, The utmost amount of work to be got from heat under any circumstances of temperature is determined by precisely the same law as that assigning the work to be had from water under similar circumstances of level. In this case the sea-level corresponds to what is called the Absolute Zero of temperature. [It is well to observe here that it is the potential energy of the water, not the quantity of water itself, which corresponds in this analogy to the quantity of heat. In this simple remark we have all that is necessary to correct Carnot's reasoning in so far as it was rendered erroneous by his assumption of the materiality (and consequent indestructibility) of heat.] 2nd, Temperatures thus defined correspond, as Thomson and Joule have shown by elaborate experiments, very closely indeed with those given by the air-thermometer—the absolute zero being about 274° of the Centigrade scale below the freezing point of water. I have made this digression as I shall have frequently to use the word temperature, and I shall always employ it in the sense just explained.

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[*] Abstract of the Rede Lecture delivered in the Senate House, Cambridge, May 23, 1873.

The subject of Thermo-electricity of course includes all electric effects depending on heat, but in this lecture I shall confine myself to the production by heat of currents in a circuit of two metals.

The transformation of heat into the energy of current electricity was first observed by Seebeck in 1820 or 1821. His paper on the subject (Berlin Ac., 1822-3, or Pogg. vi.) is particularly interesting, as he gives the whole history of his attempts to obtain a voltaic current from a circuit of two metals without a liquid, and the steps by which he was led to see that heat was the active agent in producing the currents he eventually obtained. In this paper Seebeck gave the relative order of a great number of metals and alloys in the so-called thermo-electric series, and showed that several *changes of order* occurred among them as the temperature was gradually raised.

In a note attached to this paper, Seebeck recognises that in this further discovery he was anticipated by Cumming (who seems, in fact, to have made an independent discovery of Thermo-electricity). Cumming showed that when wires of copper, gold, &c., were gradually heated with iron, the deflection rose to a maximum, then fell off, and was *reversed* at a red heat.

[Seebeck's original experiment and Cumming's extension of it were exhibited.]

You see that, keeping one of the copper-iron junctions at the temperature of the room and gradually heating the other, I produce a current which increases in intensity more and more slowly till it reaches a maximum, then falls off faster and faster till at last it vanishes and thereafter sets in the *opposite* direction. We are still far below the melting point of copper, yet further heating up to that point produces but little additional effect. The reason of this will be apparent from some facts to be described towards the end of the lecture. At the moment of maximum current the two metals are thermo-electrically *Neutral* to one another.—The temperature in the present case is about 280° C.

Seebeck pointed out that bismuth and antimony (to the choice of which he had been led by a very curious set of arguments) were very far removed from one another in the series, and therefore gave large effects for small differences of temperature. This is still taken advantage of in the Thermo-electric Pile, which, when combined with a sufficiently delicate galvanometer, is even now by far the most delicate thermometer we possess. It has recently enabled astronomers to detect and measure the heat which reaches us from the moon, and even from the brighter fixed stars. In the skilful hands of Forbes and Melloni this instrument was the effective agent in demonstrating the identity of thermal and luminous radiations—a step which, as regards the simplification of science, is as important as the discovery of magneto-electricity; and which was completed by Forbes when he succeeded in polarising radiant heat.

But when we come to look at this question from the point of view of transformation of energy, we have to ask *where* is the absorption, and *where* the letting-down of heat, to which the development of the current considered as a rise of energy is due. Very remarkably, an experiment of Peltier supplies us with at least part of the answer. Peltier showed that, given a metallic junction which when heated would give a current in a certain direction, then provided a battery were interposed in that circuit (initially at a uniform temperature) so as to send a current in that direction, the passage of the current *cooled* the junction, while a reversal of the current heated it. This, considering the circumstances under which it was made, and the deductions since drawn from it, is one of the most extraordinary experimental discoveries ever made. Water was frozen, in an experiment by Lenz, by means of the Peltier effect.

Here then is a reversible heat effect, and to it we may reasonably assume that the laws of thermodynamics may be applied; although from the very nature of the experiment the reversible effect must always be accompanied by non-reversible ones, such as dissipation by heat-conduction, and by heat generated in consequence of the resistance of the circuit. The latter of these is in general small in thermo-electric researches, but the former may have large values.

It is known from the beautiful experiments of Magnus that no thermo-electric current can be produced by unequal heating in a homogeneous circuit, whatever be the variations of section—a negative result of the highest importance. Sir W. Thomson, to whom we are indebted for the first and the most complete application of thermodynamics to our subject, showed that the existence of a neutral point necessitates the existence of some other reversible effect besides that of Peltier. And even if the circuit varied in section, the result of Magnus, just referred to, showed that this could only be of the nature of a convection of heat by the current between portions of the same metal at different temperatures. Thomson's reasoning is of the very simplest character, as follows:—Suppose the temperature of the hotter junction to be that of the neutral point, there is no absorption or evolution of heat there; yet there is evolution of heat at the colder junction, and (by resistance) throughout the whole circuit. The energy which supplies this must be that of the heat in one or both of the separate metals; but reasoning of this kind, though it proves that there must be such an effect, leaves to be decided by direct experiment what is the nature and amount of this effect in each of the metals separately. By an elaborate series of ingenious experiments Thomson directly proved the existence of a current convection of heat, and (curiously enough) of opposite signs in the first two metals (iron and copper) which he examined. In his own words, "Vitreous Electricity carries heat with it in an unequally heated copper conductor, and Resinous Electricity carries heat with it in an unequally heated iron conductor." This statement is not very easy to follow. It may perhaps be more intelligible in the form:—In copper a current of positive electricity tends to equalise the temperature of the point it is passing at any instant with that of the point of the conductor which it has just left, *i.e.*, when it passes from cold to hot it tends to cool the whole conductor; when from hot to cold, to heat it, thus behaving like a real liquid in an irregularly heated tube. The effects in iron are the opposite; and Thomson therefore speaks of the specific heat of electricity as being thus positive in copper and negative in iron. He gives a very remarkable analogy from the motion of water in an endless tube (with horizontal and vertical branches), produced by differences of density, due to differences of temperature. Here the maximum density of water plays a prominent part. Neumann has recently attempted, by means of the laws of motion of fluids, and the unequal expansibility of different metals, to give a physical explanation of thermo-electric currents. But, not to speak of the fact that positive electricity is by him considered as a real fluid, there are the fatal objections that his method makes no provision for the explanation of the Peltier, or of the Thomson, effect; and therefore cannot be looked upon as having any useful relation to the subject. Similar remarks apply to the attempt of Avenarius to account for thermo-electric currents by the variation with temperature of the electrostatic difference of potentials at the points of contact of different metals.

By employing the thermo-electric pile instead of the thermometers used by Thomson, Le Roux has lately measured the amount of the specific heat of electricity in various metals, and has shown that it is very small, or altogether absent, in lead. Strangely enough, though he has verified Thomson's results, he does not wholly accept the theoretical reasoning which led to their prediction and discovery.

One of Thomson's happiest suggestions connected with this subject is the construction of what he calls a thermo-electric diagram. In its earliest form this consisted merely of parallel columns, each containing the names of a number of metals arranged in their proper thermo-electric order for some particular temperature. Lines drawn connecting the positions of the name of any one metal in these successive columns indicate how it changes its place among the other metals as the temperature is raised. Thomson points out clearly what should be aimed at in perfecting the diagram, but he left it merely as a preliminary sketch. The importance of the idea, however, is very great; for, as we shall see, the diagram when carefully constructed gives us not merely the relative positions of the metals at various temperatures, with the temperatures of their neutral points, but also gives graphic representations of the specific heat of electricity in each metal in terms of the temperature, the amount of the Peltier effect, and the electromotive force (and its direction) for a circuit of any two metals with given temperatures of the junctions. In short, the study of the whole subject may be reduced to the careful drawing by experiment of the thermo-electric diagram, and the verification of Thomson's thermo-dynamic theory will then be effected by a direct determination either of Peltier effects or of specific heat of electricity at various temperatures, and their comparison with the corresponding indications of the diagram.

The diagram is constructed so that abscissæ represent absolute temperatures, and the difference of the ordinates of the lines for any two metals at a given temperature is the electromotive force of a circuit of these metals, one of the junctions being half a degree above, the other half a degree below, the given temperature.

It will be seen by what follows that nothing but direct measurement of the value of the specific heat of electricity at various temperatures can give us the actual form of the line representing any particular metal; but if the line for any one metal be assumed, those of all others follow from it by the process of differences of ordinates just described. So that it is well to begin by assuming the axis of abscissæ as the line for a particular metal (say lead, in consequence of Le Roux's result); and if, at any future time, this should be found to require change, a complex shearing motion of the diagram parallel to the axis of ordinates will put all the lines simultaneously into their proper form.

Thomson's theoretical investigation may be put in a very simple form as follows:—Let us suppose an arrangement of two metallic wires, one end of each of which is heated, their cold ends being united, and in which the circuit can be closed by a sliding piece or ring, always so placed as to join points of the two metals which are at the same temperature t. Let E be the electromotive force in the circuit, Π the Peltier effect, and σ_1, σ_2 the specific heats of electricity in the two metals. Then, if the sliding-piece be moved from points at temperature t to others at $t + \delta t$, the first law of thermodynamics gives by inspection the equation

$$\delta E = J\,(\delta\Pi + \overline{\sigma_1 - \sigma_2}\,\delta t),$$

and the second law gives

$$0 = \delta\left(\frac{\Pi}{t}\right) + \frac{\sigma_1 - \sigma_2}{t}\,\delta t.$$

These equations show at once that, if there were no electric convection of heat, or if it were of equal amount in the two metals, the Peltier effect would always be proportional to the absolute temperature; and the electromotive force would be proportional to the difference of temperatures of the junctions; so that there could not be a neutral point in any case. In fact, the lines in the diagram for all metals would be parallel: and, on the former of the two hypotheses, parallel to the axis of abscissæ.

Eliminating $\sigma_1 - \sigma_2$ between the equations, we have

$$\delta E = J\,\frac{\Pi}{t}\,\delta t.$$

Now, by the construction of the diagram, $\frac{dE}{dt}$ is the difference of the ordinates of the lines for the two metals at temperature t. Hence, *whatever be the form of the lines for two metals*, the Peltier effect at a junction at temperature t is always proportional to the area of the rectangle whose base is the difference of the ordinates, and whose opposite side is part of the axis of ordinates corresponding to absolute zero of temperature. This area becomes less and less as we approach the neutral point, and changes sign (i.e., *is turned over*) after we pass it; the current being supposed to go from the same one of the two metals to the other in each case.

The electromotive force itself, being the integral of $\frac{dE}{dt}$ between the limits of temperature, is proportional to the area intercepted between the lines of the two metals, and ordinates drawn to correspond to the temperatures of the junctions respectively.

Again, the second of the preceding equations shows us that the difference of specific heats in the two metals is proportional to the absolute temperature and to the difference of the tangents of the inclinations of the lines for the metals to the axis of abscissæ. If we assume this axis to be the line of a metal in which the electric convection of heat is wholly absent, the measure of this convection in any other metal is simply the product of the absolute temperature into the tangent of inclination of its line to the axis. Thus, if the thermo-electric line for a metal be straight, electric convection is in it always proportional to the absolute temperature; and it is positive or negative according as the line goes off to infinity in the first or in the fourth quadrant. If the lines for any two metals be straight, and if one junction be kept at a constant temperature, the electromotive force will be a parabolic function of the temperature of the other junction—the vertex of the parabola being at the temperature of the neutral point of the two metals, and its axis being parallel to the axis of ordinates.

For the benefit of such of my audience as are not familiar with mathematical terms, I may give an illustration which is numerically exact. Let time stand for temperature, years corresponding say to degrees. Let the ordinate of one of the metals represent a man's income, that of the other his expenditure. The difference of these ordinates represents the rate of increase of his capital or accumulated savings, which here stands for electromotive force. As long as income exceeds expenditure, the capital increases; when income and expenditure are equal (*i.e.*) at a "neutral point," capital remains stationary, indicating, in this case, a maximum value; for in succeeding years expenditure exceeds income, and capital is drawn upon. P. G. TAIT

(*To be continued.*)

ON THE SPECTROSCOPE AND ITS APPLICATIONS

IX.

NOW let me state to you how the discovery mentioned on p. 12 was finally established by Kirchhoff. In my notice of the spectroscope in the earlier articles, I had so much to say that there were several details it was absolutely essential I should curtail. One of these details was the scale by which the positions of the different bright or dark lines which are seen in the different spectra are registered, so that we may say that such a line occupies such and such a position, and such another line occupies such another position, with regard to something else. When Kirchhoff and Bunsen, two German chemists, were engaged in mapping the spectra of the elements—a research which at its commencement had nothing whatever to do with the sun—they came across this difficulty of a scale. How could they get a good scale? I have already referred to some very obvious arrangements that might determine the actual position; for instance, the observing telescope may be made to move along a graduated arc, so that by moving the telescope for the different rays and fixing it when in a proper position to see a particular ray, you might read off the index placed on the arc to a great nicety by means of a graduated *vernier* working on the curve of the arc; or you may, by a modification of the instrument, use a reduced photographic picture of a scale, so that the thing to be measured and the actual scale would appear in the field of view at the same time. Kirchhoff and Bunsen tried these methods, but they did not like them. Then it suddenly struck them that, as they made their experiments in the day-time, they might use as a scale the black lines in the solar spectrum, which had not been known to change since the time of Wollaston, who discovered them. When working in the daytime, they had thus the solar spectrum visible in one half of the field of view of the telescope, which was easily managed by placing a reflecting prism over one half of the slit, as is shown in the enlarged slit in Fig. 46, so as to light one half of the slit by the sun, and the other half by whatever substance was under examination. With this arrangement they set to work with infinite care, and made a map of the solar spectrum. Such was their proposal: first to map the unchangeable solar spectrum, and then, having this unchangeable scale, about which there could be no mistake, always visible, they would be able to refer to the dark lines in it all the unknown phenomena they were about to investigate in the bright lines of different vapours and gases. Having got this idea of the scale well into their minds, they were exceedingly anxious to test this question, which, as I have told you, was raised by Fraunhofer and many other men before them, of the asserted coincidence of the bright sodium line with the dark solar sodium lines; with a very delicate instrument, Prof. Kirchhoff made the following remarkable experiment:—"In order," says Kirchhoff, for these are his own words, "to test in the most direct manner possible the frequently asserted fact of the coincidence of the sodium lines with the lines D"—(that is to say, of the bright double lines of sodium in the yellow part of the spectrum, with the double line D of the solar spectrum)—"I obtained a tolerably bright solar spectrum, and brought a flame coloured by sodium vapour in front of the slit. I then saw the dark lines D change into bright ones." That is to say, in the spectrum of the sodium which was burning in the flame were lines so exactly coincident with the two dark lines in the solar spectrum, that the bright lines of the sodium spectrum put these dark lines out altogether, so that they seemed to vanish, as it were, from the solar spectrum. He goes on:—"In order to find out the extent to which the intensity of the solar spectrum could be reduced without impairing the distinctness of the sodium lines, I allowed the full sunlight to shine through the sodium flame." Here he varies the experiment. In the first instance he used a very feeble beam of sunlight, but he now allows the whole glare of the sun to enter the slit. What was the result? "To my astonishment, I saw that the dark lines D appeared with an extraordinary degree of clearness." That is to say, the lines which came from the sodium in the first instance, were sufficiently bright to entirely eradicate the dark lines from the solar spectrum, but the two lines D were now

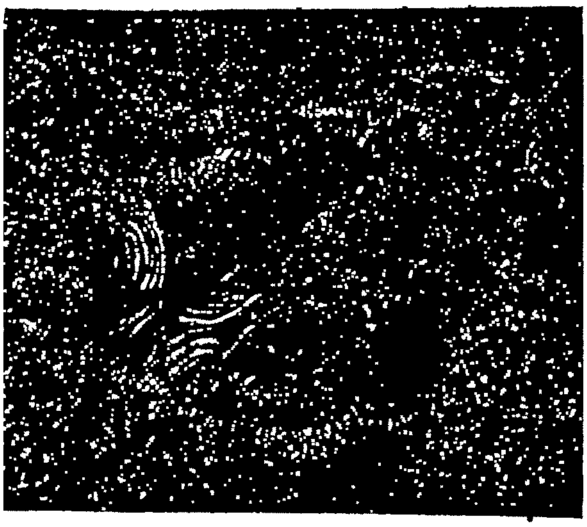

FIG. 50.—A sun-spot (Secchi), showing the "straws" in the penumbra, and the irregular masses on the general surface.

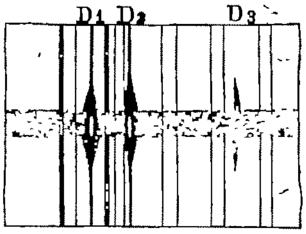

FIG. 51.—Spectrum of sun-spot. (Young.)

FIG. 52.—Spectrum of τ Coronæ (Huggins.)

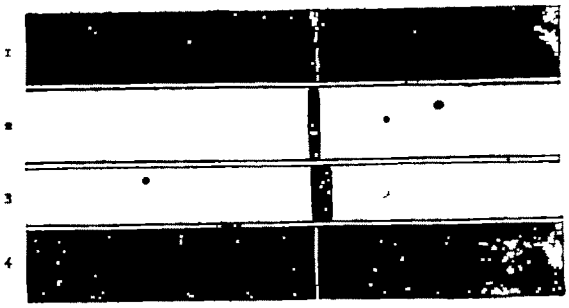

FIG. 53.—Alteration of wave length of the hydrogen in the atmosphere of Sirius. 1, Hydrogen at atmospheric pressure; 2, Solar Spectrum Line F, 3, Spectrum of Sirius; 4, Hydrogen in vacuum tube.

so utterly powerless compared with the light of the sun, that they actually appeared as black lines, and coincident with the two lines D in the solar spectrum.

We have seen that the bright line due to the radiation from sodium vapour can be very easily obtained by placing some sodium in a colourless gas flame, but if we now pass the continuous light coming from the carbon points of an electric light, or from the oxyhydrogen lime-light, through this same sodium flame, the result will be that we obtain a black absorption line on a continuous spectrum, in precisely the same position as the yellow line was originally. This is Kirchhoff's crucial experiment, which at once determined not only that the dark line in the sun was absolutely coincident with the bright line of sodium vapour, but that, under certain conditions, bright, incandescent sodium vapour could actually be made to absorb the light coming through it, and reverse its own spectrum. Kirchhoff goes on:—"I then exchanged the sunlight for the Drummond or oxyhydrogen lime-light, which, like that of all incandescent solid and liquid bodies, gives a spectrum containing no dark lines." When this light was allowed to fall through a suitable flame, coloured by common salt (or chloride of sodium), dark lines were seen in the spectrum in the position of the sodium lines." You may imagine that this conclusive experiment—perhaps the most wonderful experiment that has been made during the century—gave Kirchhoff food for thought, and at once his genius travelled to a possible explanation of this strange fact he had observed; a fact, as you know, entirely in accordance with the previsions of Prof. Stokes, Dr. Balfour Stewart, and Foucalt. Kirchhoff said to himself, "I have now got the bright lines in the spectrum of the vapour of sodium coincident with the two dark lines in the solar spectrum. What does it mean?" And again the philosopher was not at fault. He said to himself—it is almost possible to see the train of his reasoning in his memoirs—"Sodium has a most simple spectrum; suppose I take the most complicated spectrum I can find." He took for this purpose the spectrum of iron, which I think you will acknowledge to be one of sufficient complication, for the spectrum is traversed by lines throughout its whole length, and I may tell you at once that no less than 460 lines have been already mapped, and their positions are now thoroughly well known to us—as well known as the position of any star in the heavens. Kirchhoff tried the iron spectrum, and he found, absolutely corresponding in position in the spectrum and in width and darkness to the bright iron lines which he saw, black lines in the solar spectrum. He waited no longer; he instantly convinced himself, and soon convinced the world, that he had discovered this very remarkable fact, that gases and vapours have the power of absorbing those very rays which they themselves give out when in a state of incandescence. So that, if you take sodium, and get its bright lines, and mark their positions on the screen, and then observe a continuous spectrum, and interpose sodium vapour in the path of the beam, you will find black lines absolutely corresponding with the bright ones; that is to say, that the sodium vapour has the faculty of entirely eating up, absorbing, or stopping that light which would otherwise go on to the screen. In the case of iron, it is worthy of notice that when Kirchhoff made his discovery, he was only able to obtain a spectrum of iron consisting of something like 90 lines, but since then the spectrum of iron has been mapped to the extent of 460 lines, and sure enough there are solar lines corresponding to nearly all the 460 bright lines which we are able to get in our laboratories. Not only was the bright line of sodium *reversed* or changed into a dark one, but it was soon found that the lines of other metals, such as lithium, potassium, strontium, calcium, and barium, could be reversed in a similar manner. This grand discovery of Kirchhoff's met with immediate acceptance, and with it you see at once the explanation of the wonderful black lines discovered by Wollaston, about which I said something in my first lecture. The riddle of the sun was read to a certain extent, and Kirchhoff read it in this way. He said:—"There is a solid or a liquid something in the sun, giving a continuous spectrum, and around this there are vapours of sodium, of iron, of calcium, of chromium, of barium, of magnesium, of nickel, of copper, of cobalt, and aluminium; all those are existing in an atmosphere, and are stopping out the sun's light. If the sun were not there, and if these things were observed in an incandescent state, we should get exactly these bright lines from them." Later researches by many distinguished physicists have shown that the following terrestrial elements are present in the vaporous condition round the sun:—

1. Sodium.
2. Calcium.
3. Barium.
4. Magnesium.
5. Iron.
6. Chromium.
7. Nickel.
8. Copper.
9. Zinc.
10. Cadmium.
11. Cobalt.
12. Hydrogen.
13. Manganese.
14. Aluminium.
15. Titanium.

Kirchhoff further imagined that he had reason to believe that the visible sun, the sun which we see—and we may take the sun as an example of every star in the heavens—was liquid.

In the sun we have, first, a bright, shining orb, dimmed to a certain degree at the edge; and here and there, over the sun, we see what are called spots. Kirchhoff wished, not only to connect his discoveries with the solar atmosphere, but was anxious to connect them with this dimming near the limb and the spots. He said that the solar atmosphere, to which all the absorption lines were due, extended far outside the sun, and formed the corona; and that this dimming of the limb was really due to the greater absorption of this atmosphere, owing, of course, to the light of the sun travelling through a much greater length at the limb than at the centre of the disc. Furthermore, he said that the sun-spots, which astronomers, from the time of Wilson, had asserted to be cavities, were nothing but clouds floating in this atmosphere of vapour. Such was the very bold hypothesis put forward by Kirchhoff—an hypothesis which you see at once explains these strange observations from Wollaston upwards, including Fraunhofer's observation of the spectrum of the sun and stars, and the brilliant ideas of Prof. Stokes, Dr. Balfour Stewart, and others in other lands. A little simple experiment, made by means of a little sodium vapour and a beam of sunlight, with the powerful aid of a little prism, gave us this tremendous knowledge about distant worlds so immeasurably remote that it seemed absurd for men to try and grapple with any of the difficulties that are presented to us. Such, then, is Kirchhoff's theory of the sun, which I hope I have been able to make clear to you. There is a something—Kirchhoff said it was a liquid—which gives us a continuous spectrum, and between our eye and that incandescent liquid surface there is an enormous atmosphere, built up of vapours of sodium, iron, and so on; and the reason that we get these dark lines is, that the molecules of the substances named absorb certain rays, because when they are in an incandescent state they produce them. This brilliant idea of Kirchhoff's was soon carried, as you know, to the stars by Mr. Huggins in our own country. In Fig. 34 will be seen the spectra of two stars, Aldebaran and α Orionis (Betelgeux), which are so distant that it is absolutely impossible to measure their distance from us. We know a great deal about our own sun, but these suns are so lost in the depths of space that it is quite impossible that we can get anything like a correct knowledge of their size, or know much of their belongings. By means of the prism, however, we learn in a moment a great deal. In the first star we get three lines, due to the absorption of magnesium vapour, as we get them in the sun. We know, therefore, that magnesium vapour is present in the atmosphere around that sun (Aldebaran) in exactly the same way as round our own. We also get some of the iron lines, the lines of sodium, and the lines of hydrogen, calcium, and a few other elements—nine in all. At the base of the diagram you see indications of the elements, with the bright lines of which Mr. Huggins has compared the black lines which you see in the spectrum of these heavenly bodies. By means of the star spectroscope and of the induction coil, Mr. Huggins tested these lines, as Kirchhoff did in the case of the sun, by actually getting the vapour of magnesium visible at the same time in the spectroscope: and thus you see in a moment that there is no difficulty at all in determining their coincidence, you have the two things brought so closely side by side. If I had time I might remark on the presence of some elements here and the absence of others; but there is one remarkable fact about this lower star (α Orionis) which I must mention. As far as its spectrum goes, it appears that the gas hydrogen, which is a very important element in our sun's atmosphere, as we gather from the great distinctness of the hydrogen lines in the solar spectrum—and not only in our sun, but in a great many others—is absolutely absent, whilst magnesium, sodium, calcium, &c., are present.

So far, then, you see that this little prism has enabled us to read a great many secrets of the sun and of the more distant stars; and we must acknowledge that Stokes' and Kirchhoff's hypothesis is a very magnificent one, and we can but wish that there were more men like them, who, undismayed by the failure of those who, for nearly a century before their time, had been endeavouring to unravel these secrets, were still prepared to go on, and endeavour to find them out by means of a prism and a simple sodium flame.

Now, astronomers—who, as I told you, from the time of Wilson had imagined that the sunspots were cavities—very soon began to quarrel with this hypothesis of Kirchhoff's, who said that the sunspots, instead of being cavities, were really clouds floating in the atmosphere. They remarked, and I think with truth, that to make such an assertion was altogether opposed to the evidence of the telescope. And I think I may say that the astronomers have now carried the day, for another line of independent research altogether—I mean the researches into the constitution of the sun by means of the spectroscope—has come to the aid of the astronomers, and it looks very much as if we must still hold to the opinion that Wilson in his observations, now more than a century old, was perfectly right, and that Kirchhoff's analysis, as far as it deals with the sun-spots, is susceptible of improvement. In the remarks I made in my former lecture on radiation in connection with the red prominences visible during eclipses, I drew your attention particularly to the hydrogen lines, and told you that the red flames are, for the most part, composed of hydrogen. There the prism comes to our aid in a very remarkable way indeed. It is clear to you, I think, after what I have said about absorption, that the darkening of the sun's surface, which we call a spot, is really a thing about which the prism can tell us a great deal. For instance, take a sun-spot, in which the usual brilliancy of the sun in the other parts of its disc is altogether wanting. There is not only great darkness here and there, but wonderful turnings and twistings and bendings of this solar envelope, which I have already told you Kirchhoff asserts to be a liquid one, but which I think a little consideration of Fig. 50 will show you is more probably gaseous, or cloudy, than liquid. It is obvious, I say, in this case that there was a great probability of the spectroscope being able to tell us something about this absence of light, for an absence of light means one of two things; it means either that there was a defect in radiation, or that there was some excess of absorption, and I may say that this difference—which I hope you now all thoroughly understand—really formed the battle-ground between the English and French astronomers until a few years ago. Long after Kirchhoff's experiment, M. Faye, a distinguished member of the Institute of France, went all over the work again, and declared that the sun-spot was dark, because we there got the light, not from the brightly shining envelope, but from some feebly radiating gas inside the sun; that the sun was a gigantic bubble, the bubble being nothing else than the photosphere—the liquid sphere of Kirchhoff—the interior being composed of gas, glowing at such an enormous temperature that the light we got from it was extremely feeble. You will see in a moment that, if the sun-spot were really due to the radiation from gas, we should get from that sun-spot a selective spectrum, that is to say, a spectrum with bright lines. The English astronomers said: "No; a sun-spot is not due to defective radiation at all; there is something over the bright portion of the sun which eats away the light:" whether the light was eaten away generally—whether, in fact, we had an instance of general or selective absorption—was not stated, but what they did distinctly state was, that the sun-spot was simply an indication of absorption. So that, you see, here was a thing which a spectroscope might settle almost at once, provided always that a good sunspot could be obtained for the experiment. This was done in 1866. Fig. 51 gives an idea of what is seen when we observe a small sunspot, and it is one which is full of meaning. Here is a very clear image of the solar spectrum near the double line D, and also the double D itself. If it were possible to have given you the whole of the sun's spectrum on the same scale as this, it would require an engraving yards in length, but it would be almost impossible to make my meaning clearer than I hope I can do by this small portion; and I must therefore ask you to take for granted that the dark line which you see running along this yellow portion of the spectrum would really run along the whole length of the spectrum, from the extreme red to the extreme violet. This, then, you see in a moment, was an indication of general absorption; that is to say, in the way in which the light is affected by its passage through the prism, we have the problem settled in an instant, that a sunspot is due to general absorption at all events. Further, in observing the spectra of different sunspots, it was found that the spectrum of the middle of the sunspot is much darker than the outside. So that you see this simple experiment tells us not only that the sunspot is due to general absorption, but that there is more general absorption in the middle of the spot than at its edge. This is the way in which this little prism is able to deal with these great problems.

J. NORMAN LOCKYER

(*To be continued.*)

MIND IN THE LOWER ANIMALS

I RECENTLY received a letter from Mons. J. C. Houzeau, the author of the "Études sur les Facultés Mentales des Animaux comparées a celles de l'Homme," published at Mons, Belgium, in 1872, and reviewed by Mr. Wallace in NATURE of October 10, 1872. The latter eminent writer asserts that M. Houzeau's work "contains a mass of curious facts, acute observations, and sound reasoning, which fully entitle its author to take high rank among philosophical naturalists" (p. 471). I quite agree with him in his estimate of M. Houzeau's labours, being disposed to place his two volumes of "Études" on a par with the works of Mr. Darwin; and with another work, which, while little, if at all, known in this country, deserves, nevertheless, the highest consideration at the hands of all interested in comparative psychology—the "Traité de la Folie des Animaux de ses Rapports avec celle de l'Homme," by Dr. Pierquin, published in Paris (in 2 vols.), so long ago as 1839.

I need not say that any suggestions coming from an observer of such experience as M. Houzeau deserve the attention of the now many earnest students of the subject of "Mind in the Lower Animals;" and I therefore make no apology for bringing under the notice of your readers certain remarks contained in the letter aforesaid.

In the first place, M. Houzeau begs to direct attention to "the high importance of sparing—at least for observation—what remains of anthropoid animals in Asia and Africa. It is my deep regret that there are none in the country where I live" (Jamaica); "and that I am thereby deprived of an opportunity to study them. They should be tamed, domesticated, and studied in their own climate—at home. The gorilla, for instance, should be perpetuated in Guinea in domesticity. As I stated in my book, it does not appear impossible that apes might learn to talk. Should the attempt succeed even partially, what would not be the bearing and importance of it physiologically and historically? Could not some means of study be devised in the English colonies? To save the Anthropoids from destruction, and to promote the study of their mental capacity, is worthy surely of the earnest exertions of naturalists."

I quite concur with him as to the desirability of educating by domestication—so far as possible, and studying the results of such education in the anthropoid apes, and indeed the whole group of the Quadrumana. We know what has been the result in the dog of centuries of association with, and training by, man; though even in that familiar animal we do not yet know the extent of his capabilities, because training in certain directions has scarcely been attempted. Man has, for his own ends, directed special attention and effort to the development, in the dog, of his power of scent, swiftness, vision, courage, watchfulness, and other qualities that render him useful in the chase, as a watch-animal, as a companion, and so forth. But no similar persistent efforts have been made to cultivate, for instance, his moral sense—to produce an animal good in a moral point of view—honest, affectionate, benevolent, conscientious, in the highest degree. And yet that it is quite as possible to produce or educe moral greatness or goodness as physical swiftness or muscular strength, I am firmly persuaded. Notwithstanding all that has been said of the superior intelligence of the dog, horse, elephant, ant, and bee, I believe that were as much care bestowed on the training of the moral qualities of many monkeys or apes as is given to the instruction of the pointer or setter, the homing pigeon, piping bullfinch, or talking parrot, or to the training of the race-horse, results of a startling kind would be attained, or would be shown to be attainable. There are certain respects in which apes and monkeys approach more closely to man than do the dog or the other animals just mentioned: they possess potentialities or capabilities of which some of the almost marvellous stories told us by reputable traveller-naturalists give us but a glimpse.

I cannot, however, discuss that or other subjects in comparative psychology here, hoping, as I do, to have fuller and more fitting opportunity in a forthcoming volume of the "International Scientific Series" of Messrs. H. S. King and Co.

M. Houzeau expresses surprise that, at the present day, the belief should be almost universal that, while all races and conditions of man have souls, the best of other animals have none. This is obviously a matter of pure speculation, which I must not now discuss. But I may direct the attention of your readers to a curious book published in Aberdeen in 1824, by Peter Buchan, entitled, "Scriptural and Philosophical Arguments or Cogent Proofs from Reason and Revelation that Brutes have Souls, and that their Souls are Immortal." The work in question is, however, now so rare, that it may be difficult to obtain even a perusal of it. The reader of German literature may also refer to a book on the same subject by Schmarda, to which my attention was called some time ago by the late Professor Day, of St. Andrews.

M. Houzeau animadverts on the anomaly that the persons, from whom we should expect the most valuable evidence regarding the mental acquirements or capacities of the lower animals—those who are habitually and intimately associated with them—drovers and drivers, horsemen and huntsmen, shepherds and sportsmen, jockeys and grooms, butchers, and even veterinarians, are those, on the contrary, in whom we too frequently meet with the strangest ignorance or prejudice. They would seem to be, as a rule, incapable of honestly observing and of making logical inferences from facts observed; instead of using their own eyes and reason, they permit themselves to be blinded and befooled by obsolete tradition or fable.

Notwithstanding the perfectly overwhelming bulk and variety of the literature of comparative psychology—or at least of the data on which it may be founded, there are many points in the mental history of the lower animals that require and admit of elucidation by *observation and experiment*. If any person of ordinary intelligence—either abroad or at home—feels inclined to plead, as an excuse from contributing to the progress of comparative psychology, the want of proper opportunity, I would commend to his consideration the example of M. Houzeau as a noble one of the successful "pursuit of knowledge under difficulties." He modestly describes himself as a traveller-naturalist: and in the letter above referred to thus refers to the circumstances under which he collected the materials for the two bulky volumes of *Etudes*, that constitute one of the most important contributions yet made to the science of comparative psychology. "It was rather occasionally that my attention was called to the subject of the 'Mental faculties of animals,' having been almost exclusively engaged, previous to my sojourn in America, in astronomical and geographical pursuits. Still the subject was pressed upon me when, in the wildernesses of Texas and Northern Mexico, I had to live in the open air, in the constant company of domestic animals and in close proximity to wild ones; far away," as he says, "from the European field of labour and even from intellectual resources," in a foreign wild land, without the means of literary or scientific reference. Under circumstances, in a word, most unfavourable to such a publication, he has nevertheless produced a work that would do honour to any of our own *savans*, with all the appliances of our large cities, large societies, and large libraries at their command.

W. LAUDER LINDSAY

NOTES

FREE admission to the lectures and courses of practical instruction in Chemistry, Physics, Mechanics, and Biology at South Kensington will be granted to a limited number of Teachers and Students of Science Classes under the Science and Art Department, who intend to become Science Teachers. The selected candidates will also receive a travelling allowance, and a maintenance allowance of 1*l*. 1*s*. per week, while required to be present in London. The course in Chemistry will commence in October, and end in the following June. The course in Biology will commence in October and close in February or March. The course in Physics will commence about February and close in June. The course in Mechanics will probably commence about February and close in June. Students are required to attend from 9 or 10 A.M. to 4 or 5 P.M. daily, in addition to the time required in the evening for writing up their notes, &c. Candidates for these Studentships must send in their applications on Science Form No. 400, copies of which may be obtained on application to the Secretary of the Science and Art Department. For the courses in Biology and in Mechanics some power of drawing is essential, and no candidate will be admitted who cannot show that he has acquired sufficient power.

THE following courses of instruction of Science Teachers in connection with the Science and Art Department will probably be organised this summer.—1. Chemistry, Inorganic, 2. Chemistry, Organic, 4 weeks, commencing July 1, Prof. Frankland, F.R.S. 3. Magnetism and Electricity, 3 weeks, commencing June 24, Prof. Guthrie, F.R.S. 4. Heat and Light, 3 weeks, commencing July 17, Prof. Guthrie, F.R.S. 5. Botany, 4 weeks, commencing June 24, Prof. Thiselton Dyer. 6. Mechanics, 4 weeks, commencing June 25, Prof. Goodeve. 7. Geometrical Drawing, 3 weeks, commencing June 26, Prof. Bradley. Before definite arrangements can be made, however, it is necessary to know how many Teachers can and will take advantage of the courses; and therefore all Teachers who wish to attend are required to fill up and return a form (Science Form, No. 500), which may be obtained by application to South Kensington. If more Teachers apply to attend than can be accommodated at any course, those will be selected who have passed the highest examinations—in which the result of the present May Examination will be counted—and have had the most successful classes. The Teachers who are selected, and who attend one or more of the courses, will receive 2nd class railway fare and 30*s.* a week while in London.

IN connection with St. John's College, Cambridge, there will be offered for competition, in December 1873, an Exhibition of 50*l.* per annum for proficiency in Natural Science, the Exhibition to be tenable for three years in case the Exhibitioner have passed within two years the previous examination as required for candidates for honours; otherwise the Exhibition to cease at the end of two years. Candidates will have a special examination in (1) Chemistry, including practical work in the laboratory; (2) Physics (viz., Electricity, Heat, Light); (3) Physiology. They will also have the opportunity of being examined in one or more of the following subjects:—(4) Geology; (5) Anatomy; (6) Botany; provided that they give notice of the subjects in which they wish to be examined four weeks prior to the examination. No candidate will be examined in more than three of these six subjects, whereof one at least must be chosen from the former group. It is the wish of the Master and Seniors that excellence in some single department should be specially regarded by the candidates. They may also, if they think fit, offer themselves for examination in any of the classical or mathematical subjects. Candidates must send their names to one of the tutors fourteen days before the commencement of the examination. The tutors are the Rev. S. Parkinson, D.D., Rev. T. G. Bonney, B.D., and J. E. Sandys, Esq., M.A.

FROM Prof. E. D. Cope we have received the description of two apparently new fossil mammalian forms from the Eocene of Wyoming, which he places among the Carnivora. *Mesonyx obtusidens* forms, according to the author, a distinct family of the fissiped Carnivora, most closely related to the Canidæ, with weakly sectorial teeth, four of them being true molars (a marsupial character), and short, flattened, ungual phalanges in which there are no indications of collars for the reception of the nails themselves. *Synoplotherium lanius* may be a Carnivore, but the claws were flat, and the scaphoid of the carpus did not anchylose with the lunare, which shows that it belongs to a more generalised type. It must be remembered that Prof. Marsh has described very similar forms from the same strata.

MESSRS. WILLIAMS AND NORGATE have just issued the prospectus of a unique and most elaborate work by Mr. Herbert Spencer, consisting to a large extent of the tabulated material which he has accumulated for his "Principles of Sociology." In preparation for the latter work, requiring as bases of induction large accumulations of data, fitly arranged for comparison, Mr. Herbert Spencer, some five years ago, commenced, the collection and organisation of facts presented by societies of different types, past and present. Though this classified compilation of materials was entered upon slowly to facilitate his own work, yet, after having brought the mode of classification to a satisfactory form, and after having had some of the tables filled up, the results appeared likely to be of such value that Mr. Spencer decided to have the undertaking executed with a view to publication: the facts collected and arranged for easy reference and convenient study of their relations, being so presented, apart from hypotheses, as to aid all students of Social Science in testing such conclusions as they have drawn and in drawing others. The work consists of three large divisions. Each comprises a set of tables exhibiting the facts as abstracted and classified, and a mass of quotations and abridged extracts, otherwise classified, on which the statements contained in the tables are based. The condensed statements, arranged after a uniform manner, give at one view, in each table or succession of tables, the phenomena of all orders which each society presents—constitute an account of its morphology, its physiology, and (if a society having a known history) its development. On the other hand, the collected extracts, serving as authorities for the statements in the tables, are (or rather will be, when the work is complete) classified primarily according to the kinds of phenomena to which they refer, and secondarily according to the societies exhibiting these phenomena; so that each kind of phemonenon, as it is displayed in all societies, may be separately studied with convenience. The three divisions, each thus constituted, comprehend three groups of societies:—(1) *Uncivilised Societies*; (2) *Civilised Societies—Extinct or Decayed*; (3) *Civilised Societies—Recent or still Flourishing*. Several sample tables have been sent us, and as a specimen of the classificatory headings under which the immense array of facts are grouped, we shall give those belonging to Table IX. of Division I. ("Uncivilised Races"), the Sandwich Islanders, one of the Malayo-Polynesian Races. First are given their Inorganic Environment (Climate, Surface); Organic Environment (Vegetal, Animal); Sociological Environment (adjacent tribes), Physical, Emotional, and Intellectual Characters. Then follow the tables, divided into Structural and Functional, each of which is subdivided into Operative and Regulative. The Structural Operative is again subdivided into Operative and Regulative; the Structural Regulative is subdivided into Political (*Civil*, [Domestic, (Marital, Filial), Public], *Military*), Ecclesiastical, and Ceremonial (*Mutilations*, *Funeral Rites*, *Laws of Intercourse*, *Habits and Customs*). Under Functional, the Regulative is subdivided into Sentiments (*Æsthetic*, *Moral*), Ideas (*Superstitions*, *Knowledge*), and Language; the Operative into Processes (*Distribution*, *Exchange*, *Production*, *Arts*, *Rearing*, &c.), and Products (*Land-Works*, *Habitations*, &c., *Food*, *Clothing*, *Implements*, *Weapons*, *Æsthetic Products*). Under each final subdivision ample details are given. The value of such a work to all students of sociology, and of mankind generally, will be inestimable.

SIR JOSIAH MASON, who has already built and endowed an orphanage at Erdington, near Birmingham, at a cost of more than a quarter of a million, has now arranged to erect and endow a Scientific College in Birmingham, for which will probably be expended at least an equal amount. The *Times* gives the following details:—During his long experience as a manufacturer, Mr. Mason became deeply convinced of the want of and necessity for "thorough systematic scientific instruction, specially adapted to the practical, mechanical, and artistic requirements" of the Midland district, and to this want he has determined to devote a portion of his remaining property to supply. The institution is to be called "Josiah Mason's College," or "Josiah Mason's College for the Study of Practical Science." Regular systematic instruction is to be given in mathematics, abstract and applied physics, both mathematical and experimental; chemistry, theoretical, practical and applied; the natural sciences, especially geology and mineralogy, with their application to mines and metallurgy; botany, and zoology, with special application to manufactures; and physiology, with special reference to the laws of health. The English, French, and German languages will also be taught. The trustees have power to include mechanics and architecture and all other subjects necessary to carry out the objects of the founder. Mere literary education and instruction are excluded, as well as all teaching of theology and subjects purely theological. No principal, professor, teacher, or other officer of the college is ever to be called upon to make any "declaration as to or submit to any test whatever of their religious or theological opinions," nor are these in any wise to be considered either as qualifications or disqualifications for holding any office, fitness to give the instruction required being the sole and only test. Provision is also made for giving lectures and opening classes for popular or unsystematic instruction, at which the attendance shall be open to all persons, "without distinction of age, class, creed, race, or sex." The founder's object being to promote the prosperity of the manufactures and industry of the country, especially of the two towns so frequently named, the college will be open to qualified persons of all classes who have to rely on science, art, or manufactures for a livelihood, "especially the more intelligent youth of the middle class." Provision is also made, when the funds permit it, to provide instruction, for females as well as males. The site selected for the college is in the centre of the town, and the land is therefore of the greatest value, and the generous founder has already laid out upwards of 20,000*l*. on the site. He has also conveyed landed property producing about 600*l*. a year, and there is a clause in the deed in which he states it to be his intention to devote by his will additional funds for the use of the college. The total amount of this noble endowment cannot, therefore, be positively stated, as it will, of course, depend upon circumstances. Enough, however, has already been done to render the "Josiah Mason College" one of the most princely gifts yet made to posterity in England by any of her wealthy sons.

The forthcoming number of Petermann's *Mittheilungen* will contain an interesting article compiled from the Australian papers, giving an account of a three months' journey during August, September, and October of last year into the interior of Australia, by Mr. Ernest Giles, accompanied by Messrs. Carmichael and Robinson. They struck off from the route of the overland telegraph at Chambers's Pillar, about 133° 55′ E. long., and 24° 53′ S. lat., and journeyed in a north-west direction along Finke Creek, traversing ground which has not hitherto been explored. They passed among long ranges of hills, lying in an east and west direction, and varying in height from a few hundreds to 4,000 ft., though few of the heights are apparently above 1,000 ft. At about the 24th parallel, in 133° N., they came upon multitudes of magnificent fan palms growing along the bed of the creek; they named the place the "Glen of Palms." Their journey in this direction extended to 129° 55′ W., and about 23° 10′ S., the utter sterility of the region and the want of water compelling them to turn back. It was only during the last few days, however, of their western journey that water became scarce. The most characteristic vegetation throughout was Spinifex; Casuarina was also of frequent occurrence. Travelling for about 100 miles in a southern direction, the explorers came upon an extensive salt marsh, apparently from Petermann's map upwards of 100 miles long and from 6 to 7 miles broad; Baron von Müller has named this Amadeus Lake. After staying here for a few days, Giles and his companions struck northwards for about 40 miles, and then south-eastwards, passing numerous creeks and a range of hills, "Gill Range," and meeting the Finke again on November 16, not far from their starting-point. Altogether these plucky explorers travelled 1,300 English miles, and have added considerably to our knowledge of the interior of Australia.

Under the name of "Herbarium Mycologicum Œconomicum," F. Baron Thumen proposes to form a collection of those parasitic fungi which are injurious (including, also, any that are useful) in forestry, agriculture, horticulture, or in any other branch of industry. The specimens of each species will be labelled with the scientific name, diagnosis, and any needful remarks, and, where possible, will be sufficiently numerous for a portion to be submitted to microscopic examination. The collection will be issued in fasciculi of fifty species, at the price of three thalers each, and may be obtained of the collector, at Teplitz, in Bohemia.

We regret to learn that Mr. Louis Fraser, at one time prominently connected with the Zoological Society of London, author of the "Zoologia Typica," and a professional taxidermist of high repute, is suffering from destitution, in his old age, in British Columbia. On April 7 last a communication was presented before the meeting of the Academy of Sciences of San Francisco on this subject by Mr. Henry Edwards, one of the members, and an appeal for assistance was made to the friends of science. This was answered by contributions on the part of several persons, but it is not stated to what extent.

The anniversary meeting of the Royal Geographical Society was held on Monday, Sir Henry Rawlinson in the chair. Sir Bartle Frere was elected President, and the Earl of Derby, Sir H. Rawlinson, Sir R. Alcock, and Admiral Richards, vice-presidents. The retiring president, in his valedictory address, reviewed at some length the progress of scientific exploration during the past year.

At the special request of Rear-Admiral Sands, the U.S Congress, at its last session, allowed an appropriation for the purpose of completing and publishing the catalogue of southern stars, observed by Gillis in 1850–52, and the work is now being put in the hands of computers for publication as soon as possible.

A slight shock of earthquake was felt on the morning of April 14, at Goalparah, Assam.

Additions to the Brighton Aquarium during the past week: Smooth Hound (*Mustelus vulgaris*); Skate (*Raja batis*); Gurnards (*Trigla lyra*); John Dorée (*Zeus faber*); Scad, or Horse Mackerel (*Trachurus trachurus*); Lump-fish (*Cyclopterus lumpus*); Turbot (*Rhombus maximus*); Common Carp (*Cyprinus carpio*); Gold and Silver ditto (*Carassius auratus*); Tench (*Tinca vulgaris*); Herrings (*Clupea harengus*); Sharp-nosed Eels (*Anguilla vulgaris*); Sand-launce (*Ammodytes lancea*); Gar-fish (*Belone vulgaris*); Zoophytes, *Actinoloba dianthus, Tubularia indivisa, Sertularia cupressina, Obelia geniculata, Pleurobrachia pileus.*

The additions to the Zoological Society's Gardens during the past week include two Cretan Ibexes (*Capra picta*), presented by Mr. T. B. Sandwith; a Macaque Monkey (*Macacus cynomolgus*); a Rhesus Monkey (*M. erithraeus*) from India, and a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, presented by Mr. H. N. Hewett; a dark-green Snake (*Zamenis atrovirens*). and a four-lined Snake (*Coluber quadrilineatus*) from Malta, presented by Mr. C. A. Wright; a pig-tailed Monkey (*Macacus nemestrinus*) from Java; a Malabar Parrakeet (*Palaeornis columboides*) from South India; an olive Weaver Bird (*Hyphantornis capensis*), from South Africa, purchased; a Brazilian Tapir (young) (*Tapirus terrestris*) from South America; a Harpy Eagle (*Thrasaetus harpyia*) from South America, deposited; four variegated Sheldrakes (*Tadorna variegata*), and four ruddy Sheldrakes (*T. rutila*) hatched in the Gardens.

SCIENTIFIC SERIALS

Poggendorff's Annalen der Physik und der Chemie. No. 3, 1873.—This number commences with a paper by Dr. Oudemans, jun., on the influence of optically inactive solvents on the rotatory power of optically active substances. The author, employing a Wild polaristrobometer and lime-light, experimented with cane-sugar, cinchonin, brucin, phlorizin, and other substances, with water, chloroform, alcohol, ether, &c., as solvents. He unexpectedly found that the specific rotatory power of cinchonin in various mixtures of alcohol and chloroform had not values entirely intermediate between those of cinchonin in either solvent separately (which are ↑ 212° and ↑ 228°). It rises to a maximum of over ↑ 237° in a mixture of 10 per cent. alcohol and 90 per cent. chloroform. He further compared the influence of different solvents on the specific rotatory power of active substances, with their solvent action, and he considers the greater values of the former property correspond with a greater solubility of the active substance. The numerical results are given in full.—Julius Thomsen continues his "Thermochemische Untersuchungen," examining, in this paper, the affinities of the constituents of water, of sulphuretted hydrogen, of ammonia, and of carburetted hydrogen. He finds that while there is development of heat in the formation of marsh gas, there is absorption in the formation of ethylene and acetylene, from carbon and hydrogen. The author gives a *résumé* of results from the series of researches here terminated (the affinity of hydrogen to the metalloids), which presents some points of considerable interest.—In the next paper Prof. Lubimoff of Moscow calls attention to an error current in most text-books on physics. The field of view in a Galilean telescope is stated to depend on the size of pupil of the observer's eye, and to be measurable by the angle under which this will appear from the centre of the object-glass. This, he says, gives a value five or six times smaller than the actual, which is directly dependent on the size of aperture of the object-glass. He explains and illustrates his new theory at some length.—F. Rudorff contributes the first part of a paper on the solubility of saline mixtures, and Ed. Ketteler continues his mathematical inquiry into the influence of astronomical motions on optical phenomena.—Among the extracted papers may be specified those by Edlund on galvanic resistance, by Braun on direct photography of the solar protuberances, and by Baumhauer on hygrometry in meteorological observatories.

Der Naturforscher for April 1873, contains a large amount of varied and interesting scientific matter. In Physics and Chemistry, there are short accounts of M. Jamin's researches on condensation of magnetism, Dr. Mayer's on measurement of sound, M. Cornu's new method of determining the velocity of light, Herr Feddersen's paper on thermo-diffusion of gases, Herr Nasse's on the nitrogen in albumenoids, Clerk-Maxwell's lecture on action at a distance, &c. Herr Nasse finds that, in the albumen-molecule, one portion of the nitrogen is combined loosely, another much more intimately, and he sets himself to determine the proportion of loosely-combined to the entire nitrogen-contents, in various albuminous substances. His observations have an important physiological bearing. In biology proper, we may note a paper giving the results of Herr Stohmann's recent study on animal nutrition. This author endeavours to formulate mathematically the digestibility of food stuffs. P. Secchi's recent communication on the solar protuberances and spots is given, and there is a meteorological paper on the temperature of air in woods and in the open, describing experiments by Herr Ebermayer. We may further call attention to a note on Baranetzky's experiments on the periodicity of outflow of sap in plants, a phenomenon he finds based on the periodical action of light. Geology, geography, technology, &c., are also represented in this serial, and the weekly "Kleinere Mittheilungen" furnish a number of well-selected scientific data.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, May 14.—Mr. Joseph Prestwich, F.R.S., vice-president, in the chair.—The following communications were read.—"On the genus *Palæocoryne*, Duncan and Jenkins, and its affinities," by Prof. P. Martin Duncan, F.R.S.—In this paper the author referred to certain minute fossils from the Carboniferous rocks of Scotland, described by himself and Mr. Jenkins in a paper read before the Royal Society, as belonging to the Hydroida, and most nearly resembling the recent genus *Bimeria*, Wright. He stated that numerous specimens since received threw some further light on the nature of these fossils, and showed especially that in all probability the base is not really cellular, but that the cellular appearance is produced by the growth of the real base of the polype over the cells of the *Fenestella* on which it grows.—"Notes on Structure in the Chalk of the Yorkshire Wolds," by Mr. J. R. Mortimer.—In this paper the author described a peculiar structure observable in chalk from Yorkshire and elsewhere, giving it a striated appearance. This structure had been ascribed by Dr. Mackie and others to slickensides. The author adduced reasons for doubting the mechanical origin of these striations, and argued that they are of an organic nature. He ascribed them to corals, and remarked that similar striæ occur in all limestone formations.—"On *Platysiagum sclerocephalum* and *Palæospinax priscus*, Egerton," by Sir P. de M. Grey-Egerton, Bart., M.P., F.R.S.—The two species of fossil Fishes noticed in this paper were described by the author in the 13th Decade of the Memoirs of the Geological Survey, published in 1872. They are both from the Lias of Lyme Regis He now described some new specimens which add to our knowledge of their characters. An example of *Platysiagum* shows the position of the dorsal fin, which is placed very far back, occupying a place opposite to the interval between the ventral and anal fins, and the form of the trunk, which is of nearly uniform depth from the occiput to the base of the dorsal fin. The structure of the dorsal fin was described in detail. The new specimen of *Palæospinax priscus* shows especially the position of the second dorsal spine, which is placed over the 50th vertebra, the first being on the 16th, the fish thus most nearly approaching the existing *Cestracion*, which it also resembles in its dentition. In other respects it seemed to be most clearly allied to *Acanthias*.—"On a new genus of Silurian Asteriadæ," by Mr. Thomas Wright, F.R.S.E.—The specimen described showed the outline of a small Starfish, with a large disc and short rays, in a slab of Wenlock Limestone from Dudley. The outline of the ten rays was described as marked out by the border of small triangular spines, the other plates of the disc and rays being absent. Each ray was terminated by a stemlike multiarticulate process as long as the ray, from towards the extremity of which spring slender lateral processes, giving it a tufted appearance. This Starfish, which is in the collection of Dr. Grindrod, is named by the author *Trichotaster plumiformis.*

Zoological Society, May 20.—Dr. E. Hamilton, vice-president, in the chair.—Lord Arthur Russell exhibited specimens of, and made remarks upon, the different varieties of the Carp (*Cyprinus carpio*) cultivated in the German fish-ponds.—Mr. Sclater offered some remarks upon the most interesting animals observed in the Gardens of some of the continental Zoological Societies which he had lately visited.—Dr. E. Hamilton read a note confirmatory of the extraordinary fecundity of the Chinese Water-Deer (*Hydropotes inermis*).—Mr. H. E. Dresser exhibited some rare birds from the Ural, amongst which were the Smew (*Mergus albellus*) in down, nestlings of the Rustic Bunting (*Emberiza rustica*) and several specimens of Lilljeborg's *Salicaria magnirostris*, which last he believed to be identical with *Acrocephalus dumetorum* of India.—Sir Victor Brooke, Bart., read a paper on the African Buffaloes, which he considered might be reduced into two species, *Bubalus caffer* and *Bubalus pumilus.* Of these the latter exhibited two varieties in the western and eastern points of its range, while the former appeared to extend from the Cape up the eastern coast to Abyssinia without any material variation.—Mr. St. George Mivart, F.R.S., read a memoir on *Lepilemur*, *Cheirogaleus*, and other lemurine forms, to which were appended remarks on the Zoological rank of the Lemuroidea in the natural system.—Messrs. Sclater and Salvin communicated a paper on some Venezuelan Birds collected by Mr. James M. Spence, amongst which were examples of two species believed to be new to science, and proposed to be called *Lochmias sororia* and *Crypturus cerviniventris.*—A communication was read from Mr. R. Swinhoe, on the White Stork of Japan, which he referred to a species different from the *Ciconia alba* of Europe, and proposed to call *C. boyciana.*—Mr. H. E. Dresser read some notes on certain oriental species of Eagles (*Aquila*).

Royal Horticultural Society, May 16.—General meeting,—Viscount Bury, M.P., president, in the chair.—The resignation of the Assistant-Secretary was announced.—The Rev. M. J. Berkeley, who was then called to the chair, commented on the plants of interest exhibited. He called attention to specimens of *Cytisus Adami*, believed to be a graft-hybrid, which bears upon the same branches, besides its own proper intermediate flowers, the dissociated very distinct flowers of its parents.—*Tillandsia ionantha* and a large flowering specimen of *Cycas revoluta* were also alluded to.

Scientific Committee.—Dr. J. D. Hooker, F.R.S., C.B., in the chair.—Mr. Anderson-Henry sent cuttings from black currant bushes, the buds of which were swollen to an unusual size, but abortive. This was due to the presence of a four-legged acaroid, similar to those on lime and hazel. In gardens near Greenock it was seriously affecting the cultivation of the fruit; it is believed there to have been imported with plants obtained from the Low Countries.—A letter from Mr. Andrew Murray to Mr. Berkeley was read, dated Salt Lake City. He sent an *Oscillatoria*, which he had found in a hot sulphuretted spring; also specimens of a *Nostoc*, with very large-celled chains, which blackened the stones in the brooks.—Dr. Masters called attention to a mode of propagating the vine described by M. Rivière. Cuttings were planted vertically in the ground in the spring, the uppermost bud being completely covered with 3 to 4 inches of soil.

EDINBURGH

Royal Society, May 19.—Memoir on the placentation of the sloths, by Prof. Turner. After referring to the absence of any definite information on this subject in anatomical literature, the author described his dissection of the gravid uterus of a specimen of that species of two-toed sloth, which Peters has named *Choloepus Hoffmanni*. His specimen was perfectly fresh when it came into his possession, and he had succeeded in obtaining satisfactory injections both of the foetal and maternal systems of blood-vessels. His dissections have led to the following conclusions:—The placenta of the sloth is not cotyledonary, in the sense in which the term is employed to express the non-deciduate placenta sub-divided into distinct and scattered masses, as in the ruminants. In the fullest sense of the word it is a deciduate placenta. If the inference which has been drawn from Sharpey's observations on the placenta in *Manis*, viz. that it is non-deciduate, be correct, then it is clear, if any value is to be attached to the placental system of classification, that the scaly ant-eaters can no long be regrouped along with the sloths in the order Edentata, which order must therefore be broken up. The memoir concluded with some remarks on the affinities, as regards their placental form and structure, of the sloths to the other deciduate mammals.

PARIS

Academy of Sciences, May 19.—M. de Quatrefages, president, in the chair.—The following papers were read.—A note on solar cyclones, with an answer, by S. Respighi to M. Vicaire and Father Secchi, by M. Faye. M. Vicaire in his late critique on M. Faye's solar spot theory had asked how that author could compare the barometric depressions in terrestrial cyclones which only amount to a few millimetres of mercury with the enormous lowerings of the chromosphere which ought to take place on the solar spots but which are inadmissible. M. Faye now replied that these depressions are *facts* long and carefully observed by Respighi, and quoted a letter from him on the subject. With regard to Secchi's assertion that Respighi had been deceived by the small size of his telescope (4¼ inches aperture) he pronounced the objection utterly invalid, for, whatever might be the shortcomings of the telescope as regards minute details, it could never make the chromosphere appear very low where it was in reality very high.—Note on the mechanical properties of different bronzes, by M. Tresca.—Hydrologic studies of the Seine Part II., Agricultural applications, by M. Belgrand.—On the part played by the substratum in the distribution of rock lichens, by M. Weddell.—New observations on metallic deposits on zinc, &c., and a new heliographic process, by M. C. Goudon.—On an electro-diapason of continuous movement, by M. E. Mercadier.—On an electro-dynamic experiment, by MM. G. Planté and Alf. Niaudet-Breguet.—On the action of dry ammonia gas on ammonic nitrate, by M. F. M. Raoult. The author found that the liquid produced by the action varies in composition with the temperature. At – 10° C., 100 grammes of the nitrate absorb 42·50 grammes of the gas; this gradually diminishes as the temperature rises until at + 29° 20·9 grm. only are retained and the product is solid, at 79° only 0·5 grm. of NH_3 remain.—On certain peculiarities observed in spectrum researches, by M. Lecoq de Boisbaudran.—On the preparation and properties of oxymalcic acid, by M. E. Bourgoin.—On the acid derivatives of naphthylamine, by M. D. Tommasi.—On the different propylenic chlorides. A classification of the absorption-bands of chlorophyll; accidental bands, by M. J. Chantard. The author so calls the bands produced by the action of acids, alkalies, or other re-agents upon normal chlorophyll.—Observations on the regulation of the magnetic compass, by M. Caspari.—Experimental Researches on the influence of barometric changes on life, tenth note, by M. P. Bert.—Mineralogical determinations of the true meteoric irons (Holosidères) in the Museum, by M. Stan. Meunier. During the meeting an election to the vacant seat of the late M. le Comte Jaubert (Academecien libre) took place. M. de la Gournerie obtained 44; M. Bréguet, 9; M. Sedillot, 5; M. Jacqmin, 2; and M. du Moncel, 1 vote. M. de la Gournerie was accordingly declared elected.

DIARY

THURSDAY, MAY 29.

ROYAL SOCIETY, at 8.30.—Croonian Lecture on Muscular Irritability after Systematic Death: Dr B. W. Richardson

SOCIETY OF ANTIQUARIES, at 8.30.—Ballot for election of Fellows.

ROYAL INSTITUTION, at 3.—Light: Prof. Tyndall.

FRIDAY, MAY 30.

ROYAL INSTITUTION, at [illegible].—On the Radiation of Heat from the Moon. The Earl of Rosse

HORTICULTURAL SOCIETY, at 3.—Lecture.

SATURDAY, MAY 31.

ROYAL INSTITUTION, at 3.—The Historical Method: John Morley.

GEOLOGISTS' ASSOCIATION.—Excursion to Finchley.

MONDAY, JUNE 2.

ENTOMOLOGICAL SOCIETY, at 7.

ROYAL INSTITUTION, at 2.—General Monthly Meeting.

TUESDAY, JUNE 3.

ANTHROPOLOGICAL INSTITUTE, at 8.—On a ready method of measuring the Cubic Capacity of Skulls: Prof. Busk, F.R.S.—Flint Implements from St. Vincent's: Prof. Rolleston, F.R.S.—Copy of a Mural Inscription in large Samaritan Characters from Gaza: Rev. D. I. Heath.—Strictures on Darwinism, Part II.: the Substitution of Types: H. H. Howorth.

ZOOLOGICAL SOCIETY, at 8.30.—The Antelopes of the genus *Gazella* and their Distribution: Sir Victor Brooke, Bart.—The Birds of the Philippine Islands: Viscount Walden

ROYAL INSTITUTION, at 3.—Roman Archæology: J. H. Parker.

WEDNESDAY, JUNE 4.

MICROSCOPICAL SOCIETY, at 8.

THURSDAY, JUNE 5.

CHEMICAL SOCIETY, at 8.—On the Dioxides of Calcium and Strontium: Sir John Courcy, Bart.—On Iodine Monochloride: J. B. Hannay.—A new Ozone Generator will be exhibited by Mr. T. Wills.

LINNEAN SOCIETY, at 8.

ROYAL INSTITUTION, at 3.—Light: Prof. Tyndall.

BOOKS RECEIVED

ENGLISH.—The Art of Grafting and Budding. C. Baltet (W. Robinson).—Elementary Crystallography. J. B. Jordan (T. Murby).—The Noaic Deluge: S. Lucas (Hodder and Stoughton).—British Rainfall, 1872. G. J. Symons (E. Stanford).—On Coal at home and abroad. J. R. Leifchild (Longmans).—The Olive and its Products: L. A. Bernays (J. C. Beal, Brisbane).—The Philosophy of Evolution (an Actonian Prize Essay): B. T. Lowne (Van Voorst).

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ERRATA.—P. 64, col. 1, line 13 from bottom, for "drift" read "draft." Col. 2, line 14 from top, for "unnecessary" read "necessary."

THURSDAY, JUNE 5, 1873

CONDENSED MILK

THE importance of milk as an article of diet is so great that anything offered as a substitute for it, or that renders it more available as food, demands attention. The composition of cow's milk is so nearly like woman's milk that the addition of a little water and sugar may be said to convert the one into the other; hence the practice of giving cow's milk to young children, and making it a substantial article of their diet long after they have cut their teeth and are able to masticate bread and meat. No inconsiderable quantity of milk is also consumed by adults, and its nutritive effect is not exceeded by any article of diet, as it contains all the constituents that are necessary to the perfect nutrition of the human body.

There are, however, several drawbacks in the use of cow's milk which diminish its utility, limit its use, and sometimes render it dangerous. One of the great drawbacks in milk is its liability to decomposition. The sugar it contains becomes acid, the caseine separates in the form of curd, and a fermentation ensues which renders it unpleasant and sometimes even dangerous as an article of diet. The latter effect is seen more particularly in young children. During the summer months they suffer extensively from diarrhœa, and there is little doubt that this is largely due to the acidity of the milk which is given to them. Milk bought in the morning in London is frequently unfit to be used in the evening for the diet of infants. These changes in milk are hastened by the present system of bringing milk to London from a distance in cans, by which means it is shaken, and its tendency to change hastened.

Another drawback in the use of milk is its liability to adulteration. Unfortunately the agent by which milk is adulterated, is easily accessible and can be detected with great difficulty. We cannot instruct cooks and poor people in the use of lactometers and hydrometers by which the learned test milk: moreover, the natural liability of milk to vary is very great. Thus the quantity of cream in milk received by the Aylesbury Condensed Milk Company varies from 9 to 17 per cent. Dr. Hassell states that the cream given by the milk of a cow, the milk of which he personally inspected, was but 4½ per cent. Although then all milk containing less than 9 per cent. of cream may be suspected of adulteration, yet it may happen that a milk containing but 4½ per cent. may be really not adulterated with water at all.

This varying quantity of cream also shows that even when milk is not adulterated it is liable to great variations in the quantity of cream which may be taken as the measure of its usefulness as an article of food.

Many attempts have been made to overcome these objections to the use of milk, and from time to time preparations of it have been sold by which freedom from acidity and adulteration are secured. The most available of these preparations have been those that submitted the milk to a process of evaporation by which more or less of the water naturally contained in milk is got rid of. By these processes the nutritive constituents of the milk are retained; the preparation keeps for some time, is easily conveyed from place to place, and by the addition of water milk, so to speak, is readily manufactured. None of these preparations, however, seemed to succeed till a process for making what is called "Condensed Milk" was introduced. Whether America or Europe has the honour of the invention we need not dispute here. It is now made in this country by thousands of gallons daily, and its manufacture may be witnessed on a large scale at Aylesbury.

Although the process of evaporating milk may be regarded as an exceedingly simple one, the attempt to carry it out at Aylesbury on a large scale has developed a complicated machinery in which steam power is extensively used; 200 persons are employed, and the milk of 1,200 cows, each yielding 14 quarts, is daily evaporated. The milk used is brought from farms in the neighbourhood in ordinary tin cans. Each can before it is sent to the factory is carefully tested by the taste and smell and the lactometer. Any doubtful specimens are set aside for re-examination or rejection. The milk is then passed into a vacuum pan, and the vapour thus produced is carried off and condensed and thrown away. When the milk has acquired a proper consistence it is mixed with sugar. This addition of sugar is the distinguishing feature of the condensed milk process. After this the milk is still further condensed till it reaches the required consistence, and is run off into the little tin cans which are so well known. The whole of these operations are carried out with a regard for cleanliness, which would look almost fastidious if it were not known that a single particle of decomposing milk allowed to get into the receiving pans might destroy the whole mass. Every can is returned thoroughly cleansed to the farmer who sends it, having been first submitted to hot water, then to a jet of steam, and then rinsed out by a jet of cold water.

The condensed milk thus prepared is of a semi-liquid consistence, and can be taken out of a jar with a spoon. Several analyses of this milk have been made. The late Baron Liebig found that it contained—

Water	22·44
Solids	77·56
	100·00

The *Lancet* has more recently published the following analysis:—

Moisture	25·10
Butter	11·73
Caseine	15·17
Milk sugar	16·24
Cane Sugar	29·46
Ash	2·30
	100·00

From these analyses it will at once be seen that the only perceptible difference between condensed milk and ordinary milk is that the former contains more sugar and less water than the latter. Both these things are necessary for attaining the objects for which condensed milk is manufactured. The diminution of the bulk of the water from 87 per cent. in ordinary milk to 25 per cent. in the condensed secures diminution of the bulk of the milk, and thus renders transportation comparatively easy. The condensed milk is easily converted to the condition of ordinary milk by the addition of either cold or hot water. The addition of the sugar is found to be necessary, in order to enable the other constituents to resist decomposition. Milk will keep any length of time when entirely desiccated, but by the process of drying entirely the milk loses its flavour and many of its properties. The semi-liquid condition of condensed milk prevents these changes, but in this state it is liable to decompose; hence the necessity of additional sugar.

The question arises as to whether this added sugar in any way interferes with the quality of the milk in its relation to the diet of infants or invalids. In comparing human milk with cows' milk, we find that the latter contains more caseine and less sugar than the former. Hence, when given to children it is customary to add a little water and a little sugar to make it like mother's milk. This object is really effected by the addition of cane sugar to the condensed milk, and it may therefore be unhesitatingly employed in the nursery as a substitute for ordinary cows' milk.

After a personal inspection of the Aylesbury manufactory, and a full consideration of the whole subject, we are quite prepared to say that where good fresh cows' milk is unattainable, as it is almost practically so in our large towns, there is no substitute for it equal to condensed milk. Nor is this a matter of theory; hundreds of gallons are being used every day in London, and most of it under the direction of experienced medical men. One medical man assures us that he has a healthy, fine-grown child of ten months that has never taken anything but condensed milk.

As the diet of invalids, it may in some cases require watching when the action of sugar is injurious to the system: but in these cases milk should be altogether interdicted.

It is to be hoped that no disadvantage in the use of this agent has been overlooked, as the advantages of its use are so many and so obvious. It presents a pure form of milk in a condition in which it may be kept for any length of time, and is not injured by removal. It is always at hand night and day, and by the addition of cold or hot water can be converted into nutritious and wholesome food.

E. LANKESTER

THE PHYSIOLOGY OF MAN

The Physiology of Man. By Austin Flint, Jun., M.D. Pp. 470. (New York: D. Appleton and Co., 1872.)

WE have already had to speak in terms of high commendation of Dr. Flint's comprehensive treatise on human physiology, as being written in a clear, methodical, and judicial style, the statements made being carefully weighed, and in most instances supported, by the best, if not the most numerous, authorities; whilst the author has in many parts enriched it with the results of his own important researches. The present, which constitutes the fourth volume of the work, is no exception to our remarks. It is occupied with the consideration of the nervous system, excluding the special senses, and gives a very complete account of that difficult and extensive section of physiology, the study of which has engaged the attention of so many of the best workers in all civilised countries during the past twenty years. Dr. Flint commences by a short *résumé* of the principal facts that have recently been made out in regard to the structure of the nerve-centres and cords, and the mode of termination of the nerves in muscle, gland, and skin; entering into the subject perhaps as far as is necessary in a strictly physiological work, the author taking Schultze's article in the recently published "Handbook of Histology" of Stricker, Kölliker, and Robin as his guides. The first chapter concludes with an account of the recent observations of Voit on the regeneration of the cerebral hemispheres after their ablation, which show that a large portion of these bodies may be reproduced, and that the organ may recover its functions to no very inconsiderable extent.

The second chapter deals with the general functions of the motor and sensory nerves, and gives a very fair account of the history of the discovery of the difference in the function of the anterior and the posterior roots, due prominence being given to the claims of Walker, Mayo, and especially of Majendie. In speaking of the recurrent sensibility of the anterior roots, Dr. Flint is not satisfied with Brown-Séquard's explanation that it results from the compression of sensory nerves distributed to the muscles during the spasm caused by the irritation of the anterior roots; but inclines to Majendie's and Bernard's opinion that there are actually recurrent sensory nerves in the anterior roots, on the ground that the pain is sometimes apparently severe when the cramps are slight. The relations of the nervous system to electricity, and the rapidity of nerve conduction, with the means of estimating it, are well and correctly given.

The cranial nerves are next considered. In this section we think the author fails in his account of the deep origin of each nerve. He does not appear to have heard of or seen the papers of Lockhart Clarke contained in the Philosophical Transactions (1858-67). Yet these contain by very far the most minute and the most accurate descriptions hitherto published on these points, and the importance of their relations to pathology would have fully justified more elaborate details. Thus, to take one point only, whilst speaking of the deep origin of the sensory root of the fifth pair of nerves, he makes no allusion to the very interesting facts described by Clarke of the internal connection of this root with the vagus and glossopharyngeal nerves in the grey tubercle, or caput cornu posterioris; of the connection of its motor root with the glossopharyngeal nucleus and the fibres of that nerve, and with the fasciculus teres; or, finally, of the connection of the sensory root with the nucleus of the third through the intermediation of the grey tubercle, into which the sensory root penetrates. On the other hand, his account of the functions of the various nerves and their branches is given extremely well; the account of the chorda tympani, for example, being excellent; and the conclusion at which Dr. Flint has arrived, namely, that it is a nerve of gustation, as well as a motor or stimulant nerve for the submaxillary gland, being fully borne out by Lussana's observations recently published in Brown-Séquard's journal, and which, at the time Dr. Flint wrote, had not appeared. A very long section commensurate with its importance is devoted to the pneumogastric nerves, the action of which on the heart, larynx, lungs, and stomach is given, with full reference to their remarkable inhibitory and depressing powers.

In the description of the anatomy of the spinal cord, Dr. Flint takes Gerlach's article in Stricker's Handbook as his guide, and gives the following as the results of his own experiments, and those of others which he regards as most reliable. "The gray substance is probably inexcitable and insensible under direct stimulus. The antero-lateral columns are insensible, but are excitable both on the surface and in their substance, *i.e.* direct stimulation will produce convulsive movements in certain muscles, which movements are not reflex and are not attended with pain. The lateral columns are less excitable than the anterior columns. The surface at least of the posterior columns is very sensitive, especially near the posterior roots of the nerves. The deep portions of the posterior columns are probably insensible, except very near the origin of the nerves." Dr. Flint then proceeds to describe the functions of the grey matter, and of the several columns of the white, explaining and adopting the views generally accepted. The posterior white columns he regards, with Todd, as containing fibres acting as commissures between the several segments of the cord.

The functions of the cerebrum are very briefly given, indeed, except in regard to *language* they are not given at all, and for a reason that scarcely appears satisfactory, viz. that though their consideration is properly a part of physiology, the range of the subject is so extensive, that it is only treated of exhaustively in special treatises on mental physiology. This is much to be regretted, as we feel sure that if Dr. Flint had attempted it, he would have succeeded in giving a very interesting section upon it. The cerebellum he regards as the co-ordinator of the muscular movements, and he has collected many pathological cases in support of his view. The last chapters are devoted to the sympathetic nerve and to sleep. The account of the sympathetic system enters freely into the consideration of the vaso-motor and trophic nerves. Upon the whole, this volume of Dr. Flint's work may be regarded as a valuable accession to physiological literature, and as giving the results of modern research with such fulness, combined with accuracy, that the ordinary student will not require to look beyond its pages for any information on this important subject of medical knowledge. We look forward with much interest to the next volume on the "Special Senses," which the author assures us is nearly ready.

CLODD'S "CHILDHOOD OF THE WORLD"

The Childhood of the World: a Simple Account of Man in Early Times. By Edward Clodd, F.R.A.S. (London: Macmillan and Co.)

THIS genial little volume is a child's book as to shortness, cheapness, and simplicity of style, though the author reasonably hopes that older people will use it as a source of information not popularly accessible elsewhere as to the life of Primitive Man and its relation to our own. In brief chapters he states the principal points of the modern science of civilisation, discussing the condition of Præhistoric savages, the early use of stone implements and the introduction of metals, the discovery of other useful arts, the evolution of language, the invention of writing, &c. Having laid down this as a foundation, he then proceeds to his main purpose, that of explaining the successive phases of man's belief, the working of inventive fancy in mythic legend, the rudimentary ideas of the lower races as to souls and their existence in a future state, the nature of deities, and the meaning of the worship offered to them by prayer and sacrifice. Examining the religions of the less cultivated races of the world, he passes through them to arrive at doctrines which, regarding them as highest and surest, he turns all his gift of earnest eloquence to teach. This book, if the time has come for the public to take to it, will have a certain effect in the world. It is not a mere compilation from the authors mentioned in the preface, but takes its own ground and stands by and for itself. Mr. Clodd has thought out his philosophy of life, and used his best skill to bring it into the range of a child's view. Why, indeed, should not children be taught their elementary philosophy of nature at the modern level? Why should they not begin to shape their lives by the best theory of the world, and their own place and duty in it, which their parents can accept? Thoughtful children will take in most of the facts Mr. Clodd works on, and his ideas will open many doors in their minds, leading into regions to be more fully explored years later. Much of the book, it is true, is beyond a child's unhelped understanding; not that the words are too hard, but that the ideas are. Its story is anything but "a tale of little meaning tho' the words be strong;" its simple language has often to convey thoughts too abstract for easy assimilation. Yet there is no harm in this, for the best children's books are those which in part engrave knowledge on their minds with finished accuracy, and in part only stamp roughly impressions which will take their sharper lines another time.

The world is growing daily more alive to the fact that the history of man and man's ideas, with all the problems of belief and duty which can be rightly treated on a historical basis, have been shifted into new places and altered into new forms by the modern sciences of the World and Man. At this present time there are numbers of parents and teachers to whose views such a modern "Religio Medici" as Mr. Clodd offers is congenial, and who distinctly want a book like his to teach out of. The need is all the more felt, because so many of the topics treated are among those where both theology and science put forward claims to speak with authority, while the adjustment of these claims has been mostly attempted by the class of writers who may be called "reconcilers." But educated people now distrust the method of these writers as vitiated by foregone conclusion, and it is more and more felt that the great problems of humanity must be dealt with by men who do not shape their evidence, but let their evidence shape them. Mr. Clodd, at any rate, is no "reconciler." It is evident that his religious feeling has come into real union with his positive knowledge, and that this act of mental chemistry has generated doctrines which are at once his theology and his philosophy. These doctrines, it is not the office of this journal to discuss: nor, considering how far Mr. Clodd adopts (of course with due acknowledgment) evidence and theories from the heavier volumes of technical ethnologists, my own included, would it be convenient for me to enter into detailed argument on his ethnology. I need only mention as points to which exception is likely to be taken, Mr. Clodd's easy passing over of the really serious difficulty, what became of the bones of the Drift-men and Cave-men, and his too confident expressions as to the first habitat of man, and the Origin of Languages. This said, what is left for me is simply to announce his work, helping to make it known to the class of readers who are waiting for it.

E. B. TYLOR

OUR BOOK SHELF

Notes on Natural Philosophy. By G. F. Rodwell, F.R.A.S., F.C.S., Lecturer on Natural Philosophy in Guy's Hospital and Science Master in Marlborough College. (London: J. and A. Churchill, 1873.)

THIS useful little work is an enlargement of Notes which the author had prepared for the students attending his lectures at Guy's Hospital. The title is perhaps a little too wide, as the book contains no reference to Sound and but a scanty treatment of Light, polarisation, for example, being not even mentioned. These omissions are explained in the preface as caused by the adaptation of the notes to the "Preliminary Scientific" Examination at the London University. We are quite sure, however, the author will agree with us that students for this examination will have to supplement their reading by some rather stiffer work than we find here. As an *introductory* text-book for this examination it is quite the best we have seen, the author having carefully avoided that atrocious system of giving candidates only just such knowledge as may help them to scrape through an examination. The evidence of conscientious labour which is conspicuous throughout the book makes us the more regret the incompleteness of these Notes. Even of the subjects treated it is obvious that in 160 pages, only the barest outlines of natural philosophy can be given. The "Notes" therefore chiefly consist of lucid and concise definitions, and everywhere bristle with the derivations of scientific terms. To this latter point the author has devoted much labour and thereby done good service to science; though on the other hand we cannot help thinking Mr. Rodwell runs a fair chance of being accused of pedantry by his frequent use of Latin quotations. One or two little points needing correction catch our eye. Fig. 18 is printed upside down; amidst all the derivations we do not see the meaning of the terms given to different thermometric scales; here as in some other books cobalt is erroneously stated to be attracted to a magnet even at the highest temperature. As this seems to be a frequent error we will give Faraday's own words on this matter: they are to be found on the very last page of his "Experimental Researches in Electricity." "By greater elevation (of temperature) nickel first loses its distinctive power at about 635° F., then iron at a moderate red heat, and cobalt at a far higher temperature than either, near the melting-point of copper." There cannot be a doubt that this little book will be of use to science teachers and science students.

Transactions of the Norfolk and Norwich Naturalists' Society, for 1872-73. (Norwich: 1873.)

THIS little volume contains some excellent papers. The president, Dr. Beverley, in his address, suggests, rightly, we think, that members of such societies ought, in their researches and papers, never to lose sight of the views and opinions usually associated with the name of Darwin, and very justly says that "the origin of species, the theory of evolution, and other Darwinian doctrines, cannot be proved or disproved by newspaper controversy or theological discussion." The first paper is by Mr. Howard Saunders, F.Z.S., on the Ornithology of Spain, which is followed by a short paper on *Vanessa Antiopa*, by Mr. C. G. Barrett. This is followed by a long, carefully compiled, and well illustrated list of the Fungi of Norfolk, by Mr. C. B. Plowright, M.R.C.S. The president, Dr. Beverley, also contributes a paper on the edible fungi of Norfolk, in which he draws attention to the great value of this much neglected source of nutritive food. There is an interesting paper on the Otter, by Mr. T. Southwell, F.Z.S. The two last papers are, one on the "Wild Birds' Protection Act," by Mr. H. Stevenson, F.Z.S., in which he points out the many obvious holes in the Act and adds a list of "wild birds," containing the most common provincial names by which they are known in England and Scotland; and Notes on the Mammalia of Norfolk, by Mr. T. Southwell. This society deserves the greatest credit for the important work its members are doing. They are making a praiseworthy, and so far a successful effort, to publish a fauna and flora of Norfolk. Already there have been prepared a list of the Mammalia and Reptilia, the Land, Freshwater, and Marine Shells, and, as we have above said, a list of the fungi. These will be followed by the Fishes, by Dr. Lowe; the Birds, by Mr. Stevenson (author of "The Birds of Norfolk);" the Flowering Plants and Ferns, by Mr. H. D. Geldart; Lepidoptera, by Mr. C. G. Barrett; all of which, we believe, are in hand, and will be published as the society finds funds to print them. Such a society deserves the greatest encouragement, and it is a pity that it should be hindered in its good work for want of funds. This ought not to be in a county like Norfolk, and we are sure that the intelligent inhabitants of that county only need to be made aware of the value of the work the society is doing, to come forward and lend it a helping hand. This they will best do by becoming members and taking as active an interest in the work as their circumstances permit. The society ought to take effectual means of making its aims and the value of its work be known throughout the county.

Birds of the Humber District. By John Cordeaux. (Van Voorst.)

MR. CORDEAUX is so well known as a careful and trustworthy observer of nature, that any work on his favourite subject, from his hand, must be read with interest. A residence of ten years in the district of which he writes, comprising North and Mid-Lincolnshire, and Holderness, has enabled him to gain a thorough familiarity with the times of appearance and departure of the birds which visit it. These points he has noted with great pains and precision, as is proved by the fact that he has been able clearly to trace the points of the district at which each of the migratory birds enter and depart, most doing so from the sea-coast, the grey wagtail, cuckoo, and common dotteril, being the only exceptions. The sections, of considerable length, devoted to the dates on which to expect the various wading birds, and the conditions of weather which cause these to vary, will be of great interest to sportsmen in the locality; the woodcock, snipe, and plover receive the fullest attention. Among the rare birds that are recorded as having been met with formerly, or of which one or more specimens have been shot lately, we find the cream-coloured courser, Macqueen's bustard (the only British example), Tengmalin's owl, and the tawny pipit. Most extraordinary of all is a jacamar in the collection of Canon Tristram, which was shot in 1849 by S. Fox, a gamekeeper, near Gainsborough; as the author remarks, "it must ever remain an ornithological puzzle how it could have reached this country." We recommend this excellent little work to all ornithologists and sportsmen.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Permanent Variation of Colour in Fish

A QUESTION of some interest is raised by a letter published by Mr. Saville Kent, in NATURE, vol. viii. p. 25. It is stated that a Plaice, now in the Brighton Aquarium, has "the posterior half of its under surface, usually white, coloured and spotted as brilliantly as the upper one; the line of demarcation between these two colours again, though sinuous, is most abrupt," and the writer proceeds to say that, on the Darwinian theory, this may be considered as a remarkable instance of reversion—"the Pleuronectidæ being derived from ancestors originally possessing bilateral symmetry, and an equal degree of coloration on each side."

First, as to the fact:—Examples of such colouring among the *Pleuronectidæ* are not very uncommon, and they occur most frequently in the Flounder (*Pleuronectes flesus*) and Plaice (*P. platessa*). Sometimes it is the upper surface which is thus affected—more or less of it being purely white. In a specimen now before me the colouring of the upper surface occurs upon the under one in numerous blotches of various sizes, and this mode of distribution is not uncommon. In every instance that I have heard of, the line or lines of demarcation, when they exist, are such as your correspondent describes, but, in extreme cases, no such line is present—the whole of one surface having uniformly assumed the colouring of the other. Such abnormal colouring may occur either upon the upper or lower surfaces; the fish in the former case being entirely white, and in the latter entirely brown.

The *rationale* offered by your correspondent, although engaging, is not unopen to criticism. For nothing can be more evident to Darwinists than that the colouring of the *Pleuronectidæ* has been acquired because of its protective adaptation to their peculiar form and habits. But it is difficult to see how such colouring could have conferred protection upon their free-swimming ancestors, so that, unless we make the highly anti-Darwinian supposition that the common progenitor was coloured in anticipation of the habits to be contracted by its offspring, there is only one hypothesis open to us, viz., that the unmodified progenitor adopted, through natural selection, the habit of lying on its side *because* of its original sandy colour. As this view, however, will be rejected by all who know how much easier colour is to modify than habit or structure, we are compelled to adopt the supposition, as being the most probable, that the coloration of the *Pleuronectidæ* is the result and not the cause of their form, and has, therefore, been acquired during the process of their flattening.

Although, however, we cannot, without gratuitous supposition, imagine that the unmodified ancestor of the group in question was coloured exactly like his progeny, there is still one other hypothesis by which atavism might be called in to explain such instances as that adduced by your correspondent. Whatever may have been the original cause of the flattening taking place, it is not likely that the initial variations (whether these were sudden and considerable, or gradual and slight), presented nearly so great a modification as that which we now observe. During these initial stages the partially modified individuals may have lain indifferently on either side, and so have acquired protective colouring on both. As the flattening, however, proceeded (from whatever cause), and the bones of the skull, etc., became more and more contorted, the new exigencies of the case might have caused the left side to be more and more used as a ventral surface, until its colouring, being of no further use, was allowed gradually to disappear. Upon this view the deviations from the normal colouring which now occur would be reversions, not to the bilaterally-symmetrical ancestors of the flat-fishes, but to their partially modified offspring. And, if this view were tenable, it might throw some light upon the otherwise inexplicable fact that some species of *Pleuronectidæ* are normally reversed—*i.e.* the left side instead of the right, constituting the upper surface—while in both kind of species individuals often occur which are reversed with reference to their specific type.

As however, this explanation is rather far-fetched, and, moreover, fails to account for the appearance of the partly white and the wholly white specimens above mentioned, it is best, I think, altogether to abandon the reversion theory.

Another, and, to my mind, a more probable one is open to us. Accepting the occurrence of abnormally reversed fish as an unexplained fact, we might, *à priori* expect that a cross between a normal and a reversed individual of the same species might present the appearance described in your correspondent's letter—the abrupt, though sinuous line of demarcation between the two colours, which always attends the occurrence of this variation, being precisely analogous to that which obtains in higher animals when piebald. Moreover, the abnormal coloration being of most frequent occurrence in the Flounder and Plaice—fish which are also the most frequently reversed—and the occasional appearance of the entirely white and entirely brown varieties, are just the facts we should anticipate were this explanation the correct one. Of course it may be objected that abnormal colouring is not of nearly so frequent occurrence as abnormal reversal, but when we remember how utterly ignorant we are regarding the causes which determine reversal in the *Pleuronectidæ*, and the blending or non-blending of colours in all animals when crossed, we should not lay too much stress upon this objection.

The truth or falsehood of this explanation would admit of easy experimental test on the part of the Brighton Aquarium authorities. Should they, however, undertake such, they must not rest satisfied with mere simple crosses, however numerous, but also try various complex and reciprocal ones. The piebald fish they possess should also be crossed with several normal and reversed Plaice. Should all their experiments prove unsuccessful, they would still be interesting as tending to throw us back upon the only remaining explanation, viz. that all these instances of abnormal coloration are independent sports, and so affording us by far the most striking of the many examples in the animal kingdom of the tendency towards bilateral symmetry which abnormal colouring frequently presents.

Dunskaith, Ross-shire, May 15 GEORGE J. ROMANES

Venomous Caterpillars

THE concluding words of Mr. H. S. Wilson's letter in your last number only reiterate the truth of a fact. Nearly all British entomologists who have collected *Lepidoptera* must have had painful experience of the irritation caused by the hairs of some one or other of our Bombyces that have very hairy larvæ. *Porthesia chrysorrhœa* is the greatest delinquent in this respect; and some years since I suffered intense agony after collecting the pupæ of this species. The hairs of the caterpillar are woven into the cocoon and the web surrounding it, and I recommend anyone in search of a counter-irritant to rub his face and neck with his hands after collecting these pupæ. The result, although painful, will be edifying and admonitory. The hairs have no effect upon the harder skin of the palm of the hand and fingers; and I believe (with most entomologists) that their action is purely mechanical, *i.e.* they pierce the tender skin in multitudes. A precisely similar, though less severe, effect is caused by the hairs of some Boraginaceous plants, *e.g. Echium vulgare*. On the Continent the extreme irritation caused by the hairs of *Cnethocampa processionea* is well known; and the introduction of a brood of these larvæ into a drawing-room would probably be followed by effects similar to those caused by the king's "great flea" in Faust.

At present I consider that the existence of caterpillars actually venomous (*i.e.* with a poison-gland at the base of each hair) requires confirmation. There are some pachydermatous individuals upon whom the hairs of Bombyces have little or no effect. I am unhappily not one of those, but my mental hide repels the insidious attacks of romancers in Natural History.

Lewisham, May 16 ROBERT McLACHLAN

BETWEEN the years 1857 and 1862 when stationed at Belize, the capital of British Honduras, I made the acquaintance of a so-called venomous caterpillar, which was held in very great dread by the natives, who averred that "its *bite* always produced fever."

Knowing their superstitious habits, and that, as far as my knowledge of natural history went, there did not exist a caterpillar capable of producing a wound of any kind by *biting*, I resolved to test the truth of the assertion. Accordingly, and to the intense horror of the bystanders, I took one in my hand from a tree that was literally covered with them. It was about 1¼ in. long, by ¼ in. thick, of a blue-grey tint, and in addition to the fine long hairs which clothed it, was armed with clusters of short spines. These clusters were formed into rows and contained about a dozen spines each. After a careful examination, I came to the conclusion that they were most likely to be the seat of the venomous propensities attributed to the insect, so I struck the back of my right hand against them two or three times to see what would be the effect. They were very brittle, and broke off as they entered the skin. I thought no more about it till about an hour had elapsed, when I experienced in the wrist a dead pain which gradually extended to the arm-pit, followed by a swelling of the glands.

For the whole day the pain was sufficient to render my arm useless; hence I thought that there must be some poisonous secretion in the spines, for the irritation caused by fine points, even if barbed, would scarcely produce such an effect. The pain died away in the evening, unattended by any feverish symptoms whatever, for I was in excellent health at the time. Next day I examined several of the spines under the microscope; they were not barbed, but hollow, and under pressure emitted a colourless transparent fluid, to which I attributed the poisonous qualities which caused me so much pain. A. M. FESTING

The Demagnetisation of Needles.

IT may not be generally known that magnetised needles, like those used in galvanometers and telegraphs, are easily and rapidly demagnetised in the neighbourhood of other magnets, when the fields of the two magnets are not coincident—that is, when their respective lines of force are not in the same direction.

A striking instance of this has just been brought to my notice. A tangent galvanometer used for taking daily readings of the escape of the current to earth upon wires, when they are disconnected at their terminal points, was found constantly and gradually to be losing its delicacy. This was traced to be due to the demagnetisation of the needle. The needle was re-hardened and even changed but with the same effect. The galvanometer was fixed near some Wheatstone's A B C instruments, which, being worked by magneto-electric currents, have powerful permanent magnets within them. The galvanometer was shifted to the other side of the office, when the effect entirely ceased.

Hence those who have delicate galvanometers should be careful to see that they are not kept in the field of permanent magnets, unless, as in the case of the mariner's compass, they are free to move in the direction of the lines of forces of the magnetic field in which they lie.

Southampton, May 20 W. H. PREECE

Microscopes—Information Wanted

I AM following up some investigations and experiments in which I require certain data, which, however, I cannot at present arrive at, not being in possession of sufficiently delicate and exact instrumental appliances. The information which I now desire to elicit from some more experienced observers than myself is of such importance as to be both useful and interesting to many of your readers, and I therefore crave your insertion of this communication. The information I require is all the more important as having a bearing upon many questions which are now attracting public attention, such as spontaneous generation, the initial stage and transitional forms of living organisms, also various researches in experimental physics, chemistry, &c. I desire to arrive at the following data:—

1. What is the estimated dimensions of most minute particles of matter which can be visible, under any circumstances or conditions, under the highest powers of the microscope? I leave out of consideration (under this head) the question whether such matter is living or dead, organic or inorganic, or in fact regardless of any of its properties whatever except its mere visibility as a minute portion of matter. Some observers speak of visible particles $\frac{1}{100000}$th and $\frac{1}{500000}$th of an inch diameter; this is surely near the limit.

2. What is the best or most accurate method of arriving at an estimate of the dimensions of such minute objects as are too small to admit of actual measurement by any of the appliances now in use? Every microscopist knows from experience that objects may be distinctly visible, not as a mere point, but having an appreciable diameter, and yet be too minute for actual measurement to any degree of accuracy.

3. Have the most recently constructed microscopic objectives, such as the $\frac{1}{50}$th or $\frac{1}{75}$th, any advantages over the $\frac{1}{16}$th or $\frac{1}{12}$th inch objectives in the determination of the data above referred to? and have immersion lenses any advantage in this respect? I find some difference of opinion on this point. Some microscopists consider that a really first-class $\frac{1}{12}$th with the use of deep eyepieces will enable us to see anything whatever which can be seen by any other objective of shorter focus. On the other hand, it is evident that a great number of the most experienced microscopists think otherwise; and from the very fact of their purchase of such expensive high powers, argue that such lenses are found to supply what other powers cannot accomplish.

It appears to me that there is too much of vague and indefinite assertion in regard to the comparative powers and qualities of microscopic objectives, and it is very desirable that some more definite results should be arrived at. With what precision and accuracy the results of astronomical observations are made! and taking into consideration that many of these results are obtained by different methods of observation, using different instruments, and by different observers, it is astonishing that the discrepancies and errors of observation are so small. It is generally admitted that the microscope is, to say the least, equally perfect, if not more so, than the telescope; and we should therefore expect a corresponding degree of accuracy in the results of microscopical observations. There are no doubt many who, like myself, have hitherto worked with only the medium and low powers, but wish to be possessed of the improved objectives of high power, but from want of sufficient information it is difficult to make a suitable choice. H. H.

Melbourne, Victoria, March 27

Arctic Exploration

THE story of the American Arctic Expedition under Mr. Hall is a wonderfully curious one; but are we justified, from what we have been told, in coming to the conclusion that the part of the crew of the *Polaris*, that has been rescued in so remarkable a manner, are "deserters?"

As far as I have understood the reports which have appeared in the papers, none of the rescued men have said they were deserters; and until we hear what those who remained on board the *Polaris* have to say, it appears to be unjust and reprehensible to bring so grave an accusation against men, possibly innocent.

Should it so happen that Mr Tyson and his companions are deserters, can we put faith in the correctness of any part of their story?

There is certainly some mistake about the disposal of the six boats of the ship. As far as I can make out, only four, or at most five, are accounted for, namely, two abandoned in Smith Sound, and the two on the ice with Mr. Tyson, one of which was burnt for fuel, and the other, that in which they were when rescued, and which was taken on board the *Tigris*.

May 31 JOHN RAE

The Westerly Progress of Cities

IN his work on the Atmosphere, M. Flammarion draws attention to a peculiarity in the habits of our large towns which everyone must have noticed. "The wealthy classes have a pronounced tendency to emigrate westward, leaving the eastern districts for the labouring populations. This remark applies not only to Paris, but to most great cities—London, Vienna, Berlin, St. Petersburg, Turin, Liége, Toulouse, Montpellier, Caen, and even Pompeii."

Having frequently remarked this "westing" in many English towns, I have lately written to several friends, asking for definite information on this point, concerning the town in which they are resident. With scarcely an exception the reply of each showed, to alter Bishop Berkeley's line a little, that:—"Westward the course of fashion takes its way." This is true, I believe, of Edinburgh, Dublin in former years at any rate, Glasgow, Birmingham, Leeds, Southampton, Bristol, and Liverpool and Manchester to some extent. No doubt many of your readers can very largely extend this list; it would be interesting to collect wide information on this question. For supposing it established as a general fact, what an excellent speculation to buy up land in the west of a rapidly growing town like Leicester or Bradford! Perhaps it is common to do so already.

Whence arises this tendency? It can hardly be an accident, nor can it be due to the direction of the river beside which the town may happen to be built, for in the towns named, many of

the streams, where they exist, run in different directions. M. Flammarion thinks the westward movement is caused by the direction of sunset, towards which people feel disposed to form their gardens, build their houses, and in that direction most inclined to walk; the evening and not the morning being their usual time of recreation. Is not a more probable explanation to be found in the general dislike of an easterly wind? And, moreover, it has been pointed out that a westerly wind usually causes the greatest fall in the barometer, and thus the eastern portion of a town becomes inundated with the effluvia which arises on such occasions. Another and perhaps more potent cause may be the prevalence in Europe of south-westerly winds during the greater part of the year, whereby the smoke and vitiated air of a town are carried to the north-east more frequently than elsewhere; so that it is notorious the west end of a city is freer from smoke than the east end. Possibly all these causes may combine to produce this curious occidental march of the fashionable quarter.
W. F. BARRETT

Etymology of Aphis

WITH regard to the etymology of Aphis, I find the following in Lennis' "Synopsis der Natur-geschichte des Thier-reichs," p. 578:—

"Aphis, Blattlaus, nach Fabricius von ἀφίστημι trennen, abstehen; richtiger vielleicht ἄφυσσα von ἀφύω schopfen; musste dann aber Aphys heissen"

The second explanation is ingenious; but neither seems to my mind satisfactory.
W. W. SPICER

Itchen Abbas Rectory, Alresford, May 14

Phosphorescence in Wood

ONE wet evening last autumn some pieces of phosphorescent wood were brought to me, which had formed part of a dead beech-tree that had been cut down during the day. They shone brightly that evening. The next night they were dark until dipped in water, when the light revived but was much fainter than before. On the third night they seemed to have lost the phosphorescence entirely, for water produced no visible effect on them.

Your correspondent, Mr. W. G. Smith, states that the luminosity of decaying wood is due to the presence of various kinds of fungus, but does not say what is the cause of it either in fungi or glow-worms. There is something so striking in the light unaccompanied by sensible heat, that an unlearned person's curiosity is roused to know whether phosphorescence is akin to burning or not. Where can one learn what is known about it?
C. A. M.

Tears and Care of Monkeys for their Dead

WE have heard much of late about the emotions of animals, and might have heard it sooner had Charles Bell's profound work on the "Anatomy and Expression," received due attention. The moral or psychical emotions of the brutes most resembling man in structure are peculiarly interesting, and sufficient observations as to this point on the monkeys seem to be yet wanting. Before I saw a picture of a weeping monkey, by Edwin Landseer, I always thought that this animal could be moved neither to tears nor laughter; and I still think that more observations, by persons most familiar with monkeys, are required on this subject, and hope to elicit them by this note in NATURE. But an affectionate care of brutes for their dead has been considered either very rare or inexistent, though it would seem to have been shown by monkeys. At least, we have evidence to this effect in the "Oriental Memoirs," 4 vols. 4to, London, 1813, by James Forbes, F.R.S., and indeed, very likely, there may be still better observations, with which I am unacquainted, on the subject. Here is an extract thereon from Mr. Forbes's book:—

"One of a shooting party, under a banian tree, killed a female monkey and carried it to his tent, which was soon surrounded by forty or fifty of the tribe, who made a great noise and seemed disposed to attack their aggressor. They retreated when he presented his fowling-piece, the dreadful effect of which they had witnessed and appeared perfectly to understand. The head of the troop, however, stood his ground, chattering furiously; the sportsman, who perhaps felt some little degree of compunction for having killed one of the family, did not like to fire at the creature, and nothing short of firing would suffice to drive him off. At length he came to the door of the tent, and finding threats of no avail, began a lamentable moaning, and by the most expressive gesture seemed to beg for the dead body. It was given him; he took it sorrowfully in his arms, and bore it away to his expecting companions: they who were witnesses of this extraordinary scene, resolved never again to fire at one of the monkey race."
GEORGE GULLIVER

Canterbury, May 24

RECENT WORKS ON ECHINODERMS

AMONG the most important of recent works on Echinoderms may be mentioned "The Revision of the Echini," by Alex. Agassiz. Of this work, which will be completed in four parts, Parts 1 and 2 were published early in this year, Part 3 is going through the press and may possibly be published in August next; it will contain the description of species not included in Part 2. Part 4 may be published this year; it will contain a review of the anatomy and classification of the order. This part will not be so well illustrated as the author had intended, for six plates of anatomy, the results of many years' labour, with all Mr. Agassiz's drawings, were lost in the great conflagration of November 9, and it will be impossible to supply their places. The present parts are accompanied by an atlas of forty-nine plates. Part 1 contains, in addition to an introductory chapter, the bibliography of the subject, a chapter on Nomenclature, a Chronological List of Names used from 1554, a Synonymic Index, and a chapter on Geographical Distribution. Part 2 contains Description of the Echini of the Eastern Coast of the United States, together with a report on the deep sea Echini collected in the Straits of Florida, by Count Pourtalès, Assistant United States' Coast Survey in the years 1867—1869.

The synonymic index will be simply invaluable to the investigator of the Echini. He who investigates the life-history of a species must surely know the name of the species he is investigating. It is therefore, even from this point of view, by no means an unimportant task to unravel the complicated and tangled network of synonyms; themselves an evidence of lack of knowledge on the part of many. Agassiz regards—and very correctly so—synonymy as the *History of the Species*, not its natural history. His opportunities for examining the types of those authors who have written on the subject were immense, and he has thoroughly availed himself of them. The great Museums of London, Paris, Copenhagen, Vienna, Stockholm, and elsewhere, were all visited by Agassiz; while the original specimens described by Klein, Gray, Desor, Michelin, and others, were most carefully examined, and it must not be forgotten that in addition the Harvard College Museum contains one of the most perfect collections of Echini in the world.

It would serve no useful purpose if in this place we examined in any detail the catalogue of species of Echini given on pp. 88, 203 of this memoir; for convenience of reference the genera and the species in their respective genera are arranged alphabetically, but there is added a list of all known species arranged in their natural order, with the name adopted by Agassiz, the original name and the principal localities.

In treating of the geographical distribution of the Echini, Agassiz remarks that it was a matter of great surprise to him to find how few species, hitherto not noticed, were to be found in the European collections. Everywhere, although from different localities, were found repetitions of species already well known—so that in making a map of the littoral regions, but short stretches of shore were left out as unexplored. Though therefore new species may and will undoubtedly turn up, even in well explored localities, we probably have even now a very fair representation of the littoral Echini of the world. It would of course be rash to make any predictions as to the number of new forms that will doubtless be brought to light by the researches of Wyville Thomson—but these will probably be deep-sea forms. Did space allow we would gladly have dwelt longer on this most interesting portion of Agassiz's memoir.

The total number of genera adopted is 90, with 207 species. The atlas accompanying these parts contains 49 plates—the first seven are devoted to charts, representing the distribution of the Echini throughout the old and new worlds, and the remaining portion to figures of some of the new or little known species. Some of the plates are photographs—and very excellent ones - others are photo-printed by the albert type process, and while these have scarcely the brilliancy or evenness of detail as such engravings as those of Echini in the expedition to Egypt, yet when the enormous difference in cost is taken into account, these photo-printed plates must be a subject of congratulation to the working and not over-rich naturalist. Some others of the plates are lithographed from Agassiz's drawings, and these we would select as being the most useful in this atlas.

Next we would mention a very important paper by Prof. Lovén, published in "Öfversigt af Kongl. Vetenskaps-Akademiens Förhandlingar," 1871, No. 8. This paper was read on June 14, 1871, but was not, we think, published until the summer of 1872, and as a translation of it in full by Mr. Dallas has been published in the "Annals and Magazine of Natural History," vol. x., 4th series, October to December 1872, we will but very briefly allude to it here. Prof. Lovén describes some very small spheroidal button-like bodies furnished with a short stalk, which is normally attached to a small, slightly projecting tubercle, which he calls *Sphæridia*; these occur apparently in all Echinoidea except *Cidaris*; they are fully described as they occur in the different families. Lovén next describes the order which prevails in the disposition of the ambulacral plates throughout the whole class, for which he even gives a formula.

Passing from the sea urchins to the Brittle stars, we have also, from the Proceedings of the Royal Academy of Stockholm, a paper by Ljungman describing the collection of Ophiuroids made by Dr. Goes in the West Indies, in the Josephine Expedition. Fifty-seven species are enumerated, of which fifteen are described as new. Many of these latter were dredged from very considerable depths. The author adds to his paper a conspectus of the genera of Ophiodermatidæ and a conspectus of the Atlantic species of the genera *Amphiura* and *Amphipholis*.

Lutken, in an important memoir published in the Proceedings of the Royal Academy of Copenhagen, Part 2, 1872, entitled "Ophiuridarum novarum vel minus cognitarum descriptiones nonnullæ," describes a number of new species from different parts of the world, as well as gives some details of little known species. To this memoir there is appended a chapter "On Spontaneous Division in the Star Fishes," at the conclusion of which the author sums up with the following general propositions :—(1), The most energetic manifestations of the faculty of regeneration in animals is the power of divisibility ; (2), In certain forms of Radiates, in which the faculty of regeneration is very highly developed, spontaneous division takes place alone, as in Ophiuroids and Asteroids, or together with gemmation, as in Actinia ; (3), Actual spontaneous division or "Schizogony," in the Actinia, Medusa, Asteroids, and Ophiuroids (which must not be confounded with the disguised forms of gemmation met with in Infusoria and certain Chetopods) may be regarded as a peculiar form of Agamic reproduction such as Blastogony, Sporogony, and Parthenogony.

Lastly we have to mention the appearance of a modest catalogue of Echinodermata of New Zealand, with diagnosis of the species, by Capt. F. W. Hutton, F.G.S., Assistant Geologist, Colonial Department. In it thirty-four species are described, eighteen of them being described as probably new to science.

E. PERCEVAL WRIGHT

ON THE SPECTROSCOPE AND ITS APPLICATIONS

X.

I HAVE not yet done with the spot-spectrum referred to in last article. Not only is there general absorption, but there are indications of increased selective absorption in the case of the line D, as I could also show if I were dealing with the iron lines, the magnesium lines, or the other well-known lines of the solar spectrum. Not only, then, have we a general absorption, increasing as the middle of the sun-spot is approached, but this sodium line D is also thickened, so that we have, as a result of a single examination of a single sunspot, the fact that a sunspot is due to general absorption, *plus* special absorption in some particular lines.

Now, in what I said some time since on the radiation of hydrogen, I pointed out to you that the F line of hydrogen was different from the C line—in fact, I showed that it widened out towards the sun—and I also told you that Dr. Frankland and myself have asserted that that widening out is due to pressure, and we have been able artificially to widen out this F line of hydrogen by increasing the pressure. Now it struck us that possibly we might find some connection between that widening out of the F line of hydrogen and the widening out of the sodium line in the spot which I have just shown you. There is an experiment by which it is perfectly easy for us to reproduce this artificially, so that you see we can begin at the very outside of the sun by means of hydrogen, and see the widening of the hydrogen lines as the sun is approached ; and then we can take the very sun itself to pieces, and, by examining the pieces, see that the sodium lines vary in thickness in different parts of the spot, as the hydrogen does outside the spot region altogether : in fact, the pressure is continually increasing down in the spot exactly in the same way as it increases in the hydrogen envelope towards the sun.

If we take a tube containing some metallic sodium sealed up in hydrogen, and pass a beam of light from the electric lamp through it, by decomposing this beam with our prisms we shall obtain an ordinary continuous spectrum without either bright or dark lines, but by heating the metallic sodium in the tube which is placed in front of the slit, we really fill that tube with the vapour of sodium ; and as the heating will be slow, the sodium vapour will rise very gently from the metal at the bottom, so that we shall get layers of different densities of sodium vapour filling the tube. Immediately the sodium begins to rise in vapour, a black absorption line shows itself in our spectrum in precisely the same position as the yellow line of sodium, and you will find that the thickness of the sodium absorption line will vary with the density of the stratum of vapour through which the light passes. Thus from the upper part of the tube we obtain a fine delicate line, which gradually thickens as we approach the bottom ; and thus we reproduce the appearance in the spectrum of the spot where the layers of sodium vapour are very dense, and the very fine delicate line of the sodium vapour when thrown up into the sun's chromosphere.

We must next speak of what happens in the case of the magnesium lines. A very obvious magnesium line is lettered *b* in the solar spectrum. It is a triple line, separated by different intervals. There is a very important fact connected with these lines, which appear when magnesium vapour is thrown up into the envelope which I have called the Chromosphere. By means of the new method of research, it is quite possible to see, as I explained to you on a former occasion, what passes, which the eye could not possibly see. For instance, it is quite possible, by means of the spectroscope, to detect the existence of magnesium vapour outside the sun, although you know that, except during eclipses, we are never able to see these vapours. What I wish to call your attention to in the present case is this. We have there the three magnesium lines, and two of them are much thicker than the remaining one: and these two lines travel very much higher into the outside region than does the third one. Now, you will see in a moment that that indicates to us a fact something like this,—that the spectrum of magnesium, such as is generally at work, which cuts out these very black absorption lines in the solar spectrum, while the sodium gives us the yellow line D, is really a thing which is competent to give us three lines. This vapour, I say, is a thing, generally speaking, competent to give us three lines in this position; but if it so happens that when the magnesium is thrown up to a particular height we simply get two lines, the third stopping short, I think you will see that there is some force in one's reasoning, when one suggests that possibly in those regions where we find the hydrogen F line thin instead of thick, as I have shown it to you, and where the magnesium lines become reduced to two instead of three, the spectrum of magnesium vapour, like the spectrum of hydrogen, becomes very much more simple by *the reduction of pressure*, and therefore, that we should be able artificially, as in the case of hydrogen, and as in the case of sodium, to reproduce this result. In fact, it is perfectly easy to reproduce it, for we find by reducing the pressure of magnesium vapour we really can reduce that triple line of magnesium to a double one; so that, you see, we have three distinct lines of research, all leading us to the fact that where Kirchhoff placed an immensely dense atmosphere around a liquid sun, we really have vapour of considerable tenuity, by no means so dense as he supposed.

There is another point of very great interest which I should bring before you.

Mr. Huggins, who has done so much in his researches on stars, told us some few years ago that the spectrum of that wonderful variable star τ Coronæ, which had been just discovered, indicated that, over and above the light which we got from the star generally, we get evidence of incandescent hydrogen in the spectrum, so that the spectrum was a thing such as had never been seen before; for we got, in addition to the ordinary evidence of absorption visible in the spectrum of a star, as in the spectrum of the sun, indications also of selective radiation. There are indications of bright lines superposed above the others. Now, let me tell you—and this is a very important part of the question—that by observing the various changes that take place in our central luminary, it is quite possible to see on the sun almost any day evidence of its being violently agitated; that there are certain regions of the sun which appear exactly as that variable star did—that is to say, in addition to the ordinary absorption lines visible in the solar spectrum, the spectrum of these regions indicates to us that the hydrogen, instead of being black, instead of reversing the spectrum, as you have seen it in these spectra that I have shown you, really is bright, or else the hydrogen lines cease to be visible altogether, as in α Orionis.

I have to give you, as the last application of spectrum analysis, the power which the prism gives us of investigating, so to speak, the meteorology of the sun, the velocity with which the different stars are moving through space, and the velocity with which the storms are travelling over the face of our central luminary. Many of you know, no doubt, that Mr. Huggins, in his observations of the spectrum of the star Sirius, saw that the hydrogen lines were much developed; and in a further examination, carried on by the method in which the spectrum of hydrogen and other vapours which he wished to examine were absolutely visible in the field of view at the same time as was the spectrum of the star, Mr. Huggins was astonished to find that the hydrogen lines no longer occupied their usual positions, but that they were all jerked, so to speak, a little to the side of the place which they occupied in the spectrum of the hydrogen which he rendered incandescent in his tubes. The F line of hydrogen which he observed in the spectrum of Sirius he found did not exactly occupy the same position in the spectrum as did the actual F line of hydrogen, the incandescent hydrogen with which he compared it (Fig. 53). Owing to a physical law, which I have not time to explain to you now, it is perfectly easy, by means of the prism, to determine the velocity with which the light-source is moving to or from us; and therefore, if this holds good for absorption, we could determine the velocity with which any absorbing medium is rushing to or receding from us. In the case of Sirius, for instance, Mr. Huggins determined that the velocity of the star in a direction from the eye, the measure of recession, was something like twenty miles a second. I am sorry I have not time to fully explain this very beautiful adaptation of the spectroscope, but I may say that the position of a line, bright or dark, in the spectrum depends upon its wave-length—that is to say, the length of the wave of light which produces that colour. Thus, the length of a wave of red light is about $\frac{1}{39000}$ of an inch, and that of a wave of violet light is about $\frac{1}{57500}$ of an inch. I think when I mention that, you will see at once the possibility of determining any alteration of velocity—for an alteration of wave velocity we have, or appear to have, whether we move towards an object, or whether an object moves towards us, just in the same way as in the case of sound, and in the case of a wave reaching the shore. Suppose yourself a swimmer carried on a wave; if you are going with the wave it seems long, but if you attempt to swim against it it seems short. So with all these waves, beating from all these orbs peopling the depths of space on to the earth. If by the motion of those bodies or by our own motion, the waves are crushed together, we get an alteration in the light, which the prism alone is able to determine. If the luminous object is approaching the eye rapidly, the vibrations causing light will, of course, fall on the eye more frequently in the same time than if the bodies were at rest—or, in other words, the waves will be shortened; then the position of the dark or bright lines, as the case may be, will be shifted in the direction of the most refrangible rays—that is to say, towards the violet; whilst if the bodies are separating, the shifting will take place in the direction of the red or least refrangible rays. In the case of Sirius, the star was receding from us, and we got longer waves, and the lines are nearer the red end of the spectrum to such an extent as to leave unaccounted for a motion of recession from our sun amounting to something between 18 and 22 miles per second Other stars, such as Betelgeux, Rigel, Castor, Regulus, and many of the stars in Ursa Major, are found to be moving away from the sun. Some, however, move rapidly towards us. Arcturus approaches us with a velocity of 55 miles per second; Vega and α Cygni, Pollux and α Ursa Majoris, also approach the sun with a velocity varying from 40 to 60 miles per second. If now we take a spot-spectrum (Fig. 54), in which, instead of the sodium line D, we have the F line of hydrogen, this strange crookedness which you notice is really a crookedness due to the fact that in one place we have incandescent hydrogen rising up with tremendous velocity, and in another we have it rushing down cool with tremendous velocity; again, we have hydrogen in a different condition altogether. We know that in this case we have a variation of velocity, because we get distinct changes in one direction or the other, and we get changes in both directions. We can determine by the amount of crookedness of the hydrogen, whether bright or dark, how far it is driven from its normal condition, and then how fast per second the hydrogen is travelling. In one case the velocity was something like 38 miles a second; in other words, we had heated hydrogen coming up at the rate of something like 38 miles a second, and cool hydrogen rushing down at something like an equivalent rate. Now, we are not only enabled, by a practical application of the prism, to determine these up and down rushes on the sun, by which we are enabled to learn much of its physical constitution, but also the rate at which storms travel over the sun—what we should call winds. The way that has been done will be perfectly clear on an inspection of the engraving (Fig. 55). It may appear strange to you that we should be able to observe a cyclone on the sun, but I hope to be able to prove to you that this is really a cyclone. Here is a spectrum of the region of the sun near the limb, and here is the hydrogen line. It is clear, if what I have said is true, that the incandescent hydrogen is there receding from us because the line inclines to the red. It is evident also, that in this case, when we get the line widened out towards the violet, it is coming towards us; therefore we have the thing travelling in both directions. It is obvious to you, I think, that if the slit enabled us to take in the whole cyclone, we should get an indication of motion in two directions; we should have the line diverted both towards the violet part of the spectrum, in the case of the hydrogen rushing towards us, and towards the red in the case of the hydrogen rushing away from us in this circular storm, and the extreme velocity will be determined by the extreme limit to which the hydrogen line extends. In this case, the storm was moving with a velocity of something like 100 miles a second, which, I dare say, strikes you as something terrible; but if you compare the size of the sun with that of the earth, I think you will see it was nothing very wonderful after all.

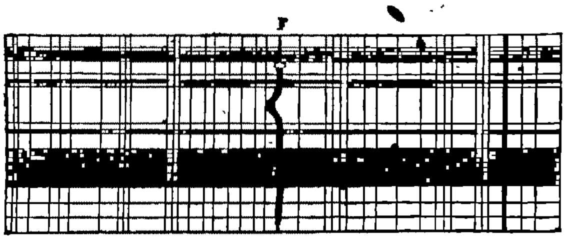

FIG. 54 – Deviation of the F line in a spot-spectrum.

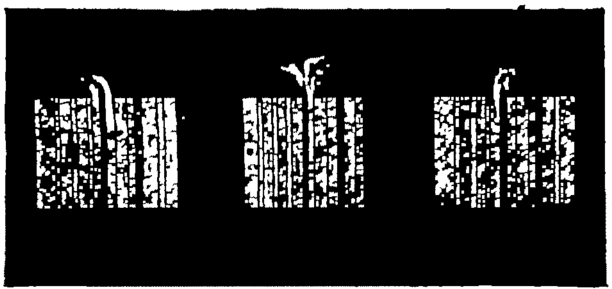

FIG. 55.—Shifting of the F line in a solar cyclone.

In further evidence of the truth of this, the last application of the spectroscope, I will show you two pictures of solar prominences 27,000 miles high, drawn at an interval of ten minutes. Here you see, first, the prominence as it appeared at a particular time on a particular day in March 1869 (Fig. 56). I wish to call your attention to the left-hand portion of the prominence, which you see is pretty straight. In ten minutes afterwards the whole thing changed, and, as you see by the next picture (Fig. 57), the nearly straight portion is quite gone. That will give you some idea of the indications which the spectroscope reveals to us of the enormous forces at work in the sun, merely as representing the stars, for everything we have to say about the sun, the prism tells us—and it was the first to tell us—we must assume to be said about the stars. I have little doubt that, as time rolls on, the spectroscope will become, in fact, almost the pocket companion of every one amongst us; and it is utterly impossible to foresee what depths of space will not in time be gauged and completely investigated by this new method of research.

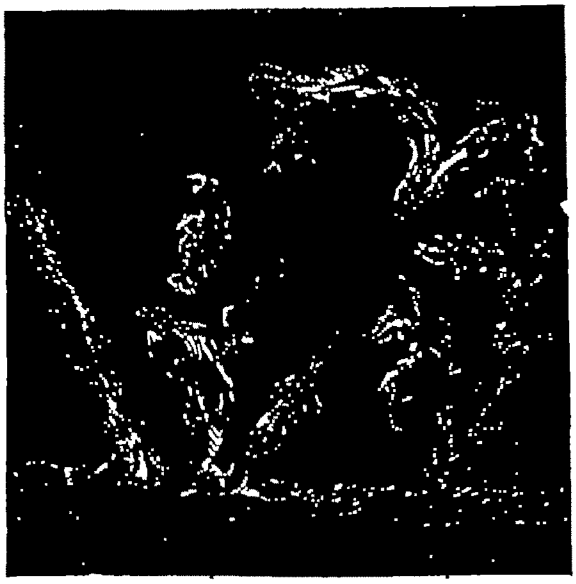

FIG. 56.—Prominence observed March 14, 1869, 11h 5m.

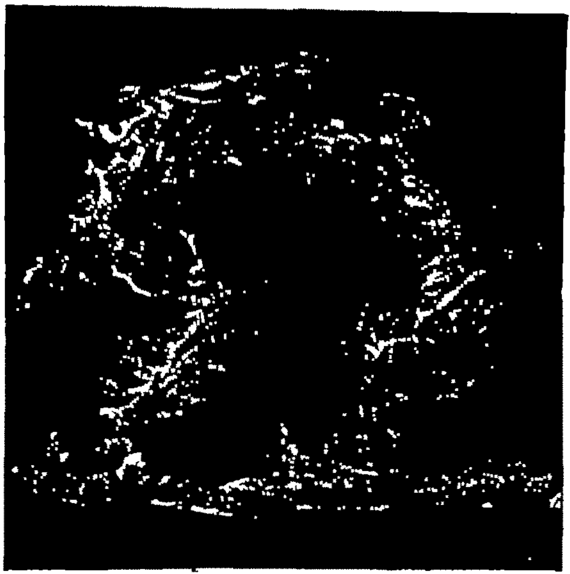

FIG. 57.—The same prominence, 11h. 15m.

J. NORMAN LOCKYER

ON THE ORIGIN AND METAMORPHOSES OF INSECTS[*]

V.

THE development of the beautiful *Comatula rosacea* (Fig. 41) has been described in the "Philosophical Transactions,"[†] by Prof. Wyville Thomson. The larva quits the egg, as shown in Fig. 42, in the form of an oval body about $\frac{1}{30}$ inch in length, something like a small barrel, surrounded by four bands or hoops of long vibratile hairs or ciliæ. There is also a still longer tuft of hairs at the narrower posterior end of the body. Gradually a number of minute calcareous spines and plates make their appearance (Fig. 43) in the body of this larva, and at length arrange themselves in a definite order, so as to form a bent calcareous club or rod with an enlarged head.

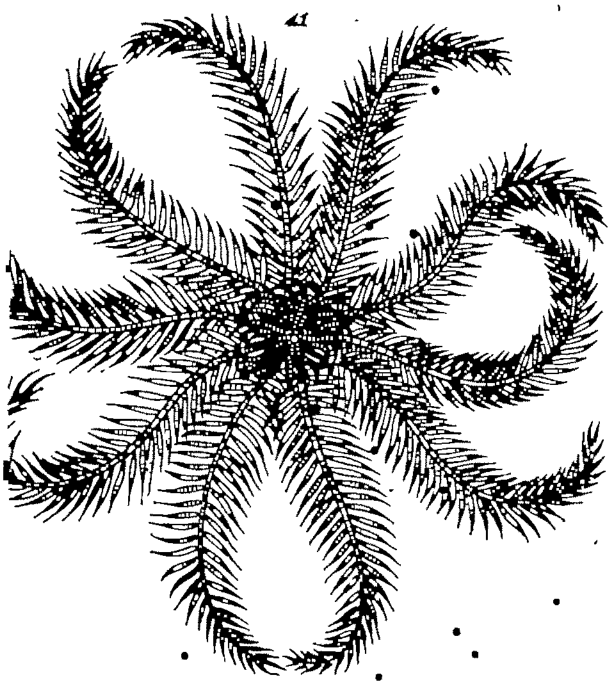

FIG. 41.—Comatula rosacea (after Forbes).

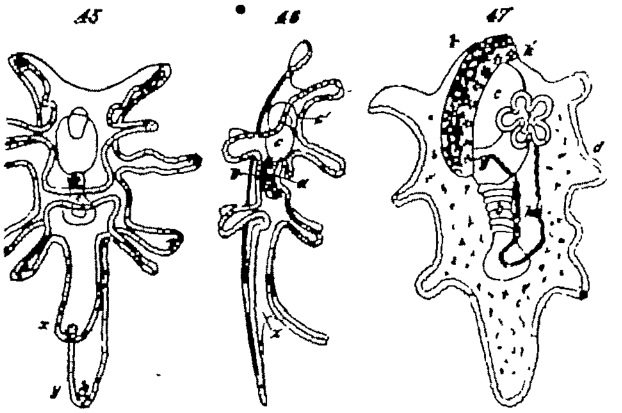

FIG. 45.—Larva of Starfish (Bipinnaria), × 100 (after Muller). 46, Larva of Starfish (Bipinnaria), × 100, seen from the side. *a*, mouth; *b*, œsophagus: *c*, stomach: c^1, intestine. 47, Larva of another Bipinnaria, showing the commencement of the starfish. *g*, canal of the ciliated sac; *i*, rudiments of tentacles; *d*, ciliated band.

As this process continues the little creature gradually loses its power of swimming and sinks to the bottom, loses the bands and cilia and attaches itself to some stone or other solid substance, by its base, the knob of the club being free. The calcareous framework increases in size, and the expanded head forms itself into a cup, round which from five to fifteen delicate tentacles, as shown in Fig. 44, make their appearance.

In this stage the young animal resembles the Crinoids, a family of Echinoderms which were very abundant in earlier geological periods, but which have now almost disappeared, being, as we see, represented by the young states of our existing, more advanced, species. This attached, plant-like condition of Comatula, was indeed at first supposed to be a Crinoid, and was named Pentacrinus, though we now know that it is only a stage in the development of Comatula. The so-called Pentacrinus increases considerably in size, and after various gradual changes, which time does not now permit me to describe, quits the stalk, and becomes a free Comatula.

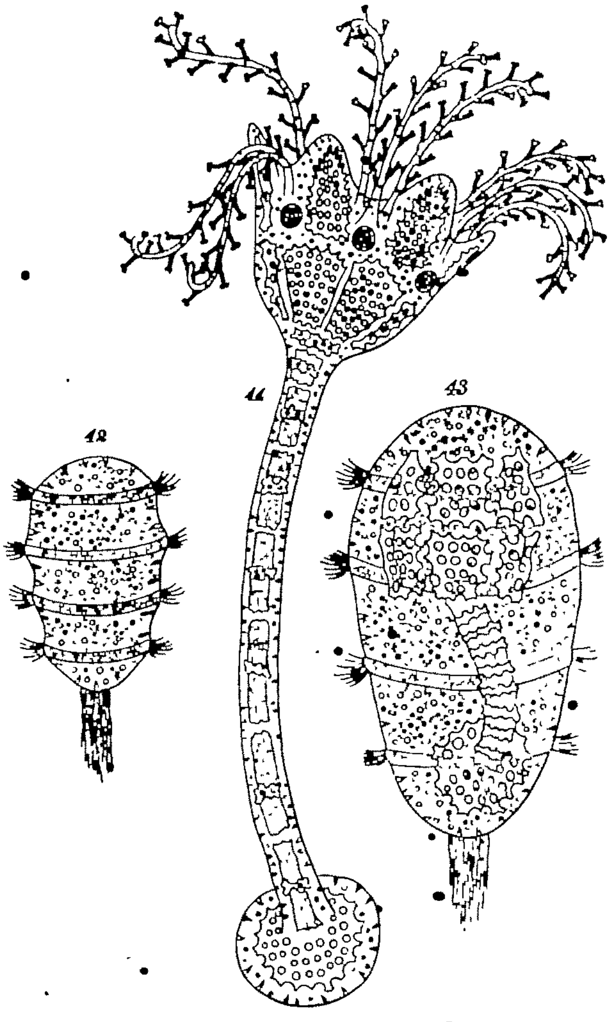

FIG. 42.—Larva of Comatula rosacea (after Thomson) 43, Larva of Comatula rosacea, more advanced. 44, Larva of Comatula rosacea, in the Pentacrinus state.

The metamorphoses of the true star-fishes are also very remarkable. Sars discovered in the year 1835 a curious little creature about an inch in length, which he named *Bipinnaria asterigera*, and which he then supposed to be allied to the ciliograde Medusæ; subsequent observations however, made in 1844, suggested to him that it was the larva of a star-fish, and in 1847 MM. Koren and Danielson satisfied themselves that this was the case.

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[*] Continued from p. 70.

[†] Philosophical Transactions, 1865, vol. clv. . 513.

Figs. 45 and 46 represent the front and side view of a Bipinnaria found by Muller* near Marseilles. *a* is the mouth, *b* the œsophagus, *c* the stomach, *c'* the intestine. Fig. 47 represents a somewhat older specimen in which the Starfish (*k*) is already beginning to make its appearance.

But while certain Starfishes thus go through metamorphoses, similar in character to, and not less remarkable than, those of sea-eggs; there are others, as, for instance, the genus Asteracanthium, in which the organs and appendages special to the Pseudembryo, are in abeyance, while in Pteraster "the zooid is reduced to an investing sheet of sarcode." †

Even in the same species the degree of development attained by the larva differs to a certain extent according to the state of the temperature, the supply of food, &c. Thus in Comatula, specimens which are liberally supplied with sea-water, and kept in a warm temperature, hurry as it were through their early stages, and the free larva becomes distorted by the growing Pentacrinus, almost before it has attained its perfect form. On the other hand under less favourable conditions, if the temperature is low, and food less abundant, the early stages are prolonged, the larva is longer lived, and reaches a much higher degree of independent development. Weissmann has observed similar differences in the larvæ of Flies,‡ and it is obvious that these facts throw much light on the nature and origin of metamorphoses as we see them among insects, but the latter question we shall now proceed to consider.

ON THE ORIGIN OF METAMORPHOSES

The question still remains, Why do insects pass through metamorphoses? Messrs. Kirby and Spence tell us they "can only answer that such is the will of the Creator;"§ which, however, is rather a general confession of faith than an explanation of metamorphoses. And this they appear to have felt themselves; for they immediately proceed to make a further suggestion. "Yet one reason," they say, "for this conformation may be hazarded. A very important part assigned to insects in the economy of nature, as I shall hereafter show, is that of speedily removing superabundant and decaying animal and vegetable matter. For such agents an insatiable voracity is an indispensable qualification, and not less so unusual powers of multiplication. But these faculties are in a great degree incompatible; an insect occupied in the work of reproduction could not continue its voracious feeding. Its life, therefore, after leaving the egg, is divided into three stages."

But there are some insects, as, for instance, the Aphides, which certainly are not among the least voracious, and which grow and breed at the same time. There are also many scavengers among other groups of animals, such, for instance, as the dog, the pig, and the vulture, which undergo no metamorphosis.

It is certainly true that, as a general rule, growth and reproduction do not occur together; and it follows, almost as a necessary consequence, that in such cases the first must precede the second. But this has no immediate connection with the occurrence of metamorphoses. The question is, not why an insect does not generally begin to breed until it has ceased to grow, but why, in attaining to its perfect form, it passes through such remarkable changes. And in addition to this, we must consider, first, the sudden and apparently violent nature of these transitions, and, secondly, the immobility of the animal in its pupa state; for undoubtedly the quiescent and deathlike condition of the pupa is one of the most remarkable characteristics of insect-metamorphosis.

In the first place, it must be observed that many species which differ considerably in their mature state, agree more nearly when young. Thus birds of the same genus, or of closely allied genera, which, when mature, differ much in colour, are often very similar when young. The young of the lion and the puma are often striped, and fœtal whales have teeth. Leidy has shown that the milk teeth of the genus *Equus* resemble the permanent teeth of the ancient *Anchitherium*, while the milk-teeth of *Anchitherium* again approximate to the dental system of the still earlier *Merychippus*. Rutimeyer, while calling attention to this interesting observation, adds that the milk-teeth of *Equus caballus* in the same way, and still more those of *E. fossilis*, resemble the permanent teeth of *Hipparion*.

In fact, the great majority of animals do go through well-marked metamorphoses, though in many cases they are passed through within the egg, and thus do not come within the popular ken. "La larve," says Quatrefages, "n'est qu'un embryon à vie independante."* Those naturalists who accept in any form the theory of evolution, consider that "the embryonal state of each species reproduces more or less completely the form and structure of their less modified progenitors."† "Each organism," says Herbert Spencer,‡ "exhibits within a short space of time a series of changes which, when supposed to occupy a period indefinitely great, and to go on in various ways instead of one way, give us a tolerably clear conception of organic evolution in general.

The naturalists of the older school do not, as Darwin and Fritz Muller have already pointed out, deny the facts, though they explain them in a different manner—generally by the existence of a supposed tendency to diverge from an original type. Thus Johannes Muller says "the idea of development is not that of mere increase of size, but that of progress from what is not yet distinguished, but which potentially contains the distinction in itself, to the actually distinct,—it is clear that the less an organ is developed, so much the more does it approach the type, and that, during its development, it more and more acquires peculiarities. The types discovered by comparative anatomy and developmental history must therefore agree."

And again, "What is true in this idea is, that every embryo at first bears only the type of its section, from which the type of the class, order, &c., is only afterwards developed."

Agassiz also observes that "the embryos of different animals resemble each other the more the younger they are." There are, no doubt, cases in which the earlier states are rapidly passed through, or but obscurely indicated; yet we may almost state it as a general proposition, that, whether before or after birth, animals undergo metamorphoses. The maturity of the young animal at birth varies immensely. The kangaroo (*Macropus major*), which attains a height of seven feet, ten inches, does not when born exceed one inch and two lines in length; the chick leaves the egg in a much more advanced condition than the thrush; and so among insects the young cricket is much more advanced, when it leaves the egg, than the fly or the bee; and it is a familiar fact, that in this respect, though not of course to anything like the same extent, differences occur even within the limit of one species.

In oviparous animals the condition of the young at birth depends much on the size of the egg; where the egg is large, the abundant supply of nourishment enables the embryo to attain a higher stage of development; where the egg is small, and the yolk consequently scanty, it is soon exhausted, and the embryo requires an additional supply of food. In the former case the embryo is more likely to survive; but, on the other hand, when the eggs are large, they cannot be numerous, and a multiplicity of germs is, in some circumstances, a great advantage. Even in the same species the development of the egg offers certain differences.*

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* lo Zweit. Abh. Pl. 1, Figs 8 and 9.

† Thomson, on the Embryology of the Echinodermata, *Natural History Review*, 1863, p. 415.

‡ Zeits. für Wiss. Zool. 1864, p. [illegible].

§ Introduction to Etymology, 6th Ed. i. p. 61.

* Metamorphoses de l'Homme et des Animaux, p. 133.

† Darwin, Origin of Species, 4th Ed. p. 532.

‡ Principles of Biology, vi. p. 349.

The metamorphoses of insects depend then primarily on the fact that they quit the egg in a very early condition; many—as, for instance, flies and bees—before the thoracic segments are differentiated; others—as locusts, dragon flies, &c., after the formation of the legs, but before that of the wings.

We may now pass to the second part of the subject, that is to say, the sudden and abrupt instance of the changes which insects undergo. The development of an Orthopterous insect, indeed—say, for instance, of a grasshopper—from birth to maturity is so gradual, that but for the influence on our nomenclature exercised by the most striking changes which occur in insects of the Heteromorphous series, they would perhaps never have been classed as metamorphoses. But though the changes from the caterpillar to the chrysalis, as from the chrysalis to the butterfly, are apparently sudden and abrupt, this is in reality more apparent than real; the changes in the internal organs, though rapid, are in reality gradual; and even as regards the external form, though the metamorphosis may take only a few moments, this is but the change of outer skin—the drawing away, as it were, of the curtain; and the new form which then appears has been in preparation for days or, perhaps, weeks before.

Swammerdam, indeed, supposed (and his view was adopted by Kirby and Spence) that the larva contained within itself "the germ of the future butterfly, enclosed in what will be the case of the pupa, which is itself included in three or more skins, one over the other, that will successively cover the larva." This is a mistake; but it is true that, if a larva is examined shortly before it is full grown, the future pupa may be traced within it. In the same manner, if we examine a pupa which is about to disclose the butterfly, we find the future insect, soft indeed and imperfect, but still easily recognisable, lying more or less loosely within the pupa-skin.

One important difference between an insect and a vertebrate animal is, that whereas in the latter, as for instance in ourselves, the muscles are attached to an internal bony skeleton, in insects no such skeleton exists. They have no bones, and their muscles are attached to the skin. Hence the necessity for the hard and horny dermal investment of insects, so different from the softness and suppleness of our own skin. Moreover the result is, that without a change of skin a change of form is impossible. The chitine, or horny substance, forming the outside of an insect, is formed by a layer of cells lying beneath it, and, once secreted, cannot be altered. From this it follows that every change of form is necessarily accompanied by a change of skin. In some cases, as for instance in *Chloeon*, each change of skin is accompanied by a change of form, and thus the perfect insect is more or less gradually evolved. In others, as for instance in caterpillars, several changes of skin take place without any material alteration of form, and the change, instead of being spread over many, is confined to the last two moults.

The explanation of this difference is, I believe, to be found in the structure of the mouth. That of the caterpillar is provided with a pair of strong jaws, fitted to eat leaves; and the digestive organs are adapted for this kind of food. On the contrary, the mouth of the butterfly is suctorial; it has a long proboscis, beautifully adapted to suck the nectar from flowers, but which would be quite useless, and indeed only an embarrassment to the larva. The digestive organs also are adapted for the assimilation, not of leaves, but of honey. Now it is evident that if the mouth-parts of the larva were slowly metamorphosed into those of the perfect insect, through a number of small changes, the insect would in the meantime be unable to feed, and liable to perish of starvation in the midst of plenty. On the contrary, in the Orthoptera, and as a general rule, among those insects in which the changes are gradual, the mouth of the so-called larva resembles that of the perfect insect, and the principal difference is in the presence of wings.

Similar considerations throw much light on the nature of the chrysalis or pupa state—that remarkable period of death-like quiescence which is one of the most striking characteristics of insect metamorphosis. The comparative quiescence of the pupa is mainly owing to the rapidity of the changes going on in it. In the chrysalis of a butterfly, for instance, not only (as has been already mentioned) are the mouth and digestive organs undergoing change, but the same is the case with the muscles. The powerful ones which move the wings are in process of formation; and even if they were in a condition favourable to motion, still the nervous system, by which the movements are set on foot and regulated, is also in a state of such rapid change that it could scarcely act.

It must not be forgotten that all insects, indeed all articulate animals, are inactive for a longer or shorter space of time after each moult.

The slighter the change the shorter the period of inaction. Thus, after the ordinary moult of a caterpillar, the insect only requires rest until the new skin is hardened. When, however, the change is great and gradual, the period of inaction is correspondingly prolonged. The inactivity of the pupa is therefore not a new condition peculiar to this stage, but a prolongation of the inaction which accompanies every change of skin. Most pupæ indeed have some slight powers of motion; those which assume the chrysalis state in wood or under ground usually come to the surface when about to assume the perfect state, and the aquatic pupæ of certain Diptera, swim about with much activity. Among the Neuroptera certain families have pupæ as quiescent as those of the Lepidoptera; others, as, for instance, Raphidia, are quiescent at first, but at length acquire sufficient strength to walk, though enclosed within the pupa skin, a power dependent partly on the fact that this skin is very thin. Others again, as, for instance, dragon-flies, are quiescent on assuming the pupa state, only in the same manner and for a similar time as at other changes of skin.

JOHN LUBBOCK

(*To be continued.*)

NOTES FROM THE "CHALLENGER"

III.

THE MILLER-CASELLA THERMOMETER

AT 8 A.M., on March 26, we sounded, lat. 19° 41′ N. long. 65° 7′ W., in 3,875 fathoms. The sounding was perfectly satisfactory, and left no doubt that the depth was estimated within a very small error. The "Hydra" sounding instrument was used weighted to 3 cwt. A slip water-bottle, and two Miller-Casella thermometers (Nos. 39 and 42) were sent down along with it as usual. The tube of the "Hydra" came up filled with a reddish clay containing a considerable quantity of carbonate of lime. The two thermometers were broken, and as the mode in which the fracture occurred is in itself curious, and has an important bearing upon the use of these instruments at extreme depths, I will briefly describe the condition of the thermometers when they came to the surface.

No. 39, a valuable instrument, with a small and constant error, which we had used for some time whenever for any reason we required extreme accuracy, was shattered to pieces (Fig. 1).

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* For differences in larvæ consequent on variation in the external conditions, see *ante*, p. 31.

In No. 42 this instrument was externally complete, with the exception of a crack in the small unprotected bulb of the right limb of the U-tube. The inner shell of the protected bulb was broken to pieces (Fig. 2).

In both of these cases there seems little doubt that the damage occurred through the giving way of the unprotected bulb. In No. 39 the upper part of that bulb was ground into coarse powder, and the fragments packed into the lower part of the bulb and the top of the tube. The large bulb and its covering shell were also broken, but into larger pieces, disposed as if the injury had been produced by some force acting from within. The thermometer tube was broken through in three places; at one of these, close to the bend, it was shattered into very small fragments. The creosote, the mercury, and bubbles of air were irregularly scattered through the tube, and it is singular that each of the steel indices had one of the discs broken off. The whole took place no doubt instantaneously by the implosion of the small bulb, which at the same time burst the large bulb and shattered the tube.

In No. 42 a crack only occurred in the small bulb, either through some pre-existing imperfection in the glass or from the pressure. When the pressure became extreme the crack yielded a little, and the sea-water was gradually forced in, driving the contents of the thermometer before it, and taking it at a disadvantage from within, breaking the shell of the large bulb, which was unsupported on account of the belt of rarified vapour between it and its outer-shell. The pressure was now equalised within and without the instrument, and the injury went no farther. Alcohol, creosote, mercury, and sea-water were mixed up in the outer case of the large bulb, with the debris of the inner bulb, and one of the steel indices lay uninjured across the centre of it.

It now becomes an important question why the thermometer should give way at that particular point, and one still more important, how the defect is to be remedied. At first sight it is difficult to imagine why the small bulb should give way rather than the outer shell of the large one. The surface exposed to pressure is smaller, the glass is thicker, and it is somewhat better supported from within, as the tube is nearly filled with fluid under the pressure of an atmosphere. I believe the cause must be that the end of the small bulb is the last point of the instrument heated and sealed after the tube is filled with liquid, and that, consequently, the annealing is imperfect at that point. It is evidently of no use to protect the small bulb in the same way in which the large bulb is protected. The outer shell is merely a precaution to prevent the indications being vitiated by the action of pressure on the elastic bulb. Against crushing, it is no protection; rather a source of weakness, from its greatly increasing the surface. The only plan which seems to be feasible is to thicken the small bulb itself, and, if possible, to improve its temper. It is only fair to say that these thermometers were tested and guaranteed to only three tons on the square inch, and that the pressure to which they were subjected was equal to four tons.

WYVILLE THOMSON

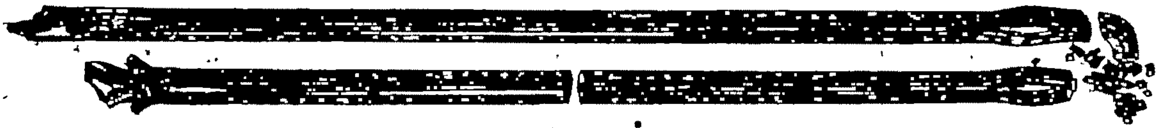

FIG. 1

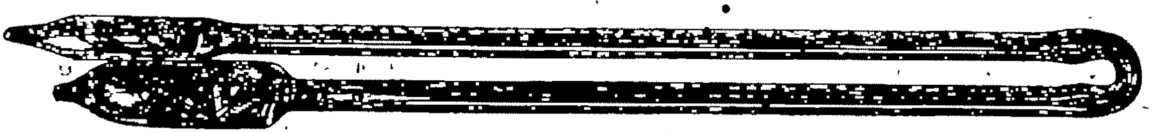

FIG. 2

NOTES

THE Albert Gold Medal of the Society of Arts has this year been awarded to M. Chevreul, Member of the Institute of France, and Director of the Gobelins and of the Jardin des Plantes at Paris, for his valuable researches in connection with Saponification, Dyeing, Agriculture, and Natural History, which, for more than half a century, have exercised a wide influence on the industrial arts of the world.

PROF. HUMPHRY announces that the Cambridge class for Practical Histology will meet during the months of July and August at the Anatomical Museum on Tuesdays, Thursdays, and Saturdays, at 9 A.M., commencing July 1. The Class for Human Osteology will meet on Mondays, Wednesdays, and Fridays at the Anatomical Museum at 9 A.M. during July and August, commencing July 2. The Professor of Zoology and Comparative Anatomy (Mr. Newton) announces that a class for practical work will be carried on in July and August by the Demonstrator in Comparative Anatomy, commencing July 2. The fee for the course will be one guinea.

THE following gentlemen have been recommended by the French Academy of Sciences to the Minister of Public Instruction for the four vacant posts in the Bureau des Longitudes:—M. Serret, M. Mouchez, M. Perrier, and M. Janssen.

THE Council of the Society of Arts having been informed that Her Majesty's Commissioners do not intend to publish reports on the different departments of the exhibition of the present year, have decided to undertake that duty, and for this purpose have engaged the services of gentlemen specially skilled in the subjects of the several sections, to prepare such reports for publication in the Society's *Journal*. A report on Ancient Objects, by Mr. C. Drury Fortnum, F.S.A., and another on Surgical Instruments and Appliances, by Mr. R. Brudenell Carter, F.R.C.S., appear in the *Journal* for May 30.

AT a meeting of the Council of the Leeds Naturalists' Field Club and Scientific Association, three of its members—Mr. Wm. Todd (vice-president), Mr. W. D. Roebuck (secretary), and Mr. John W. Taylor—were appointed a sub-committee to consider the best manner of collecting information for a series of catalogues of the natural productions of the district. The sub-committee having taken into consideration all the facts bearing upon the subject in hand, are of opinion that the following procedure should be adopted:—1. That in view of the approaching meeting in Bradford, in August next, of the British Association for the Advancement of Science, it is advisable that there should be produced by this society, and under its auspices, a brief account of the present state of our knowledge of the fauna, flora, and geological and topographical features of the district. 2. That for present use the most convenient district to illustrate would be the one produced by striking a circle of ten miles' radius, having the Leeds Town Hall for its centre. 3. That, as far as practicable, the lists should be complete, and as full as possible in detail as to the distribution of the species; and that they should be prefaced by a good outline sketch of the physical conformation and geological structure of the district. In conformity with these recommendations, the sub-committee would be glad to receive lists, as complete as possible, from all persons willing to co-operate in the work. These lists, to be available for immediate use, must be sent in before the 1st of July, 1873. Should the amount of information received up to that date warrant the Council in so doing, the lists will be placed in the hands of small committees of revision, whose province it will be to construct general catalogues combining all the information in the possession of the Society; and it is then hoped that during the first week in August, a small work may be published. The sub-committee will be very glad to receive all suggestions that may be made; and would be glad also to learn the names of all persons likely to be able to supply information. All communications to be addressed to the Secretaries, 9, Sunny Bank Terrace, Leeds.

WE have received the Report, for 1872, of Mr B. A. Gould, superintendent of the Argentine National Observatory at Cordoba, and a very creditable report it is, both to Mr. Gould and his assistants, as well as to the liberality of the Argentine Government, which seems to have done all in its power to provide the necessary buildings and instruments. The buildings are not yet quite complete, though a number of excellent instruments have been acquired, and others are being provided The principal work of the observatory has been the preparation of a Uranometry, which will contain a larger number of stars than the recently published one of Heis for the northern heavens; Heis's contains 5,421 stars. In the Uranometria Argentina, the brilliancy of the stars will be determined to single tenths of a limit of magnitude. There now remains nothing of importance to be done in the way of observation, since each star has been observed upon the average at least four times, and the degree of its brilliancy determined with the greatest precision possible. To prepare these results for publication, the position of every star will be computed for the commencement of 1875, a part of which labour has already been accomplished, and then to prepare the maps for reproduction by the engravers. The observation of the zones for the formation of an extended catalogue of stars between the 23rd and 80th degree of south declination, was commenced in September last, and is being carried on satisfactorily; as also is the photographic work of the observatory, a very considerable number of photographic impressions of clusters of stars having been made, which only need a knowledge of their zero of position to render them serviceable. Means are also taken to spread a knowledge of the exact time at regular intervals throughout the Confederation. Altogether the Argentine Observatory appears to be exceedingly efficient.

WE have received from Prof. A. Kerner, of Innsbruck, several interesting contributions to systematic and physiological botany —"Ueber die Schafgarben-Bastarte der Alpen," an account of the hybrid yarrows found in the Tyrolese Alps; "Novæ Plantarum Species, Decas iii.," containing descriptions of new Rubi of Austria and the Tyrol; and "Chronik der Pflanzen-wanderungen," in which he narrates the curious circumstances connected with the spread of a North-American plant, *Rudbeckia laciniata*. This plant first became known in Europe early in the seventeenth century, when it was introduced into the gardens of Paris; during two centuries and a half it has gradually spread over the gardens of nearly the whole of Europe, but appears only within the last twenty or thirty years to have escaped, and within that short space of time has become completely naturalised in a great number of places. In a communication to the Scientific and Medical Society of Innsbruck, Dr. Kerner states, as the result of his observations on Alpine plants, that the growth of the stem and even of the flowers of many species proceeds at the temperature of zero C.; the flowers may in some cases open, and even mature their pollen, beneath a thick covering of ice, the surface of the glacier being penetrated in innumerable places by their stems.

DR. A. KERNER reprints from the Proceedings of the Scientific Society of Innsbruck an interesting paper on the means of protection of the pollen of plants against premature displacement or damp. As the vitality of pollen is immediately destroyed by exposure to the action of either rain or dew, he finds in nature a variety of contrivances to protect it against these injurious influences during the interval between its escape from the anther and its being carried away by insects, these contrivances being generally absent in those plants where fertilisation is effected by the pollen being conveyed at once to the stigma by the wind. In plants with coherent pollen, fertilised by insect agency, where some of the anthers are so placed as to be necessarily exposed to the weather, these are generally found to be barren, or destitute of pollen, and where they would interfere with the entrance of insects into the flower, they are altogether abortive or rudimentary. Plants with coherent pollen, which require insect agency for their fertilisation, Dr. Kerner believes to be of more recent geological occurrence than those with powdery pollen, which require only the wind to convey it to the stigma.

THE proceedings of the Asiatic Society of Bengal contain remarks on winds, typhoons, &c., on the south coast of Japan, by Commander H. C. St. John, H.M.S. *Sylvia*. The most prevalent winds in the southern parts of Japan are from the north-east. Throughout an entire year the proportion was as follows, taking 1,000 hours as an index:—Between N. and E., 500; between N. and W., 200; between S and E., 100; between S. and W., 0·99 During April, May, June, July, August, and September, N.E. winds prevail, hauling more easterly in June, July, and August. In August and September S.E. winds are more frequent than during any other months. In October variable winds prevail, and the N.W. wind begins. During November, December, January, and February the N.W. winds prevail and blow hard. In March the N.W. and N.E. winds are equally distributed. The S W. winds most frequently occur during the early parts of September. It appears the winds on the southern coasts of Japan are easterly during April (spring), and hauling to the S. as the summer approaches, pass through S. and W. to N.W. during winter, coming again through N. to N.E. and E. in spring and summer. Typhoons occur between June and October, inclusive From the middle of August to the middle of October they may be expected to occur most frequently. The usual tracks of these storms on the Japan coasts appear very regular; approaching from the S.E travelling about N.W. On reaching the hot stream in about the latitude of the Bonin Islands, or between here and the Foochoo Islands, they begin to curve to the north, and following the course of the Kuro Siwo, strike the south coasts of Nipon. Owing chiefly to the high land along the coast, the northern disc of the storm becomes much flattened in, causing more easterly wind than would occur if the storm were in mid ocean. Retaining the course of the stream, they pass along in a north-easterly course, and, if not broken up previously, pass out into the Pacific Ocean on reaching Inaboya saki.

MR. JAMES WOOD-MASON has sent us a description of a Macrurous Crustacean, which he has made the type of a new genus, *Nephropsis Stewarti*. The specimen (a female) he describes was dredged in from 250 to 300 fathoms, about twenty-five miles off Ross Island on the Eastern coast of the Andamans. It is clearly allied to *Nephrops Norvegicus* of Northern European seas, its main difference being the absence of the squamiform appendage of the Antennæ. One of the most interesting points about the new crustacean is the loss of its organs of vision by disuse, a characteristic of several recently discovered crustaceans; this is compensated for by the great length and delicacy of the antennæ, and the great development of the auditory organs, the animal's habits being to burrow in the mud at the depth of about 300 fathoms.

IT may be recollected that M. Alphonse Pinart, the French philologist, visited the Aleutian Islands and Alaska in the summer of 1871, for the purpose of collecting the vocabularies and the photographs of the different tribes. This material he carried back with him to Paris, where he has been engaged in working it up. We learn that he expects to revisit the United States this month, with ample funds in his hands from the French government, in order to effect an exhaustive collection of the antiquities of Alaska, his excursions to the different islands being made in a vessel especially fitted up for his use. Alaska is one of the finest fields in the world for ethnological and prehistoric research.

PROFESSOR WYMAN has concluded, as the result of explorations among the shell mounds of Florida, U.S., during the past winter, that the aborigines by whom they were constructed must have been decided cannibals, as in eight different instances he has found considerable quantities of human bones in the shell heaps, the bones themselves being broken up and split, just as in the case of the bones of other animals. This, he is satisfied, was not the result of burial, but was done for the purpose of obtaining the marrow, probably after the flesh had been devoured.

UNDER the auspices of the Society of Biblical Archæology it is intended shortly to publish a series of translations of all the important Assyrian and Egyptian texts which exist in the various collections of England and the Continent, and thus place before the English student the remains of undoubtedly the oldest and most authentic literature in the world. Nearly all the principal translators have offered their services for this purpose, and while each author will be alone responsible for his portion of the work, the general arrangement of the materials will rest with the president of the society. The selection of the records will embrace the entire range of Egyptian and Assyrian history and literature. Each translation will quote the authorities upon which it is based, or the monument from which it is taken, and all other notes will be as few and brief as possible, to avoid controversy and expense. The first volume will be issued by Messrs. Bagster and Sons, at a price to bring it within the reach of all interested in such subjects.

THE *conversazione* of the Society of Arts will be held at the South Kensington Museum on Friday evening, June 28.

THE late distinguished chief of the U.S. Coast Survey, by his will, established a fund to be placed in the hands of executors, by whom the income is to be expended, under the direction of a committee of the National Academy of Science, for the advancement of some branch of physical research. The first report of results achieved through this bequest was recently made to the Academy by its President, Professor Joseph Henry. The committee had decided that in view of the great interest that Professor Bache had throughout his life manifested in terrestrial magnetism, it would be highly proper to further this science by gradually extending over the country the magnetic survey which, during his own lifetime, he had carried out in the Middle States. In the execution of this design they had been so fortunate as to secure, at small expense, the services of Dr. Hilgard, of St. Louis, by whom, in 1872, chiefly in the season most favourable for travelling, quite a large number of stations were occupied for the determination of the magnetic elements. These stations are mostly in the Southern States, and it is the intention of the committee to extend the work annually, northward and westward, as the income from the fund may allow.

THE U.S. Army Signal Office has made preparations for a great extension of its valuable system of reports of the heights of rivers, particularly of all those opening into the Mississippi. Over twenty-five stations are now established at suitable points on these rivers, especially, of course, on the Ohio, Missouri, and Mississippi. They are provided in some instances with automatic self-recording apparatus, and at all other places the observation of the height of the water is taken eight times daily when floods are apprehended. By this most beautiful system every wave of high water is accurately followed in its course down stream, and the approach of dangerous high floods is easily foretold by the repeated telegraphic reports. The system of river reports, which has been in operation during the past year, has given such universal satisfaction to those navigating the Western waters that the demand for increased facilities can only be met by this new and far more elaborate system of stations.

THE results of the explorations in the Gulf of St. Lawrence prosecuted during the months of July and August, 1872, by Messrs. Whiteaves and Bulger, have just been published. The area examined extended from a little above Cape Rozier to the Magdalen Islands. A depth of water somewhat over 200 fathoms was found near the centre of the mouth of the St. Lawrence, between Cape Rozier and the south-west point of Anticosti; the greatest depth actually met with was 313 fathoms, about half-way between the east point of Anticosti and the Bird Rocks. Large collections were made, embracing several species new to science. Among the novelties discovered was a sponge belonging to a genus but recently indicated in the "Depths of the Sea." About thirty-five species of corallines were obtained, large numbers of them being new. Numerous fine specimens of *Virgularia* were procured, the same kind having been found by Dr. Packard on the Georges Bank, and three species of sea-anemones were secured in addition to those of last year's collection. Two undescribed specimens of a coral (both dead) were also gathered at a considerable distance from each other. The relations of these new species are rather to the tropical forms than to those which we already know on the coast of the North Atlantic.

A SHOCK of earthquake, lasting for several seconds, was felt at Attok on the morning of Sunday, April 27.

WE have been favoured with a copy of the *Japan Gazette*, from which we take the following notes:—A huge cephalopod is now being shown in a house near the temple at Asaka, Yedo. It seems that a fishing-boat was seized by its tentacles whilst off the village of Kononoto, in the district of Kisaradzou, and that the boatmen killed the creature by repeated blows. Its length from the tail to the insertion of the tentacles is about sixteen feet; one of the arms is from its junction with the body to the sucker at its point nearly five feet. The polypus has shrunk since its death, so that living, it would probably measure considerably more.—The anomalous absence of earthquakes during the past winter has excited some speculation as to the causes of such quiet, in a country usually very tremulous towards the coming of spring. Whatever may be the real causes, the remarkable volcanic activity in Japan, during the past winter, and at present, is an interesting collateral phenomenon. From nearly all parts of the empire, during the last two months, have come tidings of mountains quaking and bursting in fissures, volcanoes casting out stones, ashes, and mud, and in some instances flame and hot lava. Smoke and steam from Asamayama have been visible from Yedo, several times this winter. In addition to the eruptions in Yechiu, Mito and Higo, the latter being especially severe and damaging to the cultivated land around it,—another mountain is reported as being affected with volcanic symptoms. Kmiokami-yama, near Nikko, which has, so far as is known, always been very quiet, was shaken with a great shock on March 12, at 3 P.M. The shock was accompanied by loud noise, and a strong smell of sulphur, which remained about six hours.

ADDITIONS to the Brighton Aquarium during the past week: a Porpoise (*Phocæna communis*) from Rye Bay; a Sturgeon (*Accipenser sturio*), 6 feet long, captured by the Bognor fishermen; Smooth Hounds or Skate-toothed Sharks (*Mustilus vulgaris*); White Hound or Toper (*Galeus canis*); Thornback Skate (*Raja clavata*); Sting Rays (*Trygon pastinaca*); Grey Mullet (*Mugil capito*); Flounders, fresh-water variety (*Pleuronectes flesus*); Butterfish or Gunnel (*Centronotus gunnellus*); Allis Shad (*Clupea alosa*); Salmon (*Salmo salar*); Ballan Wrasse (*Labrus maculatus*); Crabs (*Cancer pagurus*); (*Portunus puber*); (*Polybius Henslowii*); (*Carcinas Mænas*); Zoophytes (*Actinoloba dianthus*); (*Tealia crassicornis*); (*Sagartia miniata*); (*S. nivea*).

THE additions to the Zoological Society's Gardens during the past week include a Bengalese Cat (*Felis bengalensis*) and two Indian Crows (*Corvus splendens*) from Arracan, presented by Mr. W. Dunn; a New Caledonian Rail (*Ocydromus lafresnayanus*), presented by Dr. G. Bennett; an Indian Porphyrio (*Porphyrio indicus*) from the Navigator's Islands, presented by Rev. J. Whitmee; a dwarf Chameleon (*Chamæleon pumilis*), from South Africa, presented by Miss Siddons; an African Tantalus (*Tantalus ibis*); three Molucca Deer (*Cervus moluccensis*); a Vociferous Sea Eagle (*Haliaetus vocifer*), from Africa; a European Lynx (*Felis lynx*), and a Glutton (*Gulo borealis*) from Norway; a collared Amazon (*Chrysotis collaria*) from Jamaica; two common Spoonbills (*Platalea leucorodia*) from Europe, and two Wattled Cranes (*Grus carunculatus*) from South Africa, purchased; an American White Crane (*Grus americana*), received in exchange; three American Mocking Birds (*Mimus polyglottis*) hatched in the Gardens; and an Australian Thicknee (*Oedicnemus grallarius*) deposited.

SCIENCE IN ITALY

THE Transactions of the Academy of Sciences of the Institute of Bologna for the academical year 1871-2 contains twenty-nine memoirs read by members at the sittings of the Academy and several communications from without. I find it quite impossible to do justice to these without exceeding permissible limits, but will briefly refer to a few.

In a paper on a probable connection between solar eclipses and terrestrial magnetism, Dr. Michez, after describing the magnetic phenomena observed in Italy and more especially in Sicily during the eclipse of December 22, 1870, and pointing out the difficulty of separating the disturbances due to the eclipse from those otherwise produced, states the result of his laborious and careful study of the Greenwich magnetic records in relation to the passage of the lunar shadow over any part of the earth. Having determined the average *ordinary* declination and amount of agitation for the particular hour and season corresponding to that of each eclipse, he compares these with the declination and agitation observed while an eclipse was in progress, and collecting all these results and averaging the deviation of the eclipse periods from those of ordinary corresponding times, he concludes that an eclipse of the sun exercises a real influence on the declination needle, that this influence extends through several hours before and after the period of greatest solar obscuration, and that it is manifested by a greater agitation of the needle, and an eastward deviation. Upon theoretical considerations, Dr. Michez shows that the moon's shadow regarded in its relations to humidity should always produce an eastward deviation, but as regards the magnetic properties of oxygen should produce either an eastward or westward deviation, according to the position of the place of observation in relation to the shadow. Assuming that the latter, on a sufficiently large average, will neutralise each other, the residual phenomenon should be a slight eastward deviation.

In a paper on "The Climate of Europe during the Glacia Epoch," Dr. Bianconi, following De la Rive and Villeneuve, shows that the glacial extension of that period may have been due to greater humidity of climate rather than a lower mean temperature. Dr. Bianconi's conclusions are almost identical with those I suggested about fourteen years ago when describing a curious summer accumulation of ice in a previously unvisited Norwegian valley, where the snow line is actually lowered to an extent of about 3,000 feet, simply by a local increase of atmospheric humidity caused by the drifting spray of a double waterfall.* The subject was subsequently treated by Dr. Frankland in a lecture at the Royal Institution.

Prof. Filopanti contributed an interesting paper on the movements of the atmosphere, in which, after referring to the conclusions of Maury, that on both sides of the equator up to about the 30th parallel constant easterly winds prevail, from the 30th to the 35th variable winds, but still with the easterly predominating; from the 35th to the 40th, variable wind, with a commencement of westerly prevalence; and from the 40th to the Pole westerly winds decidedly prevailing. The object of Prof. Filopanti was to find a theoretical reason for these particular limits. To do this he regards the atmosphere as subject to the operation of two forces, viz. the resistance of the earth, and the mixture of aerial columns due to variations of temperature of the earth's surface. If only the first of these influences operated, the atmosphere would ultimately partake in every part of the velocity of the terrestrial parallel on which it rested, and there would be no sensible winds; if only of the second, the atmosphere would ultimately acquire throughout an absolutely equal velocity of rotation. He works out mathematically the amount of this velocity, and finds it equal to that of the surface of the earth at the latitude 35° 50′ 52″, which is a close approximation to the 35° of Maury. This he considers would be the uniform velocity of the air if the land and the sea were perfectly smooth, and he therefore designates the parallels of 35° on either hemisphere the "neutral parallels." Hence we are justified in theoretically anticipating that between the neutral parallels and the equator actual mean rotatory velocity of the air will be less than that of the earth, that is, the prevailing winds will be easterly, and that between the neutral parallels and the Poles the prevailing winds should be westerly, as there the mean rotatory velocity of the air should exceed that of the earth. The friction of the earth will be continually struggling to correct these differences of velocity, while the north and south movements, due to differences of temperature, will contest for their maintenance and augmentation. Prof. Filopanti goes further into details of special atmospheric currents to illustrate and confirm the above, but space will not permit me to follow him there. I have, however, so far sketched in abstract his leading idea as it appears to be an important contribution to the theory of atmospheric movements, and as far as I know is original. To some extent it is applicable to the vexed question of ocean currents.

The "Hermaphroditism" of eels has occupied a good deal of the attention of the Bolognese Academicians. Prof. Ercolani described a number of his own observations and experiments, showing that this hermaphroditism is "perfect," and the subject was further discussed at two subsequent meetings, when the results of previous researches of Vallisneri, Valsava, Allessandrini, Mondini, and others, were stated and compared. Besides the above and some others on subjects of general interest, are a few purely mathematical papers, and several on pathological, medical, and local subjects, which I must pass over.

Considering that Bologna itself is but a provincial town, and that the whole province of Bologna contains a population about equal to that of Birmingham, these Transactions of the Bologna Academy of Sciences indicate an amount of scientific activity in the highest direction of scientific research that we are unable to rival in any corresponding provincial district of Great Britain.

W. MATTIEU WILLIAMS

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* "Through Norway with a Knapsack," Chap. xv.

SCIENTIFIC SERIALS

Bulletins de la Société d'Anthropologie de Paris, 1871–72.—We find from these reports that the French palæontologists have been unusually active during the last eighteen months in continuing the exploration of the numerous bone-caverns of their country and in testing the accuracy of the older classifications of their remains. M. Barabean has been examining with great care the Dordogne district, which has become classic ground through the labours of Christie and Lartet. M. Saudon believes that the molars and maxilla recently found at Laugerie-Haute cannot be referred to the true horse—although they may provisionally, like similar remains found by M. Rivière in Italy—be accepted as belonging to some form of *equus*, for he does not think that the horse existed in Europe in pre-historic times. M. Mortillet, in obedience to the suggestions of M. Bertrand, Conservateur du Musée de S. Germain, has drawn up a chart of the palæolithic age in Gaul, the only work of the kind extant: in it are recorded 5 localities in which occur supposed traces of man in the tertiary; 43 alluvial deposits in the quaternary yielding human bones and industrial remains; and 278 caverns containing quaternary fauna with traces of pre-historic man. M. Mortillet thinks that we are no longer justified in assuming with E. Lartet that there was ever a special age of the bear or reindeer, all extinct animals having apparently lived through the whole palæolithic period. Amongst the numerous communications of M. Hamy, we may instance papers on the "Fossil Human Remains of d'Engihoul, near Liège;" "The Anthropology of Cambodia;" "The Quaternary Deposits of cut Silex recently discovered in the Pas de Calais;" "The Existence of Brachycephalic Negroes on the Western Coasts of Africa;" and "The Proportions of the Arm and Fore-arm to the different periods of Life." M. Doulish, from observations made at the close of 1871, in a bone cavern at Corgnac (Dordogne), believes that he has found incontrovertible proofs that man in the reindeer age had attained the art of *polishing* no less than of cutting stone.—M. Lagardelle communicates through M. Hamy, one of the Secretaries of the Society, some curious information in regard to the habitations of the degraded people known under the names of *Colliberts*, *huttiers*, &c., who for many ages occupied the marshy lands of Poitou, near the mouths of the Sèvre, and whose descendants were known till recently as *niolleurs*. This district was occupied by Gauls before the Norman Conquest, and after that event it became, from its inaccessible character, a place of refuge for fugitives. In the eleventh and twelfth centuries the Colliberts, whose special occupation was fishing, were dependent, as *homines conditionales*, on several religious houses, but were nevertheless left in a state of heathen, almost savage ignorance. Their huts were made of interlaced willow twigs, and their only means of locomotion before the formation of the network of canals, which have proved the chief agents in rescuing them from their isolation, were their long ash stilts and the so-called *nioles*, or light boats from which they took their name. The race is now merged in that of the contiguous *terra firma*.—M. Alph. Milne-Edwards has prosecuted an extensive series of observations on "The Embryology of the Lemurians and the zoological affinities of those animals;" and he finds that the placental system differs so widely from that of the Simiæ, with which they have been supposed to present very close relationships, that he is of opinion the Lemurs should take an intermediate, but wholly distinct, place between monkeys and carnivores.—M. Thorel's medical notes of his observations while serving in the exploring expedition to Meekong, in 1870, afford curious information in regard to the immunity to certain miasmatic affections presented by the people of Cochin China and other parts of Indo-China.—M. Sanson has laid before the Society his views on the Characterisation of Species, which are diametrically opposed to the Darwinian theory of evolution. The earlier numbers of the *Bulletins* for 1872, contain an unusually large proportion of papers on purely anatomical, psychological, medico-legal and similar subjects.—M. Broca considers, in a special mongraph, the importance of nasal configuration as a true ethnological character.—M. A. Roujon traces the analogies of the human type with that of the more ancient mammals, and proceeding to the length of concise definition, he fixes the probable appearance of the first lemurians at an epoch very remote from the secondary, and of monkeys—properly so called—before the tertiary, at the beginning of which period he thinks it not improbable that they engendered man.—The second and third numbers of vol. vii. of the *Bulletins* contain the exhaustive Treatise of M. Topinard on the indigenous races of Australia, with the valuable contributions and discussions in regard to the same subject by MM. Broca, Hamy, and Rochet. These numbers give us a general exposition of the progress and actual position of the science of Anthropology, and of the social advancement of our civilisation and its effect in obliterating ethnological characters and in elevating the lower type.

THE *Lens* for April commences with an analysis of the species of the genus *Amphora*, by Prof. H. L. Smith, in continuation of his Conspectus of the Diatomaceæ, accompanied by three excellent plates, and containing the description of nearly 100 species.—Dr. Danforth, of Chicago, describing "The Cell," developes Dr. Beale's theory respecting the nature of the nucleus, and discusses the action of carmine upon it.—Mr. H. Babcock, "On the Flora of Chicago and its Vicinity," catalogues the gramineæ and filices of that place very shortly.—There are also papers by Mr. J. H. Martin, "On the Similarity of various forms of Crystallisation to minute Organic Structures;" and by Mr. E. Colbert, "On the Figure of the Earth, and its Effect on Observations made in the Meridian."—The editor criticises the test employed by a committee of the Royal Microscopical Society of London in their decision respecting the angular aperture of Mr. Tolles's $\frac{1}{16}$th objective, thinking it unfair.

SOCIETIES AND ACADEMIES

LONDON

Royal Geographical Society, May 12.—Major-General Sir H. C. Rawlinson, K.C.B., president, in the chair.—The paper read was "Journey through Western Mongolia," by Mr. Ney Elias. The distance travelled over was 2,000 miles, accomplished between July 1872 and January 1873. The route from Kalgan (the starting-point in crossing the desert of Gobi by the usual route *viâ* Urga to Kiachta) was westerly to the Chinese frontier town of Kwei-hua, thence north-westerly to the river Onghin, and afterwards again westerly, along the foot of the Khangai Range, to the city of Uliassutai, which his observations showed to be 5,700 ft. above the sea-level. His further journey was impeded by the bands of Mahommedan Mongol rebels, the so-called Dungans, who, although badly armed, struck terror into the Chinese garrisons of the towns, and carried fire and slaughter wherever they went. He narrowly escaped the band, which a few days before his arrival destroyed the city of Kobdo, west of Uliassutai; arriving there, he saw the charred remains of the outer town and the unburied bodies of slaughtered people scattered over the streets. The Chinese garrison still occupied the fort, and received him and his party with kindness. All his endeavours, however, to obtain assistance for his further journey southward and westward to Kuldja were met by steady opposition, and he finally had to cross the frontier to the Russian town of Busk. The president informed the meeting that Mr. Elias had not only accomplished a wonderful journey over a tract of Central Asia never visited by a European since the times of Marco Polo, but had executed, unaided, a survey of the whole route travelled. His very numerous observations for longitude and latitude had been computed by Mr. Ellis, of the Greenwich Observatory, and those for heights above the sea-level by Mr. Strachan, of the Meteorological Office. For this great service rendered to geographical science, the Council of the Society has just awarded him the Founder's Gold Medal for 1873.

Meteorological Society, May 21.—Dr. J. W. Tripe, president, in the chair. The discussion was resumed on the following questions, which had been submitted to the consideration of the Meteorological Conference at Leipzig in August last:—No. 18: Can uniform times of observation be introduced for the normal observations? Remarks were made by the president, Dr. Mann, Messrs. Glaisher, Symons, Sopwith, Scott, Bicknell, Salmon, and Strachan, as to whether local or Greenwich time should be used, and whether the hours of 9 A.M. and 9 P.M., or 9 A.M., 3 P.M., and 9 P.M. should be recommended to observers. The meeting was of opinion that the hours of observation should be 9 A.M. and 9 P.M., and that local time should be adopted. The next question considered was No. 20: Division of the year for the calculation of mean results. After some discussion Mr. Sopwith suggested that a committee should be appointed to draw up a series of questions on all matters connected with this subject, and that the same be sent to the Fellows of the Society requesting their reply on all or any of the questions; this suggestion was approved of and adopted by the meeting.—A paper was then read on "Land and Sea Breezes," by Mr. J. K. Laughton, who was of opinion that sufficient attention had not been paid to the subject; and that more careful examination would show that the ordinary recorded theory is not in accordance with the facts observed; that these prove that sea and land breezes are seldom strong where the land is of that arid nature which gives rise to extreme differences of temperature, and that they frequently are strong where, from the verdant nature of the country, the differences of temperature are trifling; also that the sea breeze begins out at sea, and comes slowly in, and that the land-breeze comes, in the first instance, distinctly off the land, sometimes as sharp squalls. The necessary conclusion from these observations is that the breezes are winds of propulsion, not of aspiration; and whilst it seems probable that the propelling-force, in the case of the sea-breeze, is due to the rapid formation of vapour over the sea, the land-breeze may be the reaction, or return of the column of the air which has previously been forced upwards by the sea-breeze. A short paper by Rev. F. W. Stow, on the same subject, was read, giving an account of the observations he had made at Hawsker; after which Mr. R. H. Scott gave a description of a double rainbow observed at Kirkwall.

Institution of Civil Engineers, May 13.—Mr. T. Hawksley, president, in the chair.—The paper read, "On the Delta of the Danube, and the Provisional Works executed at the Sulina Mouth," by Sir Charles Augustus Hartley, was a sequel to a previous communication by the author on March 11, 1862. It described the mutations of the Sulina Bar from 1861 to the present time, and referred to the changes in the Sea outline of the Delta during sixteen years. Reference was made to the enormous growth of the northern part of the Kilia Delta in recent years, due to the greatly augmented volume of water which had lately flowed to the sea by the Ochakoff branch and New Stamboul Mouth; while a diminution in the advance of the southern extremity of the Kilia Delta was assigned to the impoverishment of the old Stamboul branch of the river. These changes, from natural causes, in the relative volumes of water delivered to the sea by the Kilia Mouths were favourable circumstances in considering the problem of the number of years that would probably elapse before the Sulina Mouth would be absorbed in the shallows of the Kilia Delta. Since 1857, owing to the shoaling of the Toultcha and the St. George's branches, the outflow by the Kilia had increased, so that it now delivered two-thirds of the whole volume of the Danube to the sea. Fortunately for the navigation by the Sulina Mouth, the larger portion of the detritus was transported far to sea, and comparatively little went to swell the shallows of the Kilia Mouths. In the last fifteen years the advance of the 30-feet line of soundings had been strictly confined to the sandbanks facing the mouths of the Kilia, Sulina and St. George, and it was shown that an erosive action had been long at work on the shore line and sea bottom to the north and south of the Sulina Mouth.

Society of Biblical Archæology, June 3.—Dr. Birch, F.S.A., president, in the chair. The folowing papers were read:—"The Legend of Ishtar descending to Hades." By H. F. Talbot, D.C.L., F.R.S., &c.—In this valuable paper the author translates from the tablets the Goddess's voluntary descent into the Assyrian *Inferno*. In the cuneiform it is called the Land of No Return; and the Lord of Earth gives her a green bough of the *Li*. . .tree to protect her life (comp. Virgil's Æneid). Ishtar passes successfully through the seven gates, compelled to surrender her jewels, (1) her crown, (2) her earrings, (3) her head-jewels, (4) her frontlets, (5) her girdle, (6) her finger and toe rings, (7) her necklace. The Lord of Hades seeing her sends his messenger Namtar to greet her. But as she cannot return of her own accord to the upper regions, the heavenly triad Sun, Moon, and *Hea* or Hu (Lord of Mysteries) consult, and *Hea* raises a black phantom who performs a juggler's trick before the Lord of Hades; during which he gives to Ishtar a cup full of the Waters of Life, whereby she returns to the upper world, receiving at each Hades-portal the jewels she had been deprived of in her descent. The phantom is rewarded by the most exquisite meats, wines, &c. The Greek Fate *Atropos* is supposed by the author to mean No Return, and Hades (House of Eternity) is compared with the Hebrew *Od* and *Bet-Moed* of Job xxx. 23.—"On the Egyptian Preposition," by M. P. Le Page Renouf, F.R.S.L.—"On a Remarkable Babylonian Brick described in the Bible," by Richard Cull, F.S.A.

PHILADELPHIA

Academy of Natural Sciences, February 11.—Dr. Ruschenberger, the president, in the chair. Mr. Thomas Meehan presented an apple, which was borne by a tree at Kittaning, in Pennsylvania, and which tree never produced any flowers in the popular acceptation of the term; but always yielded an abundance of fruit. The specimen furnished a practical illustration of some morphological truths which could not often be demonstrated in the way this afforded the opportunity of doing. It was admitted that a fruit was a branch with its accessory leaves transformed. The apple fruit was made up of a series of whorls of leaves comprising five each. Cutting an apple through we found a series of five formed the carpels containing the seeds. Several series of whorls, very much retarded in development, probably formed the stamens, but this could not be well seen in the apple fruit, as they seemed to be almost absorbed in the corolla series. This was the next in order that appeared in the divided apple—the green curved fibrous line which we find in all apples midway between the "core" and the "rind" is the dividing line between the series which forms the corolla, and the outer series which forms the calyx. In this tree there are no pistils, the series which usually goes to make up this part of the fruit structure being either very rudimentary or entirely wanting. Hence there was no core to the fruit. The result of this want of development was that the usual calyx basin of the apple was in this case occupied by a cavity three-quarters of an inch across. There were no petals; but in place five gland or rather bud-scalelike processes, at regular distances, on the edge of the green fibrous outline before referred to. The outer whorl, which usually forms the calyx, was almost asepalous, as a mere scarious membrane marked the place where the calyx segments or sepals should have appeared. It was so easy in this specimen to trace the dividing line between the outer or calycine whorl and the inner or coralline whorl, which, uniting and becoming succulent, formed the popular apple fruit, that it was worthy of note in this connection. But the most interesting feature in this specimen was what were probably, from their similarity in appearance, cork cells, formed abundantly on the outside of the apple. It would seem that, with the lack of development in the inner series of whorls necessary to the perfect fruit, those which remained were liable to take on somewhat the character of bark structure.

February 18.—Dr. Ruschenberger, the president, in the chair.—The following paper was presented for publication:—"Description of Mexican Ichneumonidæ, Part II," by E. T. Cresson.—Mr. Thomas Meehan presented specimens of leaves of a Begonia on which minute folioles appeared as densely as hair all over the upper surface, while the leaf was on the growing plant. The little growths first appeared as succulent hairs, and these hair-like processes subsequently divided or produced the leafy blades from their apices. Mr. M. remarked that hairs were at any rate structurally but graded thorns, of which bristles were an intermediate stage. Spines often bore leaves, but it was unusual for thorns to do so. It might not be that these leaf-bearing processes were really hairs though they had that appearance.—Mr. Thomas G. Gentry called the attention of the Academy to what he considered to be an interesting case of a change of habits which had recently occurred in the life of an ordinary chickaree, the *Sciurus hudsonius* of Pallas. During the early part of last autumn, his attention was called to the fact that the birds in a certain designated locality of Mount Airy, during the hours of the night, were undergoing a system of wholesale destruction, the work of small animals which were supposed to belong to some species of Carnivora. Labouring under this impression, and being desirous of securing a specimen or two, he started for the scene of slaughter, bent upon discovering the name and character of the animal; when within a few rods of the place, the almost deafening noise that greeted his ears, from the tall trees, led him to suspect that all was not right. After reaching the spot, a few moments of anxious waiting sufficed to reveal to him the cause of the noise and the origin of the sacrifice above alluded to; for, sitting upon a twig just above his head, he observed a *chickaree*, holding in its paws a bird which it had captured, and from which it was very contentedly sucking the life current. It is a well-established fact, he further remarked, as far as he had been able to verify it, that the numerous species of Rodents, with but two exceptions at the most, subsist principally or entirely upon vegetable matter, especially the hard parts of plants, such as nuts, bark, and roots. This habit of imitating the propensities of the *Mustelidæ*, he thought might have arisen from the habit which some squirrels possess, possibly the one under consideration, of sucking the eggs of birds; the blood-sucking habit he assumed to be an outgrowth from the other. This adoption of another's mode of life by *S. hudsonius*, he thought a discovery of some note, as usurpation of habits, leading to functional and structural changes in an animal's economy, is accounted an element of no mean weight in the *development hypothesis*, according to the testimony of able writers upon Evolution.—Prof Cope exhibited the cranium of the horned Proboscidian of Wyoming, *Loxolophodon cornutus*, and made some remarks on its affinities (see NATURE, vol. vii. p. 471).

CALIFORNIA

Academy of Sciences, April 21.—Prof. Davidson, president, in the chair.—Dr. Blake read a paper on the connection between the atomic weights of inorganic compounds and their physiological action. In a communication read before the Academy of Sciences of France, February 10, Messrs. Rabuteau and Ducoudray state that the poisonous effects of metals is greater as their atomic weight increases. When the different elements are grouped according to their isomorphous relations, there evidently exists a close connection between the *intensity* of their physiological action and relative atomic weights, and it is only under such conditions that the statement of Messrs. Rabuteau and Ducoudray is even approximately correct. That no absolute connection exists between the atomic weight of a metal and its physiological action is evident; for instance, the compounds of Beryllium with an atomic weight of 9· are far more poisonous than the salts of silver with an atomic weight of 103. As an example of the connection between the atomic weight and the poisonous qualities of a substance, the following table, drawn up from experiments which have not yet been published, furnishes strong evidence. The experiments were performed on rabbits, a solution of some salt of the metal being injected into the jugular vein.

Name of substance.	Atomic weight.	Quantity required to kill
Lithium	7	40 grs.
Sodium	23	20 ,,
Rubidium	85	6 ,,
Cæsium	133	8 ,,
Thalium	204	3 ,,

—Mr. Edwards presented a paper on the honey-making ant of Northern Mexico. The community is divided into three classes—the workers, carriers, and the honey-makers. The workers are much larger than the others, and of a black colour; they guard the nest and convey to it the materials from which the honey is made; these they deposit in a leaf over the centre of the nest, and from this leaf it is transported by the carriers to the honey-makers in the interior of the nest. The carriers are much smaller than the workers, and of a light brown colour. The honey-makers resemble the carriers in size and colour, with the exception of the enlarged abdomen. They are found in the centre of the nest, generally at a depth of two or three feet from the surface. They are supported on a sort of web made of closely woven fibres. Each ant occupies a superficial indentation in the web, in which it remains; in fact all locomotion in the honey makers is impossible, as the distended abdomen, which constitutes the honey-bag, is at least twenty times as large as the rest of the body. The honey is of a fine flavour, and much sought after by the natives.

PARIS

Academy of Sciences, May 26.—M. de Quatrefages, president, in the chair.—The Academy proceeded to the election of the candidates to be recommended to the Minister of Public Instruction for the four vacant posts in the Bureau des Longitudes. The following were the final results:—Member representing the Academy of Sciences, 1st line, M. Serret; 2nd line, M. O. Bonnet: Member of the Marine Department, 1st line, M. Mouchez; 2nd line, M. Bouquet de la Grye: Member of the War Department, 1st line, M. Perrier; 2nd line, M. Blondel: Geographical Member, 1st line, M. Janssen; 2nd line, M. d'Abbadie. The following papers were read:—On the assimilability of super-phosphates, by M. Joulie. The author found that "super-phosphate" consists of the following four bodies:—Free phosphoric acid, dihydric calcic phosphate, hydric dicalcic phosphate, and tricalcic phosphate. The first three of these can be taken up by plants; hence he decides, (1) that the amount of phosphoric acid soluble in water is not a true estimate of the value of the manure, but (2) that the amount soluble in alkaline ammonic citrate is; he therefore recommends the latter as the proper reagent for such estimations.—Rectification of a portion of the communication of M. Munk concerning the discovery of lunar variation, by M. L. A. Sédillot. This paper related to the disputed passage of Aboul Wefa.—On the calculus of the luminous phenomena produced in the interior of transparent media having a rapid motion of translation in those cases where the observer partakes of that motion, by M. J. Boussinesq.—On the electric balance and on electrostatic phenomena, by M. P. Volcipelli.—Researches on the electricity produced by mechanical action, by M. L. Joulin.—On the conditions of maximum magnetic effect in galvanometers and electro-magnets, by M. Raynaud.

DIARY

THURSDAY, JUNE 5.

LINNEAN SOCIETY, at 8.—On the Plants of Kilmanjaro: Dr. Hooker, F.R.S.—On the Lecythidaceæ: John Miers, F.R.S.

CHEMICAL SOCIETY, at 8.—On the Dioxides of Calcium and Strontium: Sir John Conroy, Bart.—On Iodine Monochloride: J. B. Hannay.—A new Ozone Generator will be exhibited by Mr. T. Wills.

ROYAL INSTITUTION, at 3.—Light: Prof. Tyndall.

FRIDAY, JUNE 6.

ROYAL INSTITUTION, at 9.—Lecture: Dr. Odling.

GEOLOGISTS' ASSOCIATION, at 8.—Ammonite Zones in the Upper Chalk of Margate, Kent: F. A. Bedwell.

ARCHÆOLOGICAL INSTITUTE, at 4.

GRESHAM LECTURES, at 7.—On Headaches: Dr. E. Symes Thompson.

SATURDAY, JUNE 7.

ROYAL INSTITUTION, at 3.—The Historical Method: John Morley.

GRESHAM LECTURES, at 7.—On Narcotics and Sedatives: Dr. E. Symes Thompson.

MONDAY, JUNE 9.

GEOGRAPHICAL SOCIETY, at 8.30.

TUESDAY, JUNE 10.

PHOTOGRAPHIC SOCIETY, at 8.—On Experiments with three wet processes: Jabez Hughes.—Notes on the Photo-collotype process: Capt. J. Waterhouse.—On some early Photo-engravings: W. H. Fox Talbot, F.R.S.

WEDNESDAY, JUNE 11.

GEOLOGICAL SOCIETY, at 8.—On the Nature and probable Origin of the superficial Deposits in the Valleys and Deserts of Central Persia: W. T. Blanford.—On *Caryophyllia Bredai*, Milne-Edwards, from the Red Crag: Prof. P. Martin Duncan, F.R.S.—On the Cephalopoda-bed and the Oolite Sands of Dorset and part of Somerset: James Buckman.—*Cetarthrosaurus Walkeri*, Seeley, an Ichthyosaurian from the Cambridge Upper Greensand: H. G. Seeley.

ARCHÆOLOGICAL ASSOCIATION, at 8.

GEOLOGISTS' ASSOCIATION.—Excursion to Brighton.

THURSDAY, JUNE 12.

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, at 8.30.

MATHEMATICAL SOCIETY, at 8.—Some general Theorems relating to Vibrations: Hon. J. W. Strutt.—Invariant conditions of three and four concurrence of three Conics: J. J. Walker.—Locus of the point of concourse of tangents to an epicycloid inclined to each other at a constant angle: Prof. Wolstenholme.

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ERRATA.—P. 85, col. 1, line 18 from bottom, for "disassociates" read "dissociates;" col. 2, line 14 from top, for "exact" read "&c.;" col. 2, line 28 from top, after "acid" insert "with tartaric acid;" col. 2, line 36 from bottom, for "solution. After boiling with acid a notable," read "solution after boiling with acid. A notable."

THURSDAY, JUNE 12, 1873

JEREMIAH HORROX

I.

IF national glory can ever be connected with a natural phenomenon, the transit of Venus over the sun's disc may be said to bring peculiar distinction to England. It is in a manner inscribed upon one of the most brilliant pages of our naval history; it led to some of the most remarkable discoveries for which mankind is indebted to our geographical enterprise, and made the renown of our most famous navigator. A hundred and thirty years before Cook, the phenomenon itself was, for the first time in human history, accurately observed in a corner of England, by an English youth, self-taught, and provided with few of the appliances of scientific research. Now that the spectacle, so striking in itself, so sublime in the infrequent regularity of its recurrence, so important as the key to numerous astronomical problems, is again attracting the attention of civilised mankind, now that the expanse of ocean from Honolulu to Kerguelen's Land is about to be dotted with watchers from the other side of the earth, the occasion appears favourable for recalling the memory of the original observer, Jeremiah Horrox, curate of Hoole, near Preston, in his day one of the most insignificant of English hamlets.

The little that is known respecting Horrox's family and circumstances at least suffices to reveal the difficulties with which he had to contend. The place of his birth was Toxteth, near Liverpool. We cannot discover that the date usually assigned, 1619, rests on any good authority, while it is rendered improbable by the fact that in this case he must have been matriculated at thirteen, and ordained at twenty. The first letter of his that has been preserved, dated in the summer of 1636, indicates, moreover, a compass of astronomical knowledge, as well as a general maturity of mind, hardly conceivable in a youth of seventeen; while his references to the discouragements which, previous to his acquaintance with his sympathising correspondent, had almost induced him to renounce astronomical study, bespeak a more protracted period of investigation than would have been possible in such early years. The date 1616, though unauthenticated by any external testimony, may very well be correct. Notwithstanding a doubtful report which traces his family to Scotland, his thoroughly Lancastrian patronymic denotes a local origin. His father's profession is unknown; we suspect him to have been a schoolmaster. The family dwelling is usually identified with a house pulled down a few years since to make room for the railway station. The family was numerous, and although it cannot have been indigent, Jeremiah's matriculation as a sizar at Cambridge, and short stay at the University, prove that it was not rich. His entrance at Emmanuel College, then a stronghold of Puritanism, is conclusive as to the auspices which presided over his bringing-up. This matriculation took place on July 5, 1632; he certainly left the university without a degree, and the fact of his first-recorded astronomical observation, June 7, 1635, having been made at Toxteth, is an almost certain testimony of his recession having taken place before that date. Want of means, and the necessity for contributing to the support of his family, are the only assignable reasons for a step which must have thrown the young student on his own resources, as regarded books, instruments, and intellectual companionship. The first glimpse we obtain of him is from the above-mentioned letter to Crabtree, dated June 21, 1636. From this and subsequent letters we gather that he has been for at least a year an observer of the heavens; that his circumstances are narrow, and prevent him from obtaining the books and instruments he desires; some, however, of the books he incidentally mentions must have been expensive, and can hardly have been procured by him elsewhere than at Cambridge. A list of these in his own handwriting is preserved, and has been noticed by Prof. De Morgan, who ("Companion to the Almanac" 1837) points out that not one was the work of an English mathematician, or printed in this country. It further appears that his time was much engrossed by other pursuits, which no doubt bore reference to his preparation for orders, and to his exertions to support himself in the interim. He was, in all probability, engaged in tuition, to which land-surveying, or some similar occupation, may have been added. Thus three years passed by, at the end of which time we find him curate of Hoole, a village about five miles to the south of Preston, the church of which was at that period a chapel of ease to the adjoining parish of Croston. The patron was Sir Robert Thorall, the incumbent the Rev. James Hyatt. Horrox may be assumed to have been recommended to the latter by their common Puritanism, Mr. Hyatt having been one of the ousted ministers of 1662. He did not, however, retain his curacy much above a year; the cause of his resignation is unknown.

It is now time to treat more specifically of Horrox's correspondence with Crabtree, the source of almost all our information respecting him. Crabtree, a clothier of Broughton, near Manchester, was one of a small band of worthies by whom astronomy was cultivated in the northern counties in those days, some particulars respecting whom will be found in the notes to Sherburne's translation of Manilius. These letters survive in the Latin version of Prof. Wallis, who naturally omitted whatever had no immediate bearing on science. A re-examination of the originals, should these still be extant in the Bodleian Library or elsewhere, might probably result in the retrieval of some interesting biographical particulars. As it is, we obtain many glimpses of the scientific circumstances of the day. Errors were inevitable in the comparative infancy of astronomical science, and the mistakes of the master were naturally a snare to the pupil. Horrox was for a time not only misled, but induced to distrust the accuracy of his own observations by their incompatibility with those of Lansbergius. Crabtree opened his eyes to the errors of the latter, and thus indirectly rendered him the still higher service of leading him to recognise the greatness of Kepler, which Lansbergius had disparaged. His study of Kepler led, as we shall see, to his own great discovery: before entering upon this, however, it will be convenient to dispatch the minor matters of scientific interest contained in the correspondence. It is curious to learn that Horrox's telescope cost him only 2*s*. 6*d*., and was nevertheless better than some more expensive ones which he had had an opportunity of examining. He did not obtain even this modest instrument until May 1638, about a year before Milton viewed the moon through "the optic glass" of "the Tuscan artist":—

> "At evening from the top of Fesole,
> Or from Valdarno, to descry new lands,
> Rivers or mountains in her spotty globe."

The "mute inglorious Miltons" of Toxteth seem not to have been wholly incurious respecting the researches of their fellow villager, who speaks in another letter of having endeavoured to exhibit Venus in her crescent phase to "sundry bystanders," who however were unable to discern the phenomenon owing to their inexperience in the use of the instrument. The possession of a telescope may have stimulated his desire to become acquainted with the writings of its inventor. Four months later we find him possessed of Galileo's dialogue on the "System of the Universe," and anxious to procure his "Nuncius Sidereus," and treatise on the Solar Spots. He had previously speculated upon the exact period of the creation of the world, which he sought to determine by a combination of astronomical and scriptural data; and upon the origin of comets, which he supposed to be emitted from the sun. The phenomena of the planetary aphelion and perihelion had likewise engaged his attention, and elicited remarks which almost seem prophetic of the great discovery of Sir Isaac Newton. In observing the setting sun he had noticed a raggedness of the margin, which he rightly attributed to atmospheric conditions. During the last three months of his life, when unable to bestow time on astronomical research, he commenced an attentive study of the irregularities of the tides, from which he hoped to obtain a demonstration of the rotation of the earth. The Lancashire coast, where the recess of the tide is very considerable, is highly favourable to similar observations.

(*To be continued.*)

CARUS'S HISTORY OF ZOOLOGY

Geschichte der Zoologie bis auf Joh. Müller und Charles Darwin, von J. Victor Carus. Pp. 739. (Munchen, 1872.)

TWO of the most characteristic qualities of the present time are scepticism and sympathy; and by a happy combination of the ability to investigate statements instead of taking them on trust, and the power of realising past states of knowledge and of feeling, a most important advance has been made in history. But the historical method is not confined to what is commonly so called. It has been applied to philology and philosophy, and has reformed both, while even in the physical sciences its importance is now fully recognised. It is true that a science like Zoology, which deals entirely with objective facts, is more independent of history than some others, and its history does not really begin till the seventeenth century. But as part of the history of the human mind, it will always be important to study the sciences of pre-scientific ages, and when we meet with such a master-mind as that of Aristotle, whatever he wrote becomes of the highest interest because it was his.

The work before us, by the son of the late eminent zoologist of the same name,* is one of the series undertaken by command of the late King of Bavaria, and published by a Historical Commission of the Royal Academy of Sciences in Munich. It embraces the history of the whole body of science in Germany, and the volumes which have already appeared have been written by men of high eminence in their several departments.

Fortunately, however, Prof. Carus does not at all confine himself to Germany, so that the present work is an attempt at a complete history of zoology, from the earliest to the present time. It naturally divides itself into two parts, the first treating of what may be called pre-scientific zoology, which is only of general historical interest, the second tracing the development of zoology, as a science of observation and experiment, from its foundation by Ray and Linnæus. These two sections are handled on a very different scale, for the former occupies more than half the book, and is therefore sufficiently minute, while the whole history of modern zoology is compressed into three hundred pages. The consequence is that, while accurate as to facts, the latter part is often little but a list of names and dates.

We shall therefore simply direct the attention of zoologists to the second portion of Prof. Carus's history as convenient and well-arranged for reference, and dwell here on his detailed account of the less known progress made in ancient and mediæval times towards a knowledge of the varieties and structure of animals.

The first chapter treats of the earliest animals known to man, including those domesticated in prehistoric times. The names of the Ox, Sheep, Goat, Pig, Dog, Horse, and Goose, occur in allied forms in most of the Indo-European languages, and their bones are found among the dust-heaps of the earliest race of men known. The Cat (*αἴλουρος*), though domesticated in Egypt, was not a household animal till much later in Western Europe: the "cat" of the Greeks and Romans (*γαλῆ*) being almost certainly the whitebreasted beech-marten (*Martes foina*) a conclusion learnedly and perspicuously established by Prof. Rolleston in a paper published in the *Journal of Anatomy and Physiology*, for November 1867. But the Flea and the Louse appear to have been familiar from the earliest times, and Mice, Flies, and Worms are also among the first named by man. To the same primitive group belong the Bear, the Beaver, which lived in English rivers up to comparatively recent times, and the Wolf and Fox, the names of which (*vulpes*, Wolf) have evidently been confounded.

After a short account of the part taken by animals in early mythology and in the fables common to the Indo-European nations—a chapter which might have been with advantage enlarged from the pages of Grimm, Dasent, and Link—our author enumerates the domestic animals known in classical times, which include, beside these already mentioned, the Camel (confounded with the elephant during the Middle ages), the common Fowl (*ὄρνις περσική* Aristoph., Av. 485), which was introduced from the East between the date of Homer and Hesiod and that of Æschylus, the Chenalopex, probably identical with our sheldrake (*Tadorna vulpanser*), pigeons of various breeds, and birds of prey which were used for hawking. The list of wild animals was greatly increased by the games of the Roman circus, and many, like the Hippopotamus, Rhinoceros, and Giraffe were better known under the Empire than they have been until very recent times. Pliny mentions the occurrence of the Platinista in the Ganges, but no notice of the Hyrax, a form so familiar to the Hebrews, is to be found in Greek or Roman authors.

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* The accomplished author himself is now lecturing in Edinburgh as Prof. W. Thomson's substitute.

The next sections are occupied by a tolerably full account of the knowledge of anatomy and physiology possessed by Aristotle, and by his successors, Herophilus and Erasistratus, and of the attempts made towards a classification of the animal kingdom. The groups recognised by the first, and perhaps the greatest, of naturalists, are surprisingly near to what are now accepted. 1. Viviparous quadrupeds, clothed with hair (*ζωοτόκα τετράποδα*)—Mammalia, exclusive of Cetacea. 2. Birds (*ὄρνιθες*) exclusive of bats. 3. Oviparous quadrupeds, inclusive of snakes and frogs. 4. Cetacea (*κήτη*), with teats and milk (Hist. An., iii. 99). 5. Fishes (*ἰχθύες*). Those with (red) blood are distinguished from the remaining "bloodless" classes. 6. The Cephalopodous mollusks (*μαλάκια*). 7. The testaceous mollusks, including ascidians, cirripedia and echinidæ (*ὀστρακόδερμα*). 8. Malacostraca—Crustacea. 9. Insecta (*ἔντομα*) including all air-breathing Arthropoda. Lastly, Starfishes, Sponges, and some other groups, are characterised as partaking of the nature of plants (Zoophyta).

On the whole, Aristotle's zoology is less imperfect than his anatomy. In spite of Prof. Carus's opinion, the well-known passage (Hist. An. i. 39) clearly states what is repeated in two other passages, that the back of the skull is empty, and his views of the position and functions of the heart, lungs, and nerves are scarcely more scientific than Plato's notions of hepatic triangles. Indeed it is difficult to believe that Aristotle can ever have completely dissected a single mammal. The digestive and reproductive systems he understood much better. But beside his wonderful industry in collecting facts, the acuteness and power of generalisation displayed by Aristotle in other branches of science are not wanting in natural history. Thus he remarks that insects with horny wings have no sting. "I have never seen an animal with solid hoofs and two horns." When horns are present there are no canine teeth. Quadrupeds which bring forth their young alive are clothed with hair, those which lay eggs, with scales. Insects with four wings have the sting behind, those with two, in front. Nor is it the least proof of Aristotle's greatness that he gave an impetus to biological science which produced the Alexandrian school of anatomy, and only ended at the beginning of the third century of our era with the death of Galen.

The contributions of Roman authors to zoology, such as those buried in the huge mass of crude and chiefly worthless material which Pliny called natural history, only mark the decay of the science. During the subsequent dark ages (the darkness of which is probably for the most part subjective) the most remarkable work on zoology is the famous "Physiologus," also called the "Bestiarius Theobaldi," of uncertain authorship and date, but known over the whole of Christendom from the eighth to the thirteenth century by translations into Syriac, Armenian, Arabic, Ethiopic, German, English, Icelandic, and French. The Greek text is probably the original, from which the Latin was taken. This long-forgotten book, like Pliny's, includes accounts of plants, stones, and other natural objects, and describes among more common-place animals, mermaids, unicorns, and onocentaurs. There are mentioned, of quadrupeds, the antelope (perhaps the Urus), beaver, elephant, hyæna, monkey, and lion, beside common European species; thirteen species of birds, including the ostrich; of reptiles, several kinds of lizards, and serpents, but only one invertebrate animal, the ant. The original plan appears to have included only the animals mentioned in the Bible, and the chief object of the book is to draw moral lessons from the habits of the creatures described. The "Physiologus," while of great historical interest, is, of course, devoid of even relative scientific value.

Passing over the Arabian naturalists, who added little original, we come to the three writers who represent the science of the Middle Ages when the writings of Aristotle became generally known and the systems of scholastic philosophy were founded—Thomas of Cantimpré, Albertus Magnus, bishop of Ratisbon, and Vincent of Beauvais. They were all Dominicans, and all belong to the thirteenth century, that remarkable era of revolution in philosophy, politics, and art. At this time knowledge of foreign animals was greatly increased by the travels of Marco Polo (1275–1292), who described the wild horses, musk deer, and yaks of Tartary, the camels and asses of Persia, and the rhinoceroses, elephants and tigers of India.

Museums only began to be formed in the sixteenth century when the discovery of America brought to light so many new animals and plants; but for a long time they were what museums still too often are, mere lumber rooms of "Dinge gantz seltzam und fremdt," as Duke Albert of Prussia wrote in 1559. All the earliest anatomical preparations, including the celebrated dissections of Harvey still preserved in the College of Physicians, are dry.

The *Lucidarius*, a medley of stories about animals, which represents in the Renaissance what the Physiologus does in the Middle Ages, appeared in 1479, and like the latter was translated into all the European languages.

The earliest attempt at a System of Zoology was by Wotton in his *Differentiis Animalium*, published at London in 1550. It is little more than a reproduction of the doctrine of Aristotle. Conrad Gesner's *Historia Animalium* appeared in 1551. Like Wotton, he was a physician, and practised in Switzerland and South Germany. His work is chiefly remarkable for its illustrations, one of which, the figure of the Rhinoceros, was drawn by Albert Durer. Passing over the names of Aldrovandi (1522-1603), Johnston (1603-1675), and Sperling (1603-1661), the next important work on zoology was Bockart's *Hierozoicon*, published in 1663. This work of the learned Norman Huguenot has been a quarry which succeeding biblical commentators have continually used, but its value is almost entirely literary: indeed it was written rather as a contribution to hermeneutics than to natural science. The figures in a work of Clusius, "Exotica," which belongs to the early part of the seventeenth century, show by those of the sloth, the manatee, the armadillo, humming-bird, cassowary, dodo, penguin, and molucca crab, how much the discoveries made in America, Madagascar, and New Holland, were increasing the list of known animals.

During the first half of the seventeenth century there also appeared the earliest monographs. Thus Nicholas Tulp, the anatomical lecturer in Rembrandt's famous painting at the Hague, gives a description and an admirable engraving on copper of what he calls an "orang-outang," evidently a chimpanzee from Africa; and in the same *Observationes Medicæ* (1641) figures a narwhal as "Unicornis marinus." The *Libellus de Canibus Britannicis* (dedicated to Gesner), of our countryman John Kay (Caius) was earlier than Tulp's papers. It was followed by monographs on the elephant by Lipsius and Caspar Horn, on the stag, with an account of its dissection, by Agricola, of the hippopotamus, from a specimen sent in brine from Damietta to Rome, by Columna, and of fishes in general by Salviani and Rondelet. In 1634 was published at London *Insectorum theatrum*, avowedly founded on the words of Wotton and Gesner, and on a compilation from both which had been begun by Thomas Penn, and interrupted by his death; the next editor was Thomas Mouffet, but he also died several years before it was published. This is a noble monograph, with woodcuts so accurate and characteristic as to compare with the best productions of modern skill. It is also remarkable for containing a full and correct account of the *Acarus scabiei*, which was afterwards so long forgotten. Beside insects (in the Linnæan sense of the word) it describes worms of various kinds, and among them what is apparently a *Bothriocephalus latus*. This species is still more distinctly figured by Tulpius (Obs. Med. tab. vii), but by some strange error it is represented with two heads. Spigelius (*de lumbrico lato*) gravely discusses whether it is an animal at all.

Meantime anatomy and physiology were making rapid progress. Vesalius (1514-1564), the father of modern anatomy, and his contemporary Eustachius, who ventured to oppose his own dissections to the authority of Galen, Fallopius, and his successor at Padua, Fabricius, and the still more illustrious pupil of Fabricius, William Harvey, form a succession of almost unequalled eminence. The dissections of our countryman Thomas Willis (1621-1675) were not confined to human subjects, and the earliest microscopical observations, by Malpighi, Leeuwenhoek, and Hooke, were also to a large extent zoological. After the middle of the seventeenth century the three most illustrious scientific societies were founded, the *Academia Naturæ Curiosorum* (1652) incorporated as the "Leopoldinisch-Carolinische Academie" in 1677, the Royal Society in 1662, and the Académie des Sciences four years later. In 1667 Ray was elected a Fellow of the Royal Society, and began the series of papers which mark the first steps of scientific zoology, and surely prepared the way for his greater successor Linnæus.

P. H. PYE-SMITH

OUR BOOK SHELF

Lehrbuch der Physik. Von Dr. Paul Reis, Zweite Lieferung, Leipzig. (Quandt and Bäudel, 1873.)

THE second part of this useful handbook of physics opens with the explanation of Mariotte's Law and the various applications of atmospheric pressure. The next division is devoted to the study of wave motion, which is discussed far more fully than in the ordinary run of scientific text-books. This leads on to acoustics, and we are at once plunged rather abruptly into the subject of musical intervals. The theory of consonance, the cause of the intensity of sound and its mode of propagation make up the novel arrangement of this chapter. Optics occupies the sixth division, and is carefully treated. Especially noteworthy is the chapter on the theory of the absorption and dispersion of light, in which there is an excellent account of spectrum analysis. The part before us breaks off in the discussion of physiological optics, where Helmholtz's researches are in part developed. It is a pity that the engravings are not equal to those generally found in continental text-books.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Jacamar in Britain

I SEE, in your review of Mr. Cordeaux's "Birds of the Humber District," mention of a Jacamar—I presume a Galbula—having been shot by a keeper named S. Fox, near Gainsborough, in 1849. You and the author, Mr. Cordeaux, naturally remark on the "extraordinary" puzzle of the fact.

As one who has often seen the Jacamar in its own tropic forests, and watched its flight and its feeding, I must be allowed to suspect some mistake, unless the most "startling"—in every sense of the word—evidence of the authenticity of the specimen is given.

Ready to believe everything, in such a world of wonders, I might have believed in a Jacamar being blown to south-west Cornwall, Ireland, or Scotland. But in the eastern counties—"Qu'allait il faire dans ce galere là?"

Harrow, June 6 C. KINGSLEY

The Use of Wires in Correcting Echo

[The following letter has been forwarded to us by Mr. J. J. Murphy]:—

Palace, Cork, May 30, 1873

My Dear Mr. Murphy,

Having seen in the newspapers some notices of the use of wires for correcting the echo by breaking the waves of sound in churches and public buildings, we were anxious to try the experiment in the cathedral of St. Fin Barre, Cork, the nave of which is of great height, between 60 and 70 feet, and narrow in proportion to its height. We were unable to obtain any reliable information as to the placing of the wires, so that what we did was very much in the way of experiment. I should state that the desks for the officiating clergy and the choir are placed at the intersection of the transepts, nave, and chancel, so that this may be regarded as the point from which the sound starts. The organ is placed in a gallery at the west end, and the organist seated in this gallery has always heard much more distinctly than the people sitting about two-thirds down the nave, particularly those close to the pillars; but the echo seemed to render the sound indistinct, more especially in the transepts, the north and south walls of which presented a large flat surface, and appeared to us to be probably the source of the echo.

At first we tried the wires strained at a considerable height, the level of the triforium, but they produced comparatively little effect; we then strained a double course of wire at about a height of 12 or 15 feet round the large piers of the central tower, so as to encompass the choir, and other wires completely across the nave and side aisles, and the effect was certainly very good. There was a greater distinctness of sound throughout the building. Our organist, who is a very accomplished musician, did not know that the wires were put up, and remarked to me one day after service that he did not know what it was, but that everything seemed to him in better tune.

This encouraged us to make further experiments. We then strained three wires completely across from the south wall of the south transept to the north wall of the north transept, so as to pass over the heads of the choir, but the effect was quite too great, it seemed to kill the sound, every sound seemed to stop at once, all resonance was gone. These wires we had at once to take down, and I should add that, as regards the organist, the wires over the heads of the choir seemed to produce a much greater effect than those directly between the choir and his seat; it appeared to him as if he had a bad cold and could not hear distinctly.

These wires appeared to prevent the voices rising and filling the cathedral. It seems very difficult to determine where to place the wires so as to produce a really good effect; but that they have a very great effect far beyond what one would have supposed, *a priori*, is admitted by all who have taken an interest in the matter here. Several members of the congregation have remarked that they heard better in the cathedral now, without knowing the cause. We have used very thin wire; a stranger would not perceive it unless his attention were called to it. We hope to make some further experiments especially with regard to the transepts of the cathedral.

The inexpensive nature of the experiment and the important result likely to be obtained make this a matter of great importance, independently of the great interest it possesses in a scientific point of view.

I may add that when in Dublin I attended Divine service in St. Andrew's Church, and having officiated in the church at different times I am well aware of the difficulty of filling it in consequence of the echo, but the use of the wires appeared to have made a very great difference, as I heard most distinctly. It seemed to me, however, that a far greater number were used, than my experience in Cork would have led me to suppose were necessary.

I hope this subject will receive the attention which it deserves.

J. J. Murphy, Esq. ROBERT S. GREGG

Fertilisation of the Wild Pansy

THERE are two points in the structure of the heartsease (*Viola tricolor*) which are not mentioned in Mr. Bennett's interesting article on its fertilisation, but which, I think, deserve notice.[*] The first of these is the lip of the stigma, which closes the entrance to the spur and must be pushed back by an insect trying to reach the nectary, thereby bending down the head of the stigma, so as to sweep any pollen that may be adhering to those parts of the insect which come into contact with it into its receptacle; while, in withdrawing, the insect necessarily presses against the lower side of the lip, and raises up the whole stigma, thus rendering self-impregnation impossible, or at least highly improbable. Modifications of the same contrivance may be seen in many other flowers, *e.g.* *Pinguicula*, *Iris*, &c.; it reaches, perhaps, its greatest perfection in *Mimulus* and *Bignonia*,[†] where, to the usual mechanical disposition of the parts, there is added irritability of the stigmatic lobes, which close together spontaneously when touched, expanding again after a while, if not already pollenated.[‡]

The second point to which I have alluded is the close, hairy lining of the fore part of the spur, forming a narrow groove at the base of the lowest petal.[§] This groove generally contains a quantity of pollen that has fallen from the overhanging anthers. There is also a small tuft of hairs at the base of each of the lateral petals, arching over the essential organs, and forcing an insect to approach the nectary from below. These lateral tufts are present, I believe, in all the violets, but *V. tricolor* (including therein several sub-species) is the only British species which has the spur lined with hairs, as well as the only one not known to bear self-fertile cleistogenous flowers.

Although the flowers of the wild heartsease are quite scentless to our blunt organs, does it follow that they are necessarily so to an insect's far more delicate sense?[*] Some of the cultivated pansies are very sweet, and I am not aware that this quality has ever been made an object for selection by florists. These large garden pansies are much frequented by *Bombus muscorum*, which may be watched while performing the act of pollenation, as described by Prof. Hildebrand. W. E. HART

Kilderry, Co. Donegal

P.S.—Mr. Farrer, in writing of *Lotus corniculatus* (NATURE, vol. vi. p. 499), says:—"Five of the stamens, viz., those of the inner whorl, are shorter than the others, and their filaments are dilated at the top." Here Mr. Farrer's usually accurate pen seems somehow to have made a slip. It is the long outer stamens, those opposite the calyx-teeth, which have their filaments thus curiously modified for the purpose there explained.

Fertilisation of Orchids

MR. DARWIN, in his "Fertilisation of Orchids," speaks of a Madagascar orchid (*Angræcum sesquipedale*) with nectaries 11½ inches long, and supposes that these plants must be fertilised by the efforts of huge moths, with probosces capable of such expansion, to obtain the last drops of the nectar which is secreted in the lower part of these whip-like nectaries. Can any of your readers tell me whether moths of such a size are known to inhabit Madagascar? They would probably be Sphingidæ of some kind, as no other moths would combine sufficient size and length of proboscis. W. A. FORBES

Culverlea, Winchester, June 2

Ground Ivy

I HAVE this spring found, in many different places, specimens of ground ivy, having flowers with undeveloped stamens. They seem generally, though not always, to be on different plants from those bearing perfect flowers, and below the average in size, the tube being more slender. Also, in nearly all my specimens, the stigmas diverge in a more or less horizontal direction (across the flower) instead of remaining open in the usual vertical one. Is this second form of the flower common? and if so, may not the greater tendency to horizontal divergence compensate for the want of stamens, by bringing the stigmas into the position most favourable for receiving from an insect any pollen which a previous visit to a perfect flower may have left on its head or back? S. S. D.

Hail Storm

DURING the passage across us this afternoon of a thunderstorm moving at so great a distance above the earth that the thunder was very feeble and the lightning very faint, we had a great hail storm, which commenced with conical-shaped opaque stones of the size of peas, at $4^h\ 27^m$ (only lasting one minute), beginning again at $4^h\ 29^m$ with circular transparent stones having a small opaque nucleus (again only lasting one minute), followed at $4^h\ 33^m$ with flattened stones of the form of common acid drops, transparent, except a thin opaque envelope (which soon melted), and having *externally* in the centre a small rugged piece of ice. The size varied from two to three inches in circumference, and the force with which they fell cut off the leaves from the trees and broke 200 panes of glass in my greenhouses. These stones continued to fall for seven minutes with very heavy rain.

Twelve hailstones were gathered after the storm was over, and on being melted yielded 0·060 inch of water when measured in the glass of an eight-inch gauge, and the amount caught within an eight-inch hoop measured 0·750 of an inch, and this added to the rain, gave 1·430 inches as the amount fallen during the storm. E. J. LOWE

Highfield House Observ., Nottingham, June 3

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[*] Both these points have already been described by Prof. Hildebrand ("Die Geschlechter-Vertheilung bei den Pflanzen," p. 53). Unfortunately, I have not the works of Sprengel and Hermann Muller to refer to.

[†] I have had no opportunity of examining the latter, but from the published descriptions it seems to correspond in its main features with *Mimulus*, of the process of fertilisation of which a full account has been given by Mr. F. E. Kitchener in the *Journal of Botany* for April.

[‡] When it becomes necessary to introduce a new word into the language it is always well to select the most appropriate that offers itself. Some time ago Mr. A. W. Bennett wrote to the *Journal of Botany* (vol. ix p. 118), asking for suggestions for a better rendering of the German word *Bestäubung* than "be-pollenment" or "pollenization." I afterwards (*Journ. Bot.*, vol. x, p. 25) proposed the term "pollenation," which has since been accepted by Mr. Bennett. He, however, continues to use the verb to "pollenize." Now, if I might be allowed the space, I should like to state my reasons for objecting to this expression—(1) The root *pollen* is Latin, while the termination ιζω is Greek. Of course, this objection is over-ruled by common usage, and by itself would go for nothing. (2) The word "pollenize" does not in its structure convey the idea intended. *Bestäuben* means to "sprinkle with dust," to "dust with pollen." The termination "ize," on the other hand, gives the signification of change or conversion; thus to "pollenize" would naturally mean to "pulverise," to "turn to flour or pollen," and might be correctly applied to the processes going on in the substance of the anthers, but not, without violence to grammar, to the application of pollen to the stigma. Numerous precedents might be cited for the use of the word "pollen," unaltered, as a verb, from which would be derivable either "pollenation" or "pollenment," but this would be at the risk of offence to ears scientific. The same objection would apply with still greater force to the word "be-pollen." "Empollen" is more euphonious, but would convey a slightly different meaning. On the whole, the word that I have used in the text is the best that I can think of. Perhaps some of your more classical readers might give us their opinions.

[§] Morphologically speaking, this is the uppermost petal, which, by the bending of the peduncle and consequent inversion of the flower, is made to assume the position best fitted to afford a convenient landing-place for insects.

[*] The flowers of *V. palustris*, which are nearly unicolorous with a few dark lines pointing to the nectary, are apparently scentless; but after standing for a short time in water in a warm room, they become quite sweet.

THERMO-ELECTRICITY *

II.

GUIDED by considerations of Dissipation of Energy, I was led some years ago to the hypothesis that specific heat of electricity must be, like thermal and electric resistance, directly proportional to the absolute temperature. If this were the case, the lines in the diagram would be straight for all metals; and parabolas would be the graphic representation not only of electromotive force, but of the Peltier effect, in terms of the temperature of a junction. And I found by actual measurement of curves plotted from experiment, that, within the range of mercury thermometers, the curves of electromotive force for junctions of any two of iron, cadmium, zinc, copper, silver, gold, lead, and some other metals, are parabolas with their axes vertical; the differences from parabolas being in no case greater than the inevitable errors of experiment and the deviation of mercury thermometers from absolute temperature. If, then, the line for any one of these metals be straight within these limits of temperature, so are those of all the others. This makes the tracing of the diagram within these limits a very simple matter indeed. And an easy verification is furnished by the fact that from the parabolas for metals A and B, and A and C, we can draw the lines for B and C, assuming any line for A; and we can then compare the temperature of the intersection of these lines with that of the neutral point of B and C as found directly. Another verification is supplied by the tangents of the angles at which these parabolas cut the axis of abscissæ, for the sum of two of them ought in every case to be equal to the third.

In fact, if we assume, in accordance with what has been said above,

$$\sigma_1 = k_1 t, \quad \sigma_2 = k_2 t,$$

where k_1 and k_2 are constants, Thomson's formulæ give at once

$$\frac{\Pi}{t} = -\int (k_1 - k_2) dt,$$

or

$$\Pi = (k_1 - k_2)(T_{1,2} - t)t$$

where $T_{1,2}$ (the constant of integration) is obviously the temperature of the neutral point.

Also

$$E = J\int \frac{\Pi}{t} dt = J(k_1 - k_2)\int (T_{1,2} - t)dt$$
$$= J(k_1 - k_2)(t - t_0)(T_{1,2} - \frac{t + t_0}{2})$$

where t_0 is the temperature of the cold junction. This is the parabolic formula already mentioned.

Comparing with the parabola as given by observation we get the values of $k_1 - k_2$ and $T_{1,2}$. Similarly we obtain $k_1 - k_3$ and $T_{1,3}$. Hence we may calculate $k_2 - k_3$, and (by the second equation above) the value of $T_{2,3}$ from the relation

$$(k_1 - k_2)T_{1,2} + (k_2 - k_3)T_{2,3} + (k_3 - k_1)T_{1,3} = 0.$$

Thus we have the means of verification above alluded to—for the equation just written expresses the relation between the tangents of the angles at which the three parabolas cut the axis of abscissæ.

[It is to be remarked that if the circuit consist of one and the same metal, we have

$$k_1 = k_2, \; T = \infty, \; (k_1 - k_2)T = \tau \text{ suppose,}$$

whence

$$\Pi = \tau t,$$

which shows that the electric convection of heat may be regarded as an infinitesimal case of Peltier effect between adjacent portions of the same metal at infinitesimally different temperatures.

Also, on the same hypothesis, we have

$$E = J\tau(t - t_0)$$

which seems to accord with the result of some experiments made for me by Mr. Durham, in which the deflection due to the contact of the hot and cold ends of the same wire was shown to be proportional to the difference of temperatures and independent of the actual temperature of either.]

Endeavouring to extend the investigation to temperatures beyond the reach of mercury thermometers, I worked for a long time with a small air-thermometer, of which the principle was suggested to me by Dr. Joule. But this involved very great experimental difficulties, due mainly to chemical action at high temperatures; and, after much unsatisfactory work, I resolved to make one thermo-electric junction play the part of thermometer in observing the indications of another. In fact, an exceedingly elegant result follows at once from the preceding formulæ, if we suppose the specific heat of electricity to be proportional to the absolute temperature in each of four metals, and then draw a curve whose ordinate and abscissa are the simultaneous galvanometric indications of pairs of these metals, with their hot and cold junctions respectively at the same temperatures. For if τ be the difference of absolute temperature of the junctions, we have

$$x = A\tau + B\tau^2$$
$$y = C\tau + D\tau^2$$

where the four constants depend upon the nature of the metals and upon the absolute temperature of the cold junction. These equations give

$$(Dx - By)^2 = (CB - AD)(Cx - Ay).$$

which is the equation of another parabola, also passing through the origin, but with its axis no longer vertical.

A simple proof of this theorem is furnished by the motion of projectiles in vacuo. Suppose a particle to move under gravity, and subject, besides, to another constant force parallel to a given horizontal line—its path would have both ordinate and abscissa parabolic functions of the time. But its path might also be found by compounding into one the two accelerations, and as each of these is constant in direction and magnitude, their resultant will have the same property, and thus the resultant path is a parabola. Tried in this way through ranges of temperature up to a red heat, I found that while some pairs of circuits gave excellent parabolas, others were far from doing so, sometimes in fact giving curves with points of contrary flexure. I was on the point of recurring to the air-thermometer, when I noticed that in nearly every case in which the curve was not a parabola, iron was one of the metals employed; and, by the help of some alloys of platinum, I was enabled to get an idea of the true cause of the anomaly, and afterwards to verify it by an independent method. The cause is this, that while, as Thomson discovered, the specific heat of electricity in iron is *negative* at ordinary temperatures, it becomes *positive* at some temperature near low red heat; and remains positive till near the melting point of iron, where it appears possible, from some of my experiments, that it may again change sign. Thus the line for iron, straight at ordinary temperatures, passes downwards from the first quadrant to the fourth, and thence rises into the first again.

To recur to our analogy, an income represented by the iron line is one which for a number of years steadily diminishes, reaches a minimum, and then steadily increases. If this be associated with a steady expenditure, the fluctuations of capital will depend upon the comparative values of the expenditure and the minimum income. If the expenditure be less than the minimum income, the capital will go on increasing slower and slower to a certain point, then faster and faster; there will be no stationary point, but there will be a point of contrary flexure. If the expenditure be just equal to the minimum income, the point of contrary flexure will be also a stationary point. If the expenditure be greater than the minimum income there will be a maximum of capital, then a point of contrary flexure, and then a minimum; the maximum and minimum being the stationary points corresponding to the two occasions on which the expenditure equals the income. The maximum and minimum will obviously be farther apart, and smaller, the larger is the expenditure compared with the minimum income.

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* Abstract of the Rede Lecture, concluded from p. 88.

The latter part of these statements is well exhibited by the behaviour of circuits of iron, and various alloys of platinum with Iridium, Nickel, and Copper.

[Some of these, involving two, and in one case three, neutral points, were shown.]

In each of these cases there are obviously two neutral points, at least. Now suppose the two junctions raised to the temperatures of these two neutral points respectively, and we have a thermo-electric current maintained *entirely* by the specific heat of electricity, as there is obviously neither absorption nor evolution of heat at either junction. Still further, suppose (as is *very nearly* the case with one of the alloys I have just used) that the specific heat of electricity is *null* in the metal associated with iron, and we have the very remarkable fact of *a current maintained in a circuit, without absorption or evolution of heat at either junction or in one of the metals, but with evolution of heat in one part of the second metal and absorption in another part.* This suggests immediately the idea that iron becomes, as it were, a different metal on being raised above a certain temperature. This may possibly have some connection with the Ferricum and Ferrosum of the chemists; with the change of magnetic properties of iron, and of its electric resistance, at high temperatures. Dr. Russell has kindly enabled me to verify these properties in a specimen of pure iron prepared by Matthiessen. I find similar effects with Nickel at a much lower temperature. The method of control which I employed to satisfy myself that these peculiarities are due to iron and not to the platinum alloys, requires a little explanation. It depends upon the fact that by the help of two metals made into a double arc (wires of the two being stretched side by side, without contact except at the ends) we can explore any portion of the field between the lines for these two metals by simply altering the ratio of the resistances in the two parts of the double arc. Such a complex arrangement gives a line passing through the intersection of the lines of the two constituents, and depending for its position on their relative resistances. I shall not, at this stage of my lecture, trouble you with the formula which gives the line for the double arc in terms of the resistances of the two metals and their lines, but simply show the experiments with the help of a gold and a palladium wire, the one having the specific heat of electricity positive, the other negative; while their neutral point is considerably below the temperature of the room. Between their lines is included the peculiar portion of the iron line, and by making shots at it, as it were, in various directions from the neutral point of gold and palladium, we shall be able to study its bearings.

[Several of these experiments were shown, till finally the gold wire was melted.]

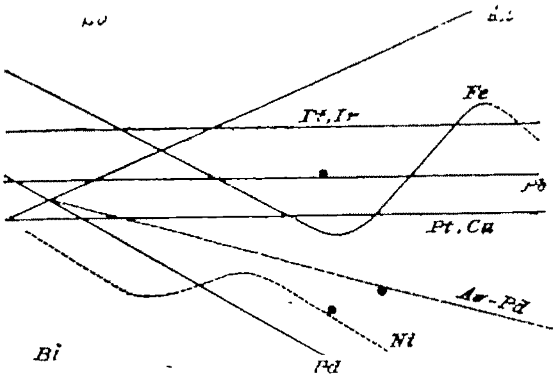
I have here wires of iron, gold, and palladium, bound together at one end, which is to be the hot junction. One end of the galvanometer coil is connected with the free end of the iron wire, the other slides along a long copper wire which connects the free ends of the gold and palladium wires. By sliding it towards either I diminish the resistance of that branch of the double arc and increase that in the other—*i.e.* I give that branch of the double arc the greater importance in the combination.

Throwing the greater part of the resistance into the palladium branch, I find a neutral point at a moderate temperature, but I cannot reach a second without melting the gold. Throw more resistance into the gold, the first neutral point occurs at a higher temperature than before; but a second is attainable. By still further increasing the resistance in the gold the two neutral points gradually approach one another, one rising in temperature the other descending, until at last we reach a maximum-minimum, the result of the confluence of the two points. The line for the double arc is now such as to *touch* the iron line. Still further increase the resistance of the gold, and we find a mere point of inflexion, the galvanometer indications having constantly *risen*, though at a retarded and then accelerated rate, during the heating of the junction.

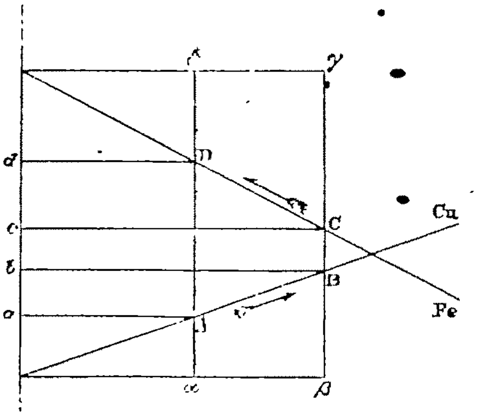
Two of the platinum alloys which I employed with iron seem to give lines almost exactly parallel to the lead line—*i.e.* in them the specific heat of electricity is practically *nil*. When a circuit is formed of these alloys the current therefore depends upon the Peltier effects at the junctions alone, and is sensibly proportional to the difference of their absolute temperatures, thus furnishing a very convenient thermometer for the approximate estimation of high temperatures. I am at present engaged in drawing the thermo-electric diagram in terms of temperatures as given by this combination, and the reduction to absolute temperatures will finally be effected by a comparison of this temporary but very convenient standard with an air-thermometer.

P. G. TAIT

NOTE.—The following rude sketch of a part of the thermo-electric diagram will perhaps render some of the preceding remarks more intelligible. It is drawn to illustrate qualitative effects alone.



The following diagram exhibits the amount of the Thomson and Peltier effects, and of the electromotive force, in a copper-iron circuit, the temperatures of both junctions being under that of the neutral point.



Peltier effect at cold junction = Area ADda (heating)
" " hot " = " BCcb (cooling)
Thomson effect in Copper = " $AB\beta\alpha$ "
" " Iron = " $DC\gamma\delta$ "
Electromotive Force = " ABCD

The arrows show the direction of the current; and Euclid's proposition as to parallelograms about the diagonal of a parallelogram shows at once the application of the first law of Thermodynamics to the figure, as the Electromotive force together with the Peltier effect at the cold junction obviously amount to the sum of the two Thomson effects and the Peltier effect at the hot junction.

Also, if we suppose the lines A D, B C, to be very close to one another, since we have always A D = $\frac{\Pi}{t}$ we get $(BC-AD)t = t\delta\left(\frac{\Pi}{t}\right) = -(\sigma_1 - \sigma_2)\delta t$, whose application to the second law is obvious. The reader may easily construct for himself diagrams for other cases of relation of the temperatures of the junctions to that of the neutral point.

Thomson's original paper will be found in the *Transactions of the Royal Society of Edinburgh*, and farther details of my experimental work in recent numbers of the *Proceedings* of the same society. I may avail myself of this opportunity of asking assistance from men of science in procuring wires or foil of the more infusible metals, such as Cobalt, Chromium, Tungsten, &c.

P. G. TAIT

THE LAW OF STORMS DEVELOPED*

I.

METEOROLOGISTS tell us that their science is as old as Aristotle. If we should judge by its progress up to the middle of the present century, its antiquity furnishes little to boast of; for, in the long lapse of centuries, it must have proved an incorrigibly dull scholar. Within the past few years, however, it has greatly improved, and, especially since it became identified with the popular and important systems of storm-warnings and weather-forecasts, it has been rapidly developed. This is peculiarly the case in America, and it is not wonderful, when we consider the comprehensive observations of our meteorological bureau, and the many beautiful phenomena which its publications disclose.

If Vasco Nunez, the discoverer of the great South Sea, was so awed by the grandeur and expanse of its waters, as seen with the naked eye, how much more may we be impressed as telegraphic meteorology enables us to discover, at a glance, the tossings and undulations of the aërial ocean over the larger part of the hemisphere!

It is to some of the deductions, that may be justly made from the extensive and synchronous observations of the modern weather-systems, as they bear upon those weather-problems, which, from time immemorial, have interested mankind, that we now ask attention.

Until the year 1821, "the law of storms," simple as it is, was unknown to the most profound meteorologists and expert seamen of the world. It was then first discovered and announced by Mr. William C. Redfield, of New York, and established by the labours of that great mind, against the constant perversions and opposition of the scientific empirics of his day. It can be easily comprehended in its great outlines, and as far as our present purposes require. It assumes nothing, supposes nothing; but, from thousands of actual and actually recorded observations, presents the phenomena of spiral currents of air seeking a common centre of depression, and, in the attempt to find that centre, acquiring a vorticose or rotatory motion. The direction of this rotation Mr. Redfield found to be uniformly, in our hemisphere, contrary to that of the hands of a watch, with its face turned upward; and, in the Southern Hemisphere, the rotation is with those hands, or with the sun in its diurnal round. It is easy to see that, if the atmospheric column, resting over any given area of the earth's surface, should, from any cause, be suddenly diminished, or its pressure and intensity be reduced, the gaseous fluid would rush in from all surrounding regions to restore the disturbed equilibrium; and if the earth was not whirling around on its axis, every particle of the centre-seeking air would endeavour to move on the shortest, or the straight line. It is known, from the principles of mechanics, that this endeavour can never strictly be executed, because the axial rotation of the globe incessantly so acts as to throw every body, while in motion, in our hemisphere, to the *right* of the line on which it is moving, no matter whether that line be from east to west, north to south, or at any conceivable angle with the meridians or the equator. Obeying, in part, this tangential impulse, every particle of wind must take up a resultant motion. If it begins to blow toward the depressed centre of the storm as a north wind, it trends to the west, and is felt as a northeaster; if it begins as a south wind, it diverges as a southwester; if as an east wind, it becomes a southeaster; and, if as a west wind, it soon changes into the boreal northwest wind.

It has often been asked whether the storms of our latitudes attain the immense size formerly attributed to them; and many eminent writers have denied the possibility of their reaching a diameter of more than two or three hundred miles. Mr. J. K. Laughton, in his recently-published "Physical Geography," would have us believe that cyclones "do not attain the enormous magnitudes which have been assigned them." But this opinion rests merely upon conjecture, not yet upon a correct physical theory.

It is a well-known fact that the monsoons generated on the central plateau north of the Himalaya Mountains, and the whole system of Asiatic wet monsoons, may be regarded as an immense and prolonged cyclone; extend their "backing" influence into the Indian Ocean, and reach far to the south, through more than forty degrees of latitude (a radius of 2,500 geographical miles), and from the 60th to the 140th meridian of east longitude, far out into the Pacific, beyond the Bonin and Ladrone Islands, southeast of Japan. The whole system of wet monsoons may also be justly regarded as a grand cyclone, whose centre is stationary over the heated plains of Central Asia, whose intro-moving winds, bearing the evaporations of the Asiatic seas and oceans, feed it with meteoric fuel for six months in the year, and whose periphery may be regarded as embracing nearly one-third of the entire eastern hemisphere. Analogy, therefore, warrants the idea of a great cyclone. But, apart from all this, actual observations in different parts of the globe prove the frequency of storms of enormous magnitude. Thus, in the celebrated Gulf-stream storm of 1839, as Sir David Brewster long ago pointed out, several staunch merchantmen were foundering off the coast of Georgia, near Savannah, in the very heart of the gale, at the same hour that the winds in its north-west quadrant were taking the roofs off houses in New York and Boston, more than 800 miles distant—clearly revealing a cyclone whose formation was symmetrical, and whose diameter must have been nearly 1,300 miles. But, not to go back to old data, the West-Indian storm of August 18, 1871, before its centre had moved north of Florida, had begun to draw upon the regions of high barometer in the Northern States, had exerted its influence as far north as New London, Connecticut, and gave us the north-easterly cyclonic winds in the north-west quadrant of the whirl, on the entire Atlantic coast. The more furious cyclone of August 24, 1871, discovered to be then south-east of Florida, and telegraphically fore-announced as likely to endanger the coasts of the Southern

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* From the *Popular Science Monthly*. Communicated by the author, Prof. Thompson B. Maury, of the Signal Office, Washington.

States in less than forty-eight hours, appeared on the 26th in full force in Northern Florida, but not until some eight or ten hours after it had set the atmosphere all around it (as far north as Boston) in cyclonic motion, and had caused the storm-cloud to spread itself over the entire region of the United States on the eastern slopes of the Alleghanies, and as far westward as Knoxville, Tennessee. It is no uncommon thing, as Redfield, Espy, Henry, Loomis, and others, long ago showed, for an area of depression on the upper lakes to make itself simultaneously felt as far south as the Gulf of Mexico, and as far east as New England.

If it fell within the scope of the design of this paper to consider the final cause of storms, it would be easy to show that, unless the law of storms ordained a large area, and a far extended path for the meteor, in some degree commensurate with the area of our immense continent, the meteor could not fulfil its office in the terrestrial economy—an office which, apparently, imposes upon it the task of gathering to its centre, through the agency of its intro-moving winds, the idle and inappreciable moisture scattered over the surface of the earth, condensing it into rain and snow, and diffusing it in these forms over immense districts of country.

It is of incalculable importance to observe, and carefully digest the fact, that when a storm-centre or area of low barometer is once formed, it is the nucleus for a vast aggregation and marshalling of meteoric forces. No matter how small at first, under favourable atmospheric conditions, the *courant ascendant* is formed, condensation aloft sets in, and the precipitation only serves to add "fuel to the flame" of the cyclonic engine. This process widens in geographical area, and after a few hours have elapsed, the storm may so develop as to cover a continent with its portentous canopy of cloud, while simultaneously strewing an ocean with wrecks, and throwing out in the upper sky, more than a thousand miles in its front, the fine filaments of the premonitory cirrus and cirronus.

In close connection with the size and magnitude of cyclones must be considered the distance over which they pass from their initial point. Much has been said on this part of our subject, and not a few writers have accepted the doctrine of Admiral Fitzroy, that they progress over but comparatively short distances. For such a view, however, it is impossible to find, either in the nature or physical office of the cyclone, any support whatever. The storm once engendered, no matter in what part of the world, may be stationary or progressive. There are well-authenticated instances of almost stationary cyclones and almost stationary typhoons, of which latter will be remembered the famous gale of the ship *Charles Heddle*—an Indiaman, carried round and round the storm-centre for five days—which progressed not more than 90 miles a day. Indeed we may, as has been said, regard every wet-monsoon region as a stationary and semi-perennial cyclone. Such a meteor has been shown to resemble an eddy moving in the current of a rapid river. The latter may be large or small, while it does not determine, but is determined by, the course of the on-flowing stream. It is true the centre of an eddy or water-hollow may soon be filled up and the whirl disappear; but it is because the depression is not maintained. If the depression could be maintained, it is easy to see that the eddy would continue, and pursue its way, as long as the current in which it is embodied continues to flow; it might be through the length of an Amazon or a Mississippi River. In the case of a cyclonic eddy or whirl, we know the atmospheric depression is maintained as long as the centre moves in a region sufficiently supplied with aqueous vapour to feed it. It is a physical impossibility, as has been often shown, that any storm, however vast or however violent, can prolong its advance or sustain its fury over a dry and desiccated surface. The most extended typhoons of the East, upon entering the dry and rainless continental regions, dwindle into the well-known and diminutive dust-whirlwind, such as Sir S. W. Baker describes as witnessed in Nubia, and as here illustrated, from the admirable pages of Mr. Buchan. The Sahara is a more formidable barrier to the passage of a storm than the majestic mountain wall of the Alps, and the simoom is, notwithstanding the stories of travellers and the legend of swallowing up the army of Cambyses on the African desert, a wasted and worn out cyclone. In his "Desert World," Mangin, compiling the more accurate observations of the phenomenon, says: "It never prevails over any considerable area, and beyond its limits the atmosphere remains serene and calm; the phenomenon is of brief duration, the atmospheric equilibrium is speedily restored; the heavens recover their serenity; the atmosphere grows clear, and the sand-columns, falling in upon themselves, form a number of little hills or cones, apparently constructed with great care, like those mimic edifices of sand made by children in their pastime." The same writer also mentions a severe simoom which was "over in a couple of hours."

Embedded in the great aërial currents, however, and supplied with abundance of moisture, there is nothing to arrest either the rotatory or progressive movements of the storm. Like the drift-bottles cast upon the current of the ocean, and found after months to have been carried thousands of miles, from the equatorial to the polar parallels, there is every reason to suppose the tropic-cradled gale, and the minor storms also, are borne in the great atmospheric currents through quite as great distances. There is an authentic and well-attested account of a Japanese junk, lost or deserted off Osaka, drifting through the immense arc of the Kuro Siwo's recurvation, and encountered (in latitude 37°, by the brig *Forrester*, March 24, 1815) off the coast of California. That tiny craft must have followed in the bands of westerly winds and warm waters for seventeen months. Why, upon theoretical grounds, should we reject the hypothesis which represents the movement of storm-areas as prolonged for many thousands of leagues, or indeed that which represents them perpetually in motion around given centres of cyclonic or anti-cyclonic areas, keeping pace with the great winds in their eternal circuit?

As a striking corroboration of all this we find—what might have been assumed on theoretical grounds—that the logs and special observations of the Cunard steamships show that a vessel bound from Liverpool westward encounters frequent advancing areas of low pressure, indicating a number of rapidly succeeding barometric hollows or depressions, "each with its own cyclonic wind-system, moving across the Atlantic as eddies chasing each other down a river-current."

The word *cyclone* has frequently, but incorrectly, been used as significant of an enormous or very violent meteor, as if its application was to be confined to the devastating hurricane of the West Indies or the terrific typhoon of the China seas. It simply means a storm which acts in a circular direction, and whose winds converge by radials or sinuous spirals, toward a centre, moving in our hemisphere in the opposite direction to that of the hands of a clock, and in the Southern Hemisphere in a contrary direction. Taking this as the definition of a cyclone, it seems clear, from observation alone, that all storms are to be regarded as cyclonic. Volumes have been written to prove that this is not the case. But we have only to examine a few series of weather-maps from week to week to see that, wherever you have an area of low barometer, into its central hollow the exterior atmosphere from all sides will pour, and that in so doing a rotatory spiral or vorticose storm is generated. The tornado, the simooms, the dust-whirlwind, the fire-storm, even the slow and sluggish storm which moves on our western plains as the labouring wheel of the steamship buried in a heavy sea, all attest that a body cannot move on the earth's surface in a straight line. It is not more true with us that the Gulf-Stream turns to the eastward, the Polar Stream to the westward, and the equatorial currents to the northward, than that every air-current, in obedience to the same law, should turn to the *right* of the line along which from any cause it is called to move. The meteorist has therefore only to ascertain by observation where the barometer is lowest, to know at once the direction of the winds from the circumjacent districts, far and near, or at least to test the mathematical law by a grand experiment.

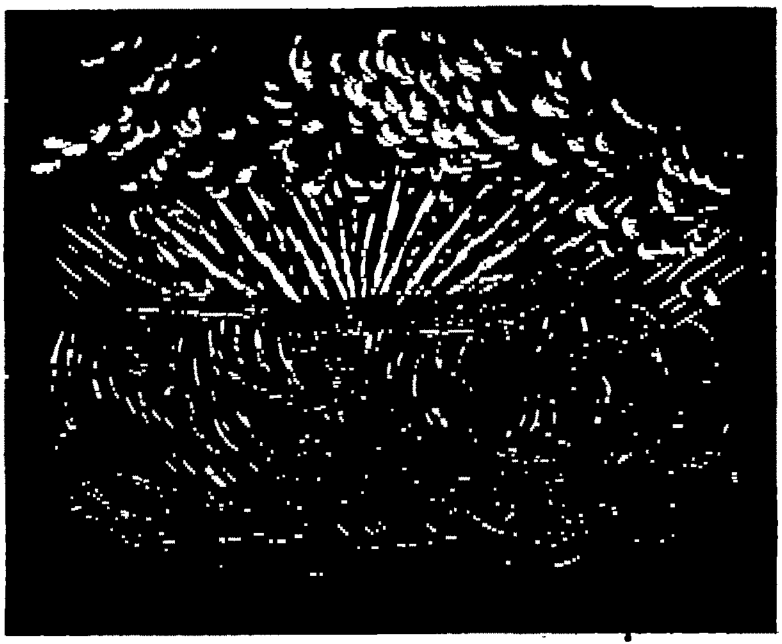


FIG. 1.—Cirrus and Cirrous Clouds.

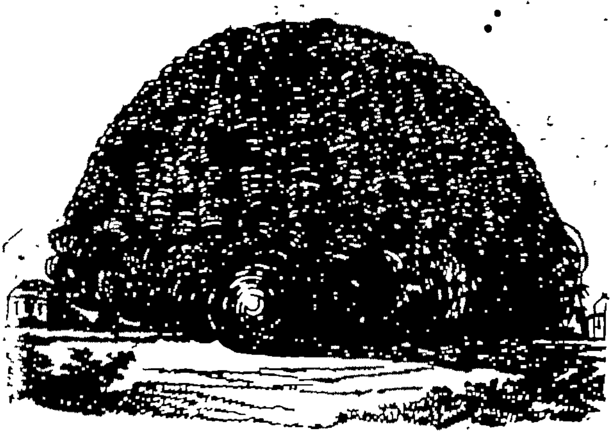


FIG. 2.—The Dust Whirlwind.

The tangential and centripetal forces, acting at the same time on any particle of air in the storm, may be equal or very unequal, and the cyclonic character of the gale may be well marked or partly concealed. In the tornado, with a diameter of only a few hundred feet, the tangential force may not be appreciable to an observer, but it is present, and intensely assists in communicating vorticose motion to the storm, whose roar is heard with awe by the stoutest heart, as it crashes through the forest and even ploughs up the soil of the earth. If the cyclonic or spiral feature should fail to manifest itself in any storm, we ought to look for such failure in the tornado. It is true that no barometric readings have ever been taken in the narrow heart of a tornado, but abundant evidence exists of the fearful rarefaction in the centre. While the meteor, once set in motion, may move forward with great velocity and destructiveness, the danger is clearly due to the intro-rushing and gyratory winds. There is not an instance, it is believed, recorded in which a tornado moved as much as 100 miles an hour; probably one-half that velocity would be too high an estimate for its usual and ordinary motion. But the wind, moving straightforward at the rate of 60 or 80 miles an hour, never worked anything like the disaster of a tornado. In the West-Indian hurricane, blowing at the rate of 100 miles an hour, houses have been blown down, ships innumerable stranded; but this is all mere child's-play compared to the suction and whirl of the tornado. The conclusion forced upon us is, that the ravages of the latter are due, not to the weight of the atmosphere, moving as a river-torrent in a straight line, nor to the rush of air behind the travelling vacuum, but to the torsive, racking motion—imparted to every object in its path—due to its gyration. To prove that this gyration is *always* from right to left, or against the hands of a watch, is, of course, practically impossible; but such a direction has often been observed in tornadoes.

It may, therefore, be safely concluded that, for all processes of meteorologic calculation, the disturbance, if not such at first, will soon become cyclonic. All daily weather-charts demonstrate this, not by a laboratory or lecture-room experiment, but on an infinitely wider and grander scale, and in a manner far more conclusive than any merely manual experiment could possibly make to appear. As Mr. Laughton has happily said, "Nature makes no distinction between small and great; the drop of mist that lights gently down on a delicate flower, and the avalanche that sweeps away a village, fall in obedience to one universal law."

(*To be continued.*)

THE CORONAL ATMOSPHERE OF THE SUN[*]

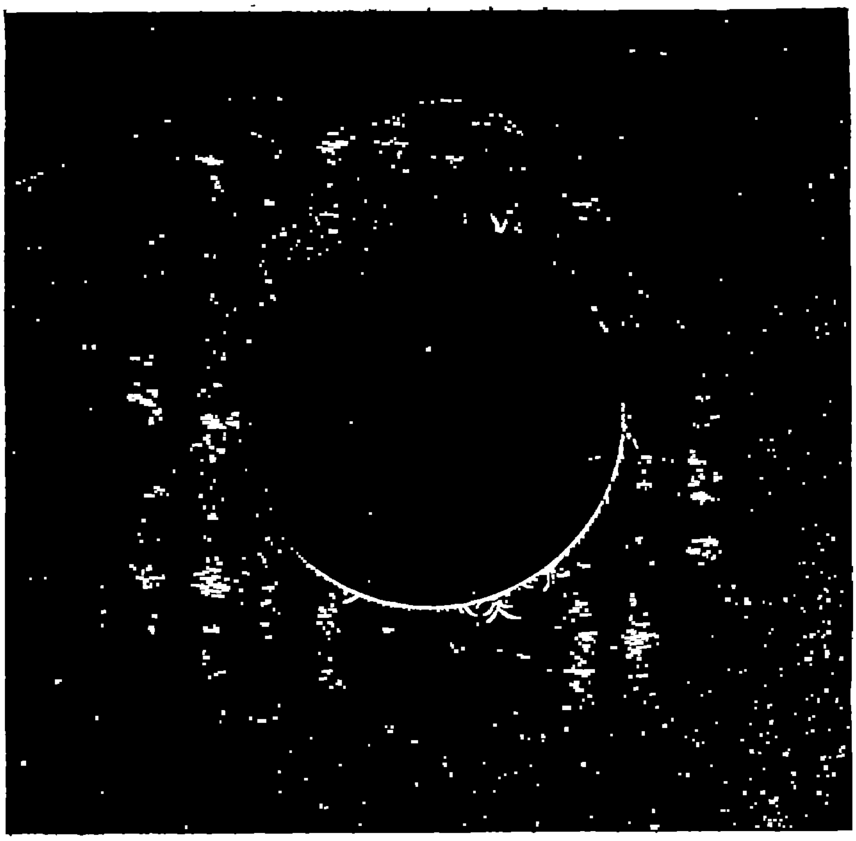
I.

I PROPOSE to bring before you rapidly the principal results obtained by me during the last total eclipse of the sun which I observed in Hindostan, at a point not very far distant from the place where I observed the great eclipse of 1868, which opened up such new horizons with regard to the constitution of the sun.

The last eclipse took place on December 12, 1871. The chief interest of the phenomenon is connected with the problem of the luminous corona which surrounds the sun during total eclipses. When that body is eclipsed by the interposition of the moon, you know that independently of those jets and luminous expansions which are known as protuberances, there is seen around the dark disc of our satellite a magnificent luminous phenomenon, resembling a glory or crown, which extends to 8′, 12′, 15′, and more from the lunar limb, and the frequent strange forms of which are variable at each eclipse. The observation of the eclipse which now occupies our attention, had for its object to definitely fix for us the nature of this singular phenomenon.

The corona is the luminous manifestation which is predominant during a total eclipse, and thus it must, at all times, attract the attention of observers. We possess, indeed, descriptions by Plantade, by Halley, by Louiville, and by others, which go back to the commencement of the 18th century; of course these observers did not indicate the cause of the phenomenon.

Arago and his school form a period in the history of the attempts which have been made to discover the nature of the corona. Our great physical astronomer applied the polariscopic methods to these investigations, but he as well as his successors were baffled. In the "Astronomie Populaire," published in 1856 (tome iii. p. 604), we read the following conclusion upon this subject: "I regret to say that the disagreement which has been found to exist between the observations made in different places by astronomers equally competent, on the luminous corona, in one and the same eclipse, has covered the question with such obscurities, that it is in the meantime impossible to arrive at any certain conclusion on the cause of the phenomenon."



By means of spectrum analysis the question has entered on a new phase. In 1868, while the nature of the protuberances was discovered, the spectrum of the corona was also obtained; it is true the observers found it continuous,[†] not an exact observation according to me, which retarded the solution of the question.

In the following year the Americans took up the matter.[*] They still found the continuous spectrum, but they established the existence of that celebrated green line (1474 in Kirchhoff's scale) which is the prevailing manifestation in the spectrum of the corona, and the meaning of which has yet to be discovered. We owe, moreover, to the Americans some very beautiful photographs of the protuberances, which show also the actinic power of the coronal light.

The eclipse of 1870 was marred by the bad weather. The few observations which could be made confirmed in general the observations of 1869.[†]

Thus, in 1871, we already possessed some very important data on the corona. Unfortunately these data were as yet incomplete, and above all inconsistent: for example, the continuity of the coronal spectrum, on the one hand, was inconsistent with the observations of polarisation of the corona, and on the other hand, led to the scarcely admissible conclusion of a corona formed of solid or liquid incandescent bodies. Thus the new eclipse, which presented a new opportunity of attacking this great question, the calculation of which, it was felt, must now be near, excited a general rivalry.

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[*] Translation of a paper read by M. Janssen at the Bordeaux meeting of the French Association for the Advancement of Science.

[†] Let us mention the observation of M. Rayet, who found luminous prolongation on the principal lines of the spectrum of a protuberance.

[*] The total eclipse of August 7, 1869, visible in N. America.

[†] We should mention, nevertheless, the beautiful observations of Mr. Young on the reversion of the lines at the base of the chromosphere.

England took the most considerable share in these observations. The [British Association, the] Royal Society, the Royal Astronomical Society, the Indian Government, worked harmoniously together. Among the noted men of science sent out, we shall mention specially Mr. Norman Lockyer, Colonel Tennant, Lieut. Herschel, Mr. Pogson, Capt. Fyers, &c. Italy was represented by M. Respighi, who was destined to make, on this occasion, some very beautiful observations; Holland by M. Oudemans, &c. At the request of the Academy and the Bureau des Longitudes, I was appointed by the French Government to represent France. It was a glorious charge for me, but at the same time a heavy one, which made me regret that circumstances did not permit of my having any French rivals.

The voyage being decided, it remained for me to settle the plan of my observations, the plan on which to set about to choose instruments, and to choose the place of observation. These points were of prime importance.

With regard to the plan of investigation, I knew very well that, coming after so many able men, I could not hope to solve the problem by simply adding to the numerous observations already made, a few similar observations. It was necessary to study the collection of known facts, to fix the obscure or contradictory points, and to secure a number of rapid observations (the totality would last only about two minutes in India) which should enable us to correct what was inaccurate, to complete what was insufficient, and to form, along with previous observations, a collection of data from which to deduce the true nature of the phenomenon. For example, I had no doubt, in spite of contrary observations, that the spectrum of the corona was not really discontinuous. I was persuaded that it must present, as a dominant characteristic, that of a spectrum of gas, and I found an explanation of the contrary appearances recorded in the feebleness of the light of the corona which did not admit spectra to be obtained, sufficiently luminous for discerning their true constitution. Thus, my intention was to bring my efforts to bear upon this chief point, to some extent the knot of the problem. The point was to obtain a spectrum much more luminous than those of my predecessors. For this purpose I constructed a special telescope having a mirror 37 centimetres in diameter, and a focus of $1^{\mathrm{m}}{\cdot}43$, which gave spectra about 16 times more luminous than those of an ordinary astronomical telescope.

I attached also great importance to seeing the corona at the same time as I analysed its light. A special arrangement of the finder enabled me to attain this end.

Finally, a polarising telescope placed upon the large telescope enabled me to join the polariscopic indications to the other data, and to judge of their agreement. Such were my instrumental arrangements.

The choice of a station was of no less importance. At the point at which we had arrived, our investigations bore upon phenomena so delicate that a sky was required of absolute purity, if I may be permitted the expression. Let us say a few words as to where I sought to realise this second condition.

The eclipse was to be total in the south of Hindostan, at Ceylon, Java, and Australia. Australia was too far away. Java is, in December, subject to the rainy monsoon. There yet remained India and Ceylon, which represented for the line of totality a very considerable extent, and offered a very great variety of stations from which to choose. To make this choice, I resolved not to trust to the general indications which we possess in Europe as to the climate of India, but to set out early, to visit all the stations, and to decide only after visiting the places, and collecting information on the spot.

I was at Ceylon by the beginning of November, nearly six weeks before the time of the eclipse, which would take place on December 12. On this island I was greatly assisted by the families Laggard and Ferguson, to whom I here beg to express my thanks. The information gathered in the north of the island, where the phenomenon would take place, was not so satisfactory as I desired, and it was agreed to seek for better fortune on the coast of Malabar. I then left Ceylon for Malabar, doubling Cape Comorin. On my way I made some magnetic determinations, and I had the good fortune to find that the magnetic equator, for the dip, passes quite close to Cochin. It was at Telecherry, an English post situated near the line of totality and the French colony of Mahé, that we disembarked. I was received by M. Baudry, a French merchant, who gave me a most gracious welcome and the most active assistance. Mahé was very valuable to me; our governor, M. Liotant, procured for me interpreters who spoke French and the dialects of the districts I was to traverse.

I had, meantime, to choose between the coast proper, the plain, and the stations of the Ghauts and the Neilgherries. As the eclipse was drawing near I could not think of sojourning at each station to make a lengthened investigation. I decided to utilise the telegraph and the railway * for making a simultaneous inquiry as to these stations. M. Baudry, whom I had instructed in observations to make every morning at the hour of the eclipse on the purity of the atmosphere at the coast, sent me these every day by telegraph. I had a similar station on the plain. The baggage had been taken to Coimbatoor, at the centre of the railway, ready to be conveyed speedily to the station selected. I myself visited the Neilgherries, and to gain time, I surveyed these mountains by utilising the night. The mass of information thus collected indicated the great superiority of the Neilgherries. A very careful investigation of this massive mountain-range induced me to locate my station in the north-west, where I had in fact much finer weather than in Dodabetta, one of the highest peaks, where Colonel Tennant and Lieutenant Herschel were afterwards established.

It was upon a mountain near Shoolor, an Indian village, lat. 11° 27′ 8″ N., long. 74° 22′ 5″ E. of Paris, that I fixed my observatory. The instruments were forwarded from Coimbatoor (at the foot of the Neilgherries) to Ootamacund in ox waggons. From Ootamacund to Shoolor the country consisted only of mountain and forest, without carriage roads, and the cases had to be carried on men's shoulders, the many difficulties attending which were happily overcome. Three days before the eclipse the observatory was erected, the instrument in place and ready for observation.

The observation at Shoolor was favoured by a sky of wonderful purity. As I have already indicated, my plan was to examine the corona from the triple point of view of its figure, its spectrum, and its phenomena of polarisation.

I first examined the corona in the telescope; the phenomenon was seen in all its splendour. The general form was that of a curvilinear square (*carré curviligne*), of which the outlines were irregular, but clearly defined. At its greatest height, the corona extended to about 14′ or 16′ from the lunar limb, and only to about half that distance at its narrowest parts. No diagonal was in the direction of the solar equator. All around the limb of the moon were seen trains of light which united towards the highest parts of the corona, and which gave to the entire phenomenon the appearance of a luminous and gigantic dahlia, the centre of which was occupied by the black disc of the moon.

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* There is a railway from Madras to the Malabar coast. I found it almost follow the direction of the line of totality.

The corona did not present any essential differences of structure at the point of contact and the opposite point. The motion of the moon did not appear to produce any change in the structure. These facts completely convinced me that the corona is a real object, situated beyond the moon, the gradual motion of which body reveals its various parts. Having finished this investigation, I turned my attention to the luminous elements of the phenomenon. My view being yet as distinct as ever, I commenced by examining the spectrum of the highest and least luminous parts of the corona. I placed the slit of the spectroscope at a point two-thirds of a radius from the moon's limb (*environ du bord lunaire*). The spectrum was seen much more vividly than I expected at that distance, a result evidently due to the luminous powers of the telescope and to the whole of the arrangements adopted. This spectrum was not continuous. I recognised at once the hydrogen lines and the green ray (1474).*

This is one point of the highest importance; I removed the slit, remaining always in the high regions of the corona; the spectra always presented the same constitution.

Starting from one of these positions, I descended little by little towards the chromosphere, examining very carefully the changes which might be produced. In proportion as I approached the moon, the spectra became more distinct and appeared richer, but they remained similar to the above in general constitution. In the middle heights of the corona, from 3′ to 6′ of arc, the dark line D was seen, as well as some obscure lines in the green; but these are at the limit of visibility. This observation proves the presence in the corona of reflected solar light, but it is seen that this light is drowned in an abundant extraneous (*étrangère*) luminous emission.

I then set myself to a very important observation, which I expected would give me the spectral relations between the corona and the protuberances. The slit was adjusted so as to take in a portion of the moon, a protuberance, and all the height of the corona. The spectrum of the moon was excessively pale; it appears due principally to atmospheric illumination, and gives a valuable idea of the feeble part which our atmosphere can play in the phenomenon of the corona.

The protuberance gave a very rich spectrum and one of great intensity; I had not time to make a detailed examination. The main point here is the establishment of the fact of the prolongation of the principal rays of the protuberance through all the height of the corona, which clearly demonstrates the existence of hydrogen in the latter.

The green line (1474), so vivid in the spectrum of the corona, appeared interrupted in the spectrum of the protuberance—a very remarkable result. I then gave a few moments to establish satisfactorily the exact correspondence of the lines of the corona with the principal lines of hydrogen in the protuberances.

There remained to me then only a few seconds for polariscopic observation.† The corona presented the characteristics of radial polarisation, and, it ought to be remarked, the maximum of effect is not observed at the lunar limb, but at some minutes from the edge.‡

I had scarcely finished this rapid investigation when the sun reappeared. JANSSEN

(*To be continued.*)

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* My spectroscope was fitted with a very exact scale, but it will be seen how I afterwards made use of the lines of a protuberance as a scale.

† To study polarisation, I have an excellent telescope excellently constructed of biquartz, by M. Prazmowski. This polariscope, placed upon and adjusted to the telescope, can be consulted in an instant.

‡ M. Prazmowski has noted this fact in his excellent polariscopic observations of the eclipse of July 17, 1860.

NOTES

THE subject of the Transit of Venus in 1874 was for the first time officially brought before the notice of the Board of Visitors at the recent Visitation of the Royal Observatory. After a careful exposition of the matter by the Astronomer Royal, and a consideration thereof by the Visitors, it was proposed and seconded by the Astronomical Professors of Cambridge and Oxford, that the Government be requested to provide the means of organising some parties of observers in the Southern Ocean, in the hope that they may find some additional localities for observing the whole duration of the Transit of Venus. In other language, they recommend strongly a sort of *roving* expedition. The meteorological and climatic difficulties both North and South are extremely great: the practical difficulties in the South are very peculiarly so; in despite of the latter, the Board of Visitors were unanimous in their advice to try what best can be done in the sub-antarctic regions. The Astronomer Royal expressed his perfect acquiescence in the proposal of the Visitors; the final decision will rest with the Admiralty and the Government. In coming to this decision, it is proper to add that the Board was in no degree either influenced or assisted by certain discussions which have taken place upon the subject out of doors; their decision would have been just the same whether these discussions had or had not taken place; and the Board came to their conclusion under a full knowledge of the very peculiar climatic and navigational difficulties which seem to attend on the roving expedition which they recommend. It is, in fact, only a realisation of an old proposal by the Astronomer Royal himself, which seems to have been set aside on account of the many serious practical difficulties attending it. The Astronomer Royal also proposed to organise some additional stations dependent on Honolulu.

MESSRS. SAMPSON LOW AND MARSTON are about to publish a volume on the subject of Arctic Exploration, by Mr. Clements Markham, entitled the "Threshold of the Unknown Region." It is intended to give a full account of all that is known of the line which, at present, separates the known from the unknown; to explain the best route by which the unexplored region may be examined; and to enumerate the important scientific results to be derived from Arctic exploration.

NATURALISTS will be glad to hear that the long-talked-of new buildings for the National Museum of Natural History, at South Kensington, have been actually commenced, and that the contractors, Messrs. Baker, have arranged to complete them within three years.

MR. F. T. WARNER, of Winchester, who for some time has been collecting materials for a Flora of Hampshire, has kindly offered the use of his collections and materials to Mr. Frederick Townsend with the proposal that he should complete the Flora. Mr. Townsend has accepted the offer, and as much work remains to be done, he invites the assistance of other botanists in furnishing him with lists of plants or in forming these during the present season. The value of lists will be greatly increased if accompanied by specimens, and in all cases exact localities and dates should be given. It is proposed to divide the country into river-basin districts. Letters should be addressed to Shedfield Lodge, Fareham, but parcels to Botley Station, London and South Western Railway. Mr. Townsend will gladly pay postage or carriage of parcels.

PROFESSOR ROLLESTON, of Oxford, is appointed to deliver the Harveian Oration at the Royal College of Physicians on June 25, at five o'clock.

IT is rumoured that Prof. Tyndall is to receive the honorary degree of D.C.L. from the University of Oxford during the ensuing Commemoration.

THERE will be an election at Magdalen College, Oxford, in October next, to not less than six Demyships and one Exhibition. Of the Demyships, one at least will be mathematical, one at least in Natural Science, and the rest classical. The Exhibition will be in Natural Science. The stipend of the Demyships is 95*l.* per annum, and of the Exhibition 75*l.*, inclusive of all allowances; and they are tenable for five years, provided that the holder does not accept any appointment which in the judgment of the electors will interfere with the completion of his University studies. The examination for the Mathematical and Natural Science Demyships will be held in common with Merton College, at the same time and with the same papers. Each candidate will be considered as standing in the first place at the College at which he has put down his name, and, unless he shall give notice to the contrary, will be regarded as standing at the other College also. In conducting the Examination for Magdalen College Demyships in Natural Science, questions will be put relating to General Physics, to Chemistry, and to Biology, including Human and Comparative Anatomy and Physiology, with the principles of the classification and distribution of Plants and Animals; but a clear and exact knowledge of the principles of any one of the above-mentioned Sciences will be preferred to a more general and less accurate acquaintance with more than one. The Examination in Biology and Chemistry will be partly practical, if necessary. Candidates for Demyships in Natural Science and Mathematics have also to satisfy the Electors of their ability to pass the ordinary Classical Examinations required by the University. Very superior excellence, however, in Natural Science or Mathematics will be allowed to compensate for any deficiency which Candidates may show in the Classical part of the Examination, provided that the Candidate, if elected, undertake to make up this deficiency at a subsequent period. The next Examination will commence on Tuesday, October 7, at 9 A.M. Particulars relating to the examinations in the various subjects may be obtained by applying to the senior tutor.

THERE will be an election at Merton College, Oxford, in October next, to three Postmasterships, value 80*l.* per annum, tenable for five years, or so long as the holder does not accept any appointment incompatible with the full pursuance of his University studies. One of these Postmasterships will be awarded for proficiency in Mathematics, two for proficiency in Physical Science. In the examination for the Mathematical Postmastership, papers will be set in Algebra, Pure Geometry, Trigonometry, Theory of Equations, and Analytical Geometry of two dimensions. Candidates for this Postmastership must not have exceeded four terms of University standing. There is no limit of age. In the examination for the Physical Science Postmasterships, papers will be set in Chemistry, Physics, and Biology; and an opportunity will be given of showing a knowledge of practical work in Chemistry and Biology. The Postmasterships will be given either for special excellence in one subject, or for excellence in two of the three subjects; but no candidate will be examined in more than two subjects. A paper will be set in Elementary Algebra and Geometry, which, *cæteris paribus*, will be of weight in the election to the Postmasterships. Candidates for these Postmasterships must not have exceeded six Terms of University standing. There is no limit of age. The examination will commence on Tuesday, October 7, at 9 A.M., in Merton College Hall. Candidates are required to call on the Warden on the same day between 4 and 5 P.M. The examination will be held in common with Magdalen College at the same time, and with the same papers. Each candidate will be considered as standing, in the first instance, at the College at which he has put down his name, and, unless he has given notice to the contrary, will be regarded as standing at the other College also.

FROM the report on the progress and condition of the Royal Gardens at Kew during the year 1872, just published by Dr. Hooker, it appears that the number of visitors to the gardens shows an increase of 6,000 over that in 1871, very nearly half the number being Sunday visitors. Considerable additions and improvements have been made during the year in various parts of the gardens; the Pinetum now numbers about 1,200 species of coniferous plants, including almost every species that can be grown out of doors in this climate. Seeds and living plants have been received from various parts of the world, and a large number of parcels sent off to our colonies and elsewhere. The acquisitions to the Museums have been considerable, and those to the Herbarium quite exceptional in magnitude and importance, including an extremely valuable presentation by the Rev. C. New of plants collected on the Alpine zone of Kilima-njaro, the only hitherto visited snow-clad mountain in Equatorial Africa; 2,000 Brazilian plants from M. Glaziou, Director of the Botanic Gardens at Rio de Janeiro; and a beautiful collection of Appalachian mosses from Prof. Asa Gray of Cambridge, U.S. Among the publications issued during the last year either officially or by private botanists working at Kew, are the commencement of the second volume of Bentham and Hooker's "Genera Plantarum," the sixth volume of the "Flora Australiensis," by Mr. Bentham; the first part of the "Flora of British India," by Dr. Hooker; several parts of Martius's "Flora Brasiliensis;" Col. Grant's account of the plants collected by Capt. Speke and himself in Central Africa, &c.

SPECIAL certificates of proficiency have been taken at the recent examination for women of the University of London in the following scientific branches:—in Mathematics, by Miss Black and Miss Orme; in Chemistry and Natural Philosophy, by Miss Eaton and Miss Wood; in Human Physiology, by Miss Kilgour of the Ladies' College, Cheltenham, the first time this branch has been taken by a lady; and in Political Economy by Miss Lord and Miss Orme.

MR. GWYN JEFFREYS is about to join the *Challenger* at Madeira for a cruise to the Canaries, Cape de Verde Islands, and Bahia.

M. P. J. VAN BENEDEN describes, in the Bulletin of the Belgian Academy of Sciences, a fossil bird found in the Rupelian clay of Waes, in all respects similar to the existing *Anas Marila*.

LAST Saturday appeared the first number of a new French scientific periodical named *La Nature*. The articles are all popular, and the illustrations are plentiful and well executed.

DR. LEONE LEVI, the Consul-General for Paraguay, is arranging a scientific commission to inquire into the resources of Paraguay. The commission is to consist of botanical, agricultural, geological, mineralogical, and geographical surveyors. It is understood that the Consul-General has in view to appoint a French botanist, of great reputation, and a Scotch agriculturist, but has made no arrangement for the geologist and geographer. Dr. Levi would be glad to give information to anybody who might be willing to offer his co-operation in such a scientific expedition.

LETTERS from Sydney announce the arrival there of the Italian frigate, *Vettor Pisani*, with the naturalist D'Albertis on board, he having been forced to leave New Guinea by repeated attacks of fever. His companion, Odoardo Beccari, well known for the valuable collections he made between 1865 and 1868 in Borneo, and subsequently in N.E. Africa, and which are now in the civic museum of Genoa, has remained in New Guinea. Signor D'Albertis is coming overland to London, and will bring with him a large collection of Zoological specimens.

THE second of the two parts of Prof. C. J. Sundevall's new Synopsis of the Classification of Birds has just reached us from Stockholm. This important contribution to ornithological literature, the work of so justly celebrated and painstaking an ornithologist, will be found replete with suggestions, as its author bases his methods of arrangement on details worked out mostly by himself, and with a truly scientific spirit. Some of the arrangements suggested are particularly striking, and though they will probably not all bear the test of future inquiry, yet are undoubtedly based on characters, the importance of which has been too little attended to. Among these peculiarities may be mentioned the placing of the Hoopoo with the Larks, quite away from *Irrisor*; and the adoption of Strickland's eccentric idea that the Pratincole is only a modified Nightjar; to say the least, would it not be more reasonable to call the Nightjar a modified Plover?

THE correspondent of the *New York Herald* at Khartoum writes to that journal as follows, under date of April 30:—Three boats engaged in the ivory trade arrived from Gondokoro, April 7, with direct news that Sir Samuel Baker and family were well at Fatuka in the month of February. The reinforcement of 200 men which went forward from Gondokoro reached Baker, at Fatuka, February 5. It was said that with these troops Baker would renew his march towards the Albert Nyanza and the territory of Kaberego (formerly Kamrasi). We are hourly expecting the arrival of a fleet of nineteen Government vessels with mails, which will doubtless bring full particulars of Baker's recent movements.

IN No. 145 of the *Gazzetta Ufficiale del Regno d'Italia*, Prof. Lorenzo Respighi, director of the observatory at Campidoglio, gives an account of his observations of the eclipse of May 26. He states that though the maximum phase was so small as to be of little importance, he considered it a good opportunity for making spectroscopic time observations. The method is very simple, and is well known to spectroscopists; it need only be said that it consists in observing accurately the moment at which the dark body of the moon cuts out one of the chromospheric bright lines. Prof. Respighi observed the C-line and was able to perceive the moon's approach across the chromosphere about one minute before first contact, which took place at 46°·30′ from north towards the west point of the sun at 8h. 42m. 35·9s. Roman mean time. The greatest phase occurred at 9h. 7m. when 0·05 of the sun's diameter was covered. The last contact was observed at 10° from the north towards west at 9h. 31m. 3·4s. Roman mean time. The dark moon was seen passing over the chromosphere for about a minute after last contact. The Sicilian expedition had before noticed the power which the spectroscope gave of observing the first and last moments of contact before the times given in the Nautical Almanac, and there can be no doubt that this method is of very great value for time observations of eclipses and transits. Unfortunately in the latter cases it is almost or quite impossible to keep the slit at the exact point at which the body is expected to enter the solar disc on account of the difficulty in obtaining perfect adjustments of the driving clock, &c. It might however be possible to follow the body in transit across the sun and note the exact time of last contact.

WE have received the fifteenth report of the East Kent Natural History Society, containing reports of the scientific meetings for the year 1872, and various statistical reports. The society has probably never been in a more prosperous condition as to funds and members, the number of the latter being reported as 109, and the reports of the meetings show that the society is in good working trim. Prefixed is a brief but pointed address by the President, Dr. Mitchinson, in which he points out the utility and some of the dangers of Provincial Natural History Societies. He refers to one evil which is apt to result from the labours of such societies, an evil which has with justice been animadverted on from various quarters recently, viz. a morbid mania for indiscriminate collecting, which is apt to lead to the extinction of the rarer fauna and flora of a district. No doubt, as Dr. Mitchinson says, collecting is inseparable from the thorough study of botany and zoology; but, as he forcibly remarks, no surer sign exists of a spurious pursuit of either or both of these sciences than when rare plants are torn up, and rare animals made still rarer by that selfish acquisitiveness which passes with so many for a love of science. It is the duty of every Natural History Society to discourage such a practice.

THE discovery of another planet, No. 131, is telegraphed from America.

IT has been resolved by the United States' Government to hold an investigation into the circumstances connected with the loss of the Arctic exploring ship *Polaris* and the death of her commander, Captain Hall.

THE publication of the West Kent Natural History, Microscopical, and Photographic Society, is mainly occupied by two valuable and extremely interesting addresses by the president, Mr. J. Jenner Weir, F.L.S. The first was delivered at the annual meeting in February last, and consists chiefly of some careful observations and facts illustrating the doctrine of evolution in the animal kingdom. His other address was delivered at a *soirée* held at the Crystal Palace, its subject being "The Aquarium and its Contents," Mr. Weir noticing some of the most remarkable facts connected with the organisation and habits of the different classes of animals in the aquarium. We are glad to see from the Council's report that the Society continues prosperous and efficient.

ADDITIONS to the Brighton Aquarium during the past week:—One Sturgeon (*Accipenser sturio*), from Rye Bay; Smooth Hounds, or Skate-toothed Sharks (*Mustelus vulgaris*); Toper, or White Hound (*Galeus canis*); Gurnards (*Trigla lyra et lineata*); Lesser Weevers (*Trachinus vipera*); Scald Fish (*Arnoglossus laterna*); Sea Trout (*Salmo trutta*); Surmullet (*Mullus surmuletus*); Conger Eels (*Conger vulgaris*); Octopus (*Octopus vulgaris*); Lobsters (*Homarus vulgaris*); Sea Crayfish (*Palinurus vulgaris*); Sea Cucumbers (*Cucumaria pentactes*); Zoophytes (*Alcyonium digitatum, Tubularia indivisa, Pleurobrachia pileus*).

THE additions to the Zoological Society's Gardens during the past week include a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mrs. W. Simpson; an Eyed Lizard (*Lacerta ocellata*) from S. Europe, presented by Mr. T. Blackmore; a Loggerhead Turtle (*Thalassochelys caouana*) from the Atlantic Ocean, presented by Lieut. N. Clark; a Rough-legged Buzzard (*Archibuteo lagopus*) from Europe, presented by Mr. W. Stokes; a Blotched Genet (*Genetta tigrina*) from W. Africa, presented by Mr. A. B. Worthington; two Emus (*Dromæus novæ-hollandiæ*) from Australia, presented by Hon. Sir A. Gordon; a Persian Gazelle (*Gazella subgutturosa*), presented by Captain Phillips; seventeen Turtle Doves (*Turtur auritus*) and a Barbary Turtle Dove (*Turtur risorius*), presented by Mr. Gandlot, Jun.; two Lions (*Felis leo*) from Persia; a Wapiti Deer (*Cervus canadensis*) from N. America, purchased; four Trumpeter Swans (*Cygnus buccinator*) and a Purple Kaleege (*Euplocamus horsfieldii*) hatched in the Gardens; four Aldrovandi's Lizards (*Plestiodon auritus*) and two Ocellated Skinks (*Seps ocellatus*) from N.W. Africa, deposited.

ON MUSCULAR IRRITABILITY AFTER SYSTEMIC DEATH*

THE object of the lecture was to put forward certain facts the author had learned on the phenomenon of muscular irritability after systemic death. He included in the same study certain examples in which muscular irritability has for a time ceased, but has become re developed under new conditions. He thus included the study of those states which favour the continuance of irritability or which destroy it, and those conditions which suspend it but do not destroy it. By this method of research the author thinks we may proceed backwards towards living irritability, and may determine upon what that depends with more facility than by experimenting on the phenomena of irritability in the living animal. He imagines that if he knew nothing of the construction of a watch, or why for a certain time a watch maintains its motion, and if he had nobody to teach him these things, he might be better able to arrive at the fact he wanted by trying to set the motionless watch into motion than by interfering with it while it is in motion.

The record of experimental endeavour carried out with the design above explained, included a review of the work of twenty-five years. The subjects brought under consideration were arranged as follows:—

(1) The effect of cold on muscular irritability after systemic death.

(2) The effect of motor forces, mechanical, calorific, electrical.

(3) The effect of abstracting and supplying blood.

(4) The effect of certain chemical agents, organic and inorganic.

Effects of Cold

Previous to the time of John Hunter it was supposed that cold was the most effective agent for destroying muscular irritability. The effects of cold employed in various ways in the author's experimental researches were now detailed systematically. The effect of cold in suspending the muscular irritability of fish, reptiles, and frogs was first described. On all these animals it was shown that cold could be made to suspend without destroying the muscular irritability, for a long period of time, and that in fish, carp (on which the author had made the greatest number of experiments) the restoration of irritability could be perfected to the extent of the restoration of the living function.

Passing to warm-blooded animals, the author showed that in the process of cooling in every animal that has been suddenly deprived of life without mechanical injury, there is a period in the process when general muscular irritability may be made manifest. He demonstrates this fact by the simple experiment of throwing a current of water heated to 120° Fahr. over the arterial system of the recently dead animal. If the surrounding temperature be high at the time of this experiment, the operation should be performed within a few minutes after death; but if the temperature be below freezing-point, it may be delayed for a long period. In one experiment the author reproduced active muscular contraction in an animal that had lain dead and exposed to cold, 6° below freezing-point, for a period of three hours. In this case the muscles generally remained irritable for seven minutes after the injection of the heated water, while in the muscles of the limbs, by repeating the injection at intervals, the irritability was maintained for two hours.

The author drew a comparison between these experimental results and the phenomena of muscular irritability that have been observed in the human subject after death by cholera. The movements were not conscious, nor were they promoted by electrical excitation; but the flexors and extensors belonging to each part in which there is movement are alternately contracted and relaxed as if from some internal influence.

The influence of cold in suspending without destroying muscular irritability was further evidenced by the experiment of subjecting some young animals to death by the process of drowning them in ice-cold water. It was shown that in the kitten the muscular irritability may be restored to the complete re-establishment of life after a period of two hours of apparent systemic death, and although the muscles when the animal is first removed from the water give no response to the galvanic current. This same continuance of irritability after apparent systemic death by drowning in ice-cold water has been observed in the human subject, not in so determinate, but in an approximated degree. An instance was adduced in which a youth who had been deeply immersed for twelve minutes in ice-cold water retained muscular irritability so perfectly that he recovered, regained consciousness, and lived for a period of seven hours.

Commenting on the method of restoration of irritability, the author showed that a certain period of time is required before the irritability is raised from a mere passive condition, in which it responds only to external stimuli, into the condition necessary for independent active contractility. The change of condition from the passive to the active, when it does occur, is so sudden as to seem instantaneous at first, then it is slowly repeated. This rule holds good in respect to voluntary muscles and involuntary. It is specially true in regard to the heart, which organ, the author states, may perform its office under two distinct degrees of tension or pressure—a low tension, in which the organ itself is reduced in size, and moves almost insensibly; and a full tension, in which it is of larger size, and moves with a sufficient power to impel the blood so as to overcome the arterial elasticity and the capillary resistance.

Another fact bearing on this subject is that in rapid decline of muscular irritability the muscles most concerned in the support of the organic functions, namely, the heart and the muscles of respiration, are the last to yield up their spontaneous power; but when they have lost their power, they are the last to regain it. To this rule there is one exception, viz., in the muscular fibre of the right auricle of the heart.

The author then explained that the degree of cold which suspended irritability is fixed within certain measures of degree, from 38° to 28° F. being the most favourable degrees of exposure.

Effect of Motor Forces

Cold, by the inertia it induces, suspends, under certain conditions, but does not destroy muscular irritability. The motor forces, on the contrary, quicken the irritability for a brief period, and then completely destroy it. The mode in which all the motor forces act in arresting irritability is by the induction of a contractile state, which, once established, remains permanent. The author here related his experiments on the effect of the different forces upon the right auricle of the heart, and reported as the result of his observations that, while all the forces act ultimately alike in producing permanent contraction, the mechanical excitation is much slower than the calorific; while electrical excitation appears to hold an intermediate place, as if it were a combination of mere mechanical motion with an increased temperature. Electrical tension may nevertheless be increased so as to rival heat in its immediate effect on contraction.

The author here traced out the results of a series of short sharp irritations of muscle with a needle-point, and compared them with the effect of a blow, showing that in each case rigidity follows, but is much slower in development when it is excited by the needle.

The influence of heat in destroying irritability by its power in producing permanent contraction was described from experiments bearing on the relation of temperature to the muscular contraction of different animals—frogs, pigeons, and rabbits. It was shown that a relative rise in temperature in each class, a rise averaging 12° in Fahr. scale, from the natural temperature of the animal was the efficient for producing permanent rigidity, the cause of the ultimate rigidity being coagulation of the myosine.

The effect of electrical excitation is in the same direction, but is varied according to the mode in which the excitation is performed. Discharge from the Leyden jar produces contraction, which is permanent or intermittent in accordance with the mass of the muscle and the intensity of the discharge. This fact was elucidated by reference to a series of experiments with a Leyden battery, placed in cascade, and the effect produced by the discharge from 96 feet of surface upon animals of different sizes and weights, from sheep down to pigeons, as well as on sections of the bodies of the same animals immediately after death. The experimental facts demonstrated that with an efficient discharge the whole muscular system of a small animal could be fixed instantly in the rigidity of death, and that the precise position of the animal at the period immediately preceding death was retained with such perfection, so sudden was the change, that nothing but physical examination by the hand could bring to the mind the fact that the animal had passed from life into death.

But the same shock passed through a sheep weighing 54 pounds produced only a temporary contraction of muscle, and required repetition before the rigidity was rendered permanent.

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* The Croonian Lecture, by Benjamin W. Richardson, M.A., M.D., F.R.S.

By employing discharges of less tension it was found that muscles, or special tracts of muscles, in the same animal immediately after its death, could be made rigid quickly or slowly by variation of the intensity of the discharge.

The effect of the intermittent electro-magnetic current was next brought forward, and was shown to resemble closely that of the simple electrical discharge from the Leyden phial.

Intensified it induces permanent contraction; and if it be repeated even with low tension, so as to call forth contraction, it destroys the irritability, *cæteris paribus*, more quickly than if the muscle had been left to itself.

Parenthetically, the lecturer dwelt here on the common practice, after sudden death, of endeavouring to excite the action of the enfeebled heart by passing through it an electrical current. Some practitioners, said the author, have gone so far as to introduce a needle into the heart itself, and to make the needle act as one of the conductors from a battery. Such experimentalists, before they undertake this operation on the human subject, should at least observe the effect of the agency they are employing on the exposed heart of an inferior animal recently and suddenly killed by drowning or by a narcotic vapour. They would learn then with what infinite facility the muscular irritability of the heart, in all its parts, is excited for a moment only to be permanently destroyed. They would learn that if blood be not passing through the muscular structure concurrently with their exciting current, they could not more effectually arrest function than by the very method they have adopted to sustain it.

The influence of the continuous current on muscular irritability was introduced by the author, together with a special reference to the first experiments of Aldini on the bodies of malefactors who had been recently executed; and it was shown from Aldini's most noted experiment how largely the phenomena of motion he induced in a dead man, and the recital of which caused so much sensation in the year 1803, was due, not to the galvanism, but to the circumstance that the dead body had been exposed for the hour after death and before the experiments commenced, to the action of cold two degrees below freezing-point. On the whole the continuous current acts on muscular fibre after the manner of heat. If the muscle, recently dead, be exposed to cold, the current, when sufficient, restores for a limited period the irritability, and finally destroys it by inducing persistent contraction. If the muscle, recently dead, be left at its natural temperature, the current simply shortens the period of irritability by quickening contraction.

Abstraction and Supply of Blood

Under this head the author first considered the effect of abstraction of blood from the living muscular fibre. He showed that when the flow of blood was very rapid, there was invariably a given period of muscular excitation. In sheep killed in the slaughterhouse he found that this muscular excitement occurred at the time when the proportion of blood removed from the animal was equivalent to about the 320th part of the weight of the animal. The increased irritability passes rapidly into general convulsion without consciousness, and, as a rule, ceases for a time with a temporary cessation of further loss of blood. After this the irritability remains, if the bleeding be arrested altogether, and can be called into action by any external stimulus, although it is rarely spontaneously manifested when the vessels are left divided and open. After an interval of one or two minutes there is a recurrence of loss of blood, followed by a muscular excitement which marks the moment of systemic death.

The fact of the two stages of exalted muscular irritability during abstraction of blood is important, as indicating the two different tensions of muscle to which reference has already been made. The first convulsive action, convulsion of syncope, marks a definite period, when the tension of the heart and therewith the whole vascular system is reduced to a degree of action well defined and attended with definite phenomena. The second excitement, convulsion of death, indicates the period when the passive or lower tension of the muscular power ceases.

A distinction was here drawn by the author between the muscular conditions present during syncope and during death. Syncope, it was urged, means the continued action of the heart at a low tension, from which it can be suddenly raised into full tension with restoration of the powers of life; death means the cessation of the lowest tension at which the heart can effectively work.

It was shown that in all the cases of restored animation after apparent death, the condition of the heart was that of a muscle acting under the lower degree of tension.

The experiments of the author for re-establishing artificial respiration together with artificial circulation, and of these combined with electrical excitation of the nervous centres, were next referred to; but as they had already formed the subject of a paper read before the Society, they were but briefly dwelt upon.

Effect of some Chemical Agents

In this portion of his lecture the author adduced a series of experimental researches with various chemical substances, organic, inorganic, and intermediate, which tend to prolong the period of muscular irritability by diffusion through the tissues of animals recently dead. These substances, which suspend irritability, act in two ways. Some, like chloride of sodium and other soluble saline substances, act merely by holding the coagulable fluid of the muscular tissue in a continued state of fluidity; others seem to have a different action, and to hold the nervous function also in suspense. The nitrite of amyl and other members of the nitrite series belong to this last-named class of agents, and some of the cyanogen bodies exert a similar influence. In experiments with nitrite of amyl on cold-blooded animals (frogs), the author had suspended muscular irritability for a period of nine days, and had then seen it restored to the extent even of restoration of life. In one instance this restoration took place after the commencement of decomposition in the web of the foot of the animal. In warm-blooded animals a series of suspensions had been effected by nitrites and also by cyanogens, not for so long a period, but for periods of hours, in one instance extending to ten hours.

In the whole series of his inquiries no fact had impressed the author more forcibly than this: that the muscular irritability, in so far as it belongs to the muscle, may be sustained for hours after the nervous excitation which calls it into spontaneous action has ceased. Hereupon he infers that after death the nervous matter undergoes a change of condition which, *in result*, is identical with that change in muscle which we call rigor. There is evidence, moreover, from some rare cases, that the final inertia of nervous matter may be suspended and revived, so that all the muscles may be reanimated. This point was elucidated by reference to the phenomena that had recently been observed by Mr. Wadsdale Watson, of Newport, Monmouthshire, on a double monster, drawings of which were placed before the society. In this instance two children were born so attached that the separation of them was impossible. Both lived equally for three hours after birth, and then one died and remained dead for three hours, while the other lived. At the end of the time named the dead child recommenced to breathe, and showed other signs of restored muscular power; then it sank into a seemed death, but at intervals of about four hours moved again; at length, twenty-three hours after its first apparent death, during a fit of crying of the living child, it recovered sufficient power to breathe and even to cry, and manifested evidence of life in all its muscles, except the heart, for twenty minutes, when it had a severe convulsion, which closed all further motion.

In this instance the author believed that the retention of spontaneous muscular irritability depended upon the retention in the nervous organism of the conditions necessary for independent action. He then concluded by giving a description of his researches as to the possibility of suspending nervous changes incident to death, so as to retain the conditions requisite for the communication of nervous impulse to muscular fibre.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, Neue Reihe, Band xci, Heft 1, May 6, 1873.—The number opens with a long paper by Oscar Jacobsen on the gases of sea-water. Notices of former researches on this subject are given. In a table the results of 95 analyses by the author are given with the localities of collection. These are in the North Sea and the Baltic.—On the oxidation of allantoin by means of potassic ferricyanide, by F. C. E. van Embden. The two bodies were mixed, one molecule of each, in solution, and the mixture acidulated with acetic acid. A crystalline precipitate was produced, having the formula $C_4N_3H_4KO_4$. This the author regards as the potassium salt of the new acid $C_4N_3H_3O_4$, which he proposes to call allantoxanic acid. Various other salts are described. The acid is found to be bibasic.—On the action of sodium-amalgam on dinitrohephtylic acid, by H. A. Kullhem. The result of the action appears to be the formation of a monobasic acid having the formula $C_8H_{14}(NO_2)O_2$.—On the products of the decomposition of the chlorhydrin of glyceric acid, by Messrs. Werigo and Okulitsh.—On a new acid from aloes, by P. Weselksy. The body in question was obtained from Socotra aloes; its formula is, $C_9H_{10}O_4$ when dried in the air, and its anhydride has the formula $C_{18}H_{18}O_7$. The acid is apparently dibasic.—Dr. H. Sprengel communicates a paper on the water air-pump.—On liquid carbonic anhydride, by L. Cailletet, is a translation from the author's late paper in the *Comptes Rendus*—On the addition of cyanamide, by Dr. E. Baumann, is an account of the compounds formed when this body is added to various others.—On the combination of bromine and ether, by P. Schutzenberger, has already appeared in the *Comptes Rendus*.—An examination of a new alkaloid, by Prof. Hlasiwetz. The body in question is a product of the oxidation of cinchonin.—On the isomers of dinitrophenol, by H. Hübner and W. Schneider.—On the nature of sulpho and sulphonitrobibrombenzolic acid, by H. Hubner and R. Douglas Williams.—On the synthesis of carbazol and on phenathren, by C. Graebe.—Contributions to the history of the orcins, by J. Stenhouse, has already appeared in the Proceedings of the Royal Society, the present communication, No. III. of the series, deals with the amido-derivatives of those bodies.—On a new method of preparing carbonic tetrabromide from bromoform, by J. Habermann. The author acted on bromoform in the presence of potash with bromine. The mixture exposed to direct sunlight for 5–6 days gives a good product of tetrabromide. In the dark, after an exposure of three months, only a trace was formed. The reaction occurs as follows:—

$$CHBr_3 + Br_2 + KHO = CBr_4 + KBr + H_2O.$$

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 1.—"On the Condensation of a Mixture of Air and Steam upon Cold Surfaces." By Prof. Osborne Reynolds.

The object of this investigation is to ascertain how far the pressure of a small quantity of air affects the power of a cold surface to condense steam.

The conclusions which the author draws from the experiments are as follows:—

1. That a small quantity of air in steam does very much retard its condensation upon a cold surface; that, in fact, there is no limit to the rate at which pure steam will condense but the power of the surface to carry off the heat.

2. That the rate of condensation diminishes rapidly, and nearly uniformly as the pressure of air increases from two to ten per cent. that of the steam, and then less and less rapidly until thirty per cent. is reached, after which the rate of condensation remains nearly constant.

4. That in consequence of this effect of air the necessary size of a surface-condenser for a steam-engine increases very rapidly with the quantity of air allowed to be present within it.

5. That by mixing air with the steam before it is used, the condensation at the surface of a cylinder may be greatly diminished, and consequently the efficiency of the engine increased.

6. That the maximum effect, or nearly so, will be obtained when the pressure of the air is one-tenth that of the steam, or when about two cubic feet of air at the pressure of the atmosphere and the temperature 60° F. are mixed with each pound of steam.

As this investigation was nearly completed, the author's attention was called to a statement by Sir W. Armstrong, to the effect that Mr. Siemens had suggested as an explanation of the otherwise anomalous advantage of forcing air into the boiler of a steam-engine, that the air may prevent, in a great measure, the condensation at the surface of the cylinder. It would thus seem that Mr. Siemens has already suggested the probability of the fact which is proved in this investigation. The author is not aware, however, that any previous experiments have been made on the subject, and therefore he offers these results as independent testimony of the correctness of Mr. Siemens's views as well as of his own.

"On the effect of Pressure on the Character of the Spectra o Gases." By C. H. Stearn and G. H. Lee.

May 8.—"Contributions to the Study of the Errant Annelides of the Older Palæozoic Rocks." By Prof. H. Alleyne Nicholson, M.D., F.R.S.E.

In this communication the author endeavoured to elucidate the abundant and obscure organic remains which are found so commonly in the Palæozoic Rocks, and especially in the Silurian strata of Britain, and which are generally known by the vague and convenient names of "Fucoids," "Annelide burrows," and "Tracks." After expressing his opinion that the first step towards the study of these obscure fossils lay in the provisional grouping and naming of the more marked forms which are already known to exist, the author proceeded to divide the remains under consideration into two great groups. In the first of these groups are those fossils which are truly the *burrows* of marine worms, as distinguished from mere trails and surface-tracks. Some of these burrows (*Scolithus*) are more or less nearly vertical in direction as regards the strata in which they are found; and they are to be looked upon as being true burrows of habitation. In this section are placed the genera *Scolithus*, *Arenicolites*, and *Histioderma*.

The second great group of Annelide remains comprises genuine surface-trails or "tracks," which of necessity never pass below the surface of the bed on which they occur.

"The Action of Light on the Electrical Resistance of Selenium." By Lieut. Sale, R.E. Communicated by J. N. Lockyer, F.R.S.

The following were the general results of the experiments:—

1. That the resistance of selenium is largely affected by exposure to light.

2. That this effect is not produced by the actinic rays, but is at a maximum at, or just outside the red rays, at a place nearly coincident with the locus of the maximum of the heat-rays.

3. That the effect of varying resistances is certainly not due to any change of temperature in the bar of selenium.

4. That the effect produced on exposure to light is sensibly instantaneous, but that on cutting off the light the return to the normal resistance is not so rapid.

It would seem that there exists a power in rays, nearly coincident with the heat-rays of high intensity, of altering instantaneously and without change of temperature the molecular condition of this particular element.

May 15.—"On Jeypoorite, a Sulph-antimonial Arsenide of Cobalt." By Major W. A. Ross, R.A. Communicated by Prof. H. Miller, Foreign Sec. R.S.

"Determination of the Number of Electrostatic Units in the Electromagnetic Unit made in the Physical Laboratory of Glasgow University." By Dugald M'Kichan, M.A.

The object of this paper is to describe experiments made at intervals from 1870 to 1872 in the Physical Laboratory of Glasgow University to determine the relation between the fundamental units in the two systems of absolute electrical measurement, the electromagnetic and the electrostatic. A summary is also given of the results of similar observations made by W. F. King in 1867 and 1868.

The two systems of electrical measurement, or the units which they employ, are founded on the fundamental units of time, mass, and space applied to the observed effects of electricity at rest and electricity in motion. The dimensions of quantity in the two systems are such that the ratio of the electromagnetic and the electrostatic unit of quantity is expressible as a velocity.

This velocity, usually known as v, is not only of great importance in all combinations of electromagnetic and electrostatic action, but it is also of great scientific importance in the theory of the propagation of electromagnetic disturbances through a dielectric medium. It occupies a very important place in the development of the electromagnetic theory of light by Professor Clerk Maxwell, according to whose theory this velocity v is the same as the velocity of light.

The first experimental determination of v was made by Weber from a common electrostatic and electromagnetic measure of capacity. The result of Weber's experiments was that v was $310{\cdot}74 \times 10^8$ centims. per second.

Another determination was made by Prof. Clerk Maxwell in 1868, by means of a direct comparison of electrostatic attraction with electromagnetic repulsion. His experiments gave $v = 288{\cdot}0 \times 10^8$ centims. per second.

The value of v given by the experiments here described is 293×10^8 centims. per second. The method employed was that of obtaining an absolute electrostatic and an absolute electro-magnetic measurement of the same electromotive force. v is defined as the ratio of the units of quantity in the two systems; but it follows from the definition of electro-motive force, that v is also the ratio of the units of electromotive force in the two systems.

The electromotive force, or the difference of potentials between the two poles of a constant Daniell's battery, was measured electrostatically by means of Sir William Thomson's absolute electrometer. The absolute electromagnetic value of this electromotive force was given by the effect of the current which it maintained in the circuit of an electrodynamometer. The determination of this value depended on the resistance of the electrodynamometer-circuit, which was reckoned in terms of the absolute value of the British-Association standard unit of resistance. Any correction which may hereafter be found to be applicable to the absolute value of this standard coil, as measured at King's College by Professors Clerk Maxwell, Balfour Stewart, and Fleeming Jenkin, must be applied to the value of v give above.

The comparisons made in 1867 and 1868 by Mr. King gave as the mean value of v, $284{\cdot}6 \times 10^8$ centims. per second. The experiments made in 1870 with the new absolute electrometer gave as the mean result $v = 294{\cdot}5 \times 10^8$ centims. per second. The result of the later observations made under much more favourable circumstances was $v = 292{\cdot}4 \times 10^8$ centims. per second. The latest observations (1872) furnish the most probable value of v, 293×10^8 centims. per second.

Zoological Society, June 3.—Viscount Walden, F.R.S., president, in the chair. The secretary read a report on the additions that had been made to the Society's collection during the month of May. The following, among other objects, was exhibited:—The figure of a supposed new species of *Chelodina* from the Burnett River, Queensland.—A letter was read from Dr. George Bennett, F.Z.S., referring to the supposed existence of a species of Tree Kangaroo (*Dendrolagus*) in Northern Queensland, some such animal being apparently well known to the blacks of Cardwell.—A memoir was read by the Viscount Walden on the birds of the Philippine Archipelago, founded mainly on the recent collections of Dr. A. B. Meyer, but containing a complete account of all the known species of Philippine birds, and remarks on their geographical range. The total number of known Philippine species was estimated at 215, but a large number of the islands remained unexplored.—A paper was read by Sir Victor Brooke, Bart., F.Z.S., on the antelopes of the genus *Gazella*, of which 20 species or "persistent modifications," as the author preferred to call them, were recognisable. Sir Victor Brooke entered at full length into the questions connected with the present geographical distribution of the group, and its supposed descent from pliocene and miocene forms.—Mr. A. H. Garrod read a paper on the pterylosis and on some points in the anatomy of the Guácharo (*Steatornis caripensis*) and showed that this singular bird must be constituted a family *per se*, related in some respects to the Caprimulgidæ and their allied forms, and in other respects to the Owls (*Striges*).

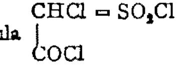
Chemical Society, June 5.—Dr. Odling, F.R.S., president, in the chair.—Six communications were read before the society, the first being "On the dioxides of calcium and strontium," by Sir John Conroy, Bart., in which the author gave the method of preparation and properties of these substances.—Mr. T. Wells then described a new form of ozone generator which gives abundance of ozone and has the advantage of being easily constructed and not liable to be broken.—The other papers, which contained but little of general interest, were entitled "On the behaviour of acetamide with sodium alcohol," by W. N. Hartley; "On iodine monochloride," by J. B. Hannay; "On triferrous phosphide," by Dr. R. Schenk; and "On sulphur bromide," by J. B. Hannay.

Anthropological Institute, June 3.—Prof. Busk, F.R.S., president, in the chair.—The president exhibited and described a new apparatus for measuring, with ease and accuracy, the cubic capacity of skulls. Prof. Rolleston, while approving generally the method of Prof. Busk, differed with him in the nature of the material to be employed; he thought that sand was objectionable as being subject to hygrometric variation from which rape-seed was entirely free.—Prof. Robinson exhibited a remarkable bronze sword found in the bed of the Charwell, Oxfordshire, a bronze spear from Speen, near Newbury, and other implements of bronze and stone.—The president exhibited a series of stone implements from the Island of St. Vincent, West Indies, and Mr. A. W. Franks exhibited a bow and poisoned arrows lately used by the Modoc Indians, and found in Captain Jack's stronghold in the lava beds of Siskiyou County, California.—The Rev. Dunbar I. Heath contributed Notes on a Mural Inscription, in large Samaritan characters, from Gaza, and claimed for it a higher antiquity than the date of the Moabite Stone.—Mr. H. Howorth read a paper entitled, "Strictures on Darwinism, part II., the Extinction of Types." The substitution of species involved two factors; 1st, the extinction of certain types; 2nd, the introduction of certain others. The paper dealt with the former factor only. Pre-Darwinian naturalists, and some of those who now oppose Darwin, have agreed that species become extinct through the operation of causes, such as climatic change, &c., acting *ab extra* and operating upon whole classes at once from without. Mr. Darwin has argued, on the other hand, that this extinction has arisen from the mutual struggles of individuals by which a certain strong and vigorous type has been evolved, and a certain weak and decrepit type extinguished; the difference between the two theories being that one relies upon external, the other upon internal causes for the explanation of the extinction of certain types. In the present paper the author examined the problem and attempted to show that the old view was the correct one. The paper passed in review the various elements that have gone to destroy types of life, changes in physical geography, changes in climate, epidemics, &c., and showed how the evidence of all of these supported the old view that extinction of type is the result of external influences, and not, as Mr. Darwin contends, of an internal struggle for existence. Prof Rolleston, Mr. Boyd Dawkins, and the president, combated the criticisms of the author.

Royal Microscopical Society, June 4.—Chas. Brooke, F.R.S., president, in the chair. The secretary read a paper by Mr. F. Kitton, of Norwich, descriptive of a new species of *Navicula*, with remarks on *Aulacodiscus formosus*, *Omphalopelta versicolor*, &c., collected in Peru by Captain Perry, of Liverpool; the paper was illustrated by specimens exhibited in the room.—A paper was also read by Mr. J. Stephenson, on the appearances of the inner and outer layers of *Coscinodiscus* when examined in bisulphide of carbon and in air, in which the author pointed out the different effects obtained by mounting the diatoms in media of different refrangibility, and showed the value of such comparisons in determining the nature of the markings, as well as the general structure. The paper was illustrated by a number of very carefully executed drawings by Mr. Charles Stewart, and by specimens exhibited under the microscope. The meetings of the society were adjourned until October.

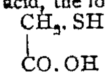
BERLIN

German Chemical Society, May 26.—A. W. Hofmann in the chair. Dr. Seligsohn investigating the origin of the oxalates deposited in the human body, has found that oxamide can be transformed into urea by ozone, and thinks therefore that oxamide is an intermediate product of digestion between the higher compounds of carbon and urea.—Dr. Rudorff has found that saturated solutions of chloride of ammonium and nitrate of potassium are not influenced in their composition by adding either of these salts, while saturated solutions of nitrate of ammonium and chloride of potassium are changed in their composition by adding either one or the other to these solutions. In the same way behave most salts, so that the solution of one couple is influenced, while the other couple remains unchanged. But when K_2SO_4 and NH_4NO_3 are dissolved to saturation, this solution is influenced in the way described, and solutions of the opposite couple show likewise the alteration mentioned. These changes were proved by analyses and by determination of the changes of temperatures occurring. Self-evident conclusions offer with regard to the old question, if two salts in solution represent two or four different compounds.—C. Bulk spoke on the manufacture of arsenic acid from fuchsine-residues; by sublimation, as used in Elberfeld. The same chemist described a simple apparatus replacing spring-clamps in volumetric analysis. It consists of a piece of glass-rod inserted into an indiarubber tube. By pressing it cautiously drop after drop can be let out of the burette.—J. Grosshaus continued his speculation on the nature of chemical elements.—H. Vogel denied the existence of what Becquerel called "rayons continuateurs." He explains the fact that photographic negatives, exposed for a few seconds to chemical light, and then to the red and yellow part of the spectrum, are acted upon by these rays, by admitting that during the first exposure chloride of silver is reduced only to the state of sub-chloride, which in its turn is acted upon by yellow light, and thus reduced to the metallic state. This explanation appears the more probable, as iodide and bromide of silver do not exhibit the same property, iodine and bromine forming but one compound with silver.—Julius Thomsen reported on the amount of heat yielded by mixing nitric acid and water. The result of his experiments he sums up as follows:—A diluted nitric or sulphuric acid, when further diluted with the same quantity of water it already contains, will yield the smallest amount of heat, when the molecular heat of the acid is equal to that of the water which is contained in it.—Henry Armstrong sent a summary of his researches on isomeric derivatives of phenole, most of which are familiar to the English public.—Heinrich Baumhauer published some remarks on the natural system of chemical elements, and the relations between atomic and specific weights.—F. Birlstein and A. Kullberg have found that α-dinitro-naphthaline treated with a mixture of nitric and sulphuric acids yields a new γ-trinitro-naphthaline fusing at 147°, while fuming nitric acid produces only the ordinary α-trinitro-naphthaline.—E. Mulder obtained a yellow solid combination by precipitating cyanamide with nitrate of silver. Its composition, CN_2Ag_2, leads the author to suppose cyanamide to be constituted according to the formula $C(NH)_2$ of carbodiimide.—R. Siemens submitted sulfo-acetic acid to the action of perchloride of phosphorus in order to investigate the chloride thus obtained as well as its reduction. To the former he gives the formula



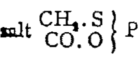
$$\begin{array}{l}CHCl - SO_2Cl\\ |\\ COCl\end{array}$$

to the compound obtained from it by the action of tin and hydrochloric acid, the formula of thio-glycollic acid:



$$\begin{array}{l}CH_2.SH\\ |\\ CO.OH\end{array}$$

forming the lead-salt



$$\left.\begin{array}{c}CH_2.S\\ CO.O\end{array}\right\}Pb.$$

The chloride is decomposed by water into the body formerly described by Kolbe under the name of trichlor-methyl-sulfor-chloride, $CCl_3.SO_2Cl$.

PARIS

Academy of Sciences, June 2.—M. de Quatrefages, president, in the chair.—The president announced the death of M. de Verneuil, *membre libre*, which occurred at Paris, May 29.—M. de Chevreul communicated the principal results of his researches on uric acid, which will shortly be published. The president presented the first part of the work on the crania of the human race upon which he and Dr. Hamy are engaged.—The following papers were read:—Note accompanying the presentation of a work on cellular anatomy and physiology, by M. Ch. Robin.—On the transit of Venus in 1882, by M. Puiseux.—Trial, during an eclipse of the sun, of the new spectroscopic method proposed for the observation of the next transit of Venus by Father Secchi. The method consists in placing a direct-vision system of prisms before the slit of the spectroscope, and then observing the interruption of the chromosphere by the dark body. The author compares observations by his method with those of Prof. Respighi, published in the *Gazetta Officiale*, No. 145. Respighi saw the approach of the moon 21·9 secs. before Secchi, but Secchi saw the last contact 12·3 secs. before Respighi. The Rev. Father therefore suggests the use of the ordinary method (that used by Respighi) for first contact, and of his own for last.—A study of the action of the principal derivatives of amylic alcohol on polarised light, by MM. Pierre and Puchot.—Development of the freshwater algæ of the genus *Batrachospermum*, alternate generation; second note, by M. Sirodot.—On the nature and treatment of ear tumors (*oreillons*), by M. Bouchut.—On *Hylodes martinicensis*, by M. A. Bavay.—Documents relating to the short-period Comet II, 1867, by Mr. Hind, M. Stephan, MM. Paul and Prosper Henry, M. André, and M. Baillaud. Communicated by M. Le Verrier.—Discovery of a new small planet by Mr. J. Henry, at Washington, U.S.A.—Displacement of a body subjected to four conditions, by M. Ribaucour.—On the action of the electric fluid on flames, liquids, and powders, second note, by M. Neyreneuf.—On the detection and estimation of plumbic sulphate in the lead chromates of commerce, by M. Duvillier. The author adds nitric acid and alcohol, the chromate is then reduced, the lead and cromic oxide are dissolved by the nitric acid, and the sulphate, if present, remains insoluble.—On the action of nitric acid on plumbic chromate, by the same author.—On a base isomeric with piperidin, and on the nitrated derivatives of the hydrocarbons of the formula C_mH_m, by M. H. Gal.—On the molecular rotation of gases, by M. Hinrichs.—Experimental researches on the pathogeny of infarctus, &c., by M. V. Feltz.—Observations on a recent note, by M. Rabuteau, relative to the toxic properties of the iodides of tetramethylammonium and tetramylammonium, by Messrs. A. Brown and Th. Fraser.—General results of the analysis of the Geyser springs of the island of San Miguel, Azores, by M. Fouqué. During the meeting an election to the place in the Mechanical section left vacant by the decease of M. Ch. Dupin, was held with the following results:—M. Resal, 31 votes; M. Bresse, 17; M. Boussinesq, 3; M. Haton de la Goupillière and M. Maurice Lévy, 1 each. M. Resal was accordingly declared duly elected.

DIARY

THURSDAY, JUNE 12.

Royal Society, at 4.—Election of Fellows.

Society of Antiquaries, at 8.30.—What Parts of Lincoln Cathedral are really of the Time of St. Hugh of Grenoble, A.D. 1192–1200? J. H. Parker, C.B.

Mathematical Society, at 8.—Some general Theorems relating to Vibrations: Hon. J. W. Strutt.—Invariant conditions of three and four concurrence of three Conics: J. J. Walker.—Locus of the point of concourse of tangents to an epicycloid inclined to each other at a constant angle: Prof. Wolstenholme.

FRIDAY, JUNE 13.

Astronomical Society, at 8.

Quekett Club, at 8.

Horticultural Society, at 3.—Lecture.

Society of Arts, at 12.—Purchase of Railways by the State: Wm. Galt.

SATURDAY, JUNE 14.

Royal Botanic Society, at 3.45.

MONDAY, JUNE 16.

Asiatic Society, at 3.

TUESDAY, JUNE 17.

Anthropological Institute, at 8.—The Ainos: Lieut. S. C. Holland, R.N.—Account of an Interview with a Tribe of Bushmans in South Africa: G. W. Stow.—Specimens of Native Australian Languages: Andrew Mackenna.

Zoological Society, at 8.30.—On the Osteology of the Maltese fossil Elephants: Dr. A. Leith Adams, F.R.S.—On the Geographical Distribution of Asiatic Birds: H. J. Elwes.

Statistical Society, at 7.45.

WEDNESDAY, JUNE 18.

Meteorological Society, at 7.—On some Results of Temperature Observations at Durham: John J. Plummer.—On the Meteorology of New Zealand, 1872: C. R. Marten.—On the Climate of Vancouver Island: Robert H. Scott, F.R.S.—Meteorological Observations at Zi-Ka-Wei, near Shanghai: Rev. A. M. Colombel and Rev. S. J. Perry.—Notes on the Connection between Colliery Explosions and Weather: R. H. Scott, F.R.S., and Wm. Galloway.—Annual General Meeting.

Horticultural Society.—Exhibition.

THURSDAY, JUNE 19.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Linnean Society, at 8.

Chemical Society, at 8.—On the Influence of Pressure upon Fermentation. Part II.: Horace Brown.—Researches on the Action of the Copper-Zinc Couple on Organic Bodies, III., and on Normal and Iso-Propyl Iodides: Dr. J. H. Gladstone and A. Tribe.—On Cymenes from different sources optically considered: Dr. J. H. Gladstone.—On the Action of Bromine on Alizarine: W. H. Perkin.—On some Decompositions and Oxidation Products of Morphine and Codeine Derivatives: G. L. Mayer and Dr. C. R. A. Wright.—On the Decomposition of Tricalcic Phosphate by Water: R. Warington.—On a new Tellurium Mineral, with Notes on a Systematic Mineralogical Nomenclature: J. B. Hannay.—Communications from the Laboratory of the London Institution, No. XII.:—On New Derivatives of Cresol: Dr. H. E. Armstrong and C. L. Field.

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THURSDAY, JUNE 19, 1873

*JEREMIAH HORROX**

II.

IT is now time to pass to the particular incident which has immortalised the name of Horrox, his observation of the transit of Venus over the sun's disc on November 24, 1639 (O.S.) It would have been sufficient for his renown to have been the first witness of the phenomenon, but he had in addition the honour of supplying an omission of Kepler's, who had indeed predicted the transit of 1631, but had failed to point out the occurrence of another eight years subsequently. The transit of 1631 had not been observed owing to its occurrence at night, and that of 1639 had been foreseen by no one save Horrox, and was watched by no one but himself and his friend Crabtree, whom he apprised of the forthcoming event in a letter dated on the October 26 previous.

We borrow Mr. Whatton's account of the observation ("Life of Jeremiah Horrox," pp. 44—46).

> "After having deliberated on the best method of making the observation, he determined to admit the sun's image into a dark room, through a telescope properly adjusted for the purpose, instead of receiving it through a hole in the shutter merely, as recommended by Kepler. He considered that by the latter method the delineation would not be so perfect, unless it were taken at a greater distance from the aperture than the narrowness of his apartment would allow; neither was it likely that the diameter of Venus would be so well defined; whereas his telescope, through which he had often observed the solar spots, would enable him to ascertain the diameter of the planet, and to divide the sun's limb with considerable accuracy. Accordingly, having described a circle of about six inches diameter upon a piece of paper, he divided its circumference into 360°, and its diameter into 120 equal parts. . . When the proper time came, he adjusted his apparatus so that the image of the sun should be transmitted perpendicularly to the paper, and exactly fill the circle he had described. From his own calculations he had no reason to expect that the transit would take place, at the earliest, before three o'clock in the afternoon of the 24th, but as it appeared from the tables of others that it might occur somewhat sooner, in order to avoid the chance of disappointment, he began to observe about mid-day on the 23rd. Having continued to watch with unremitting care for upwards of four-and-twenty hours, excepting during certain intervals of the next day when, as he tells us, he was called away by business of the highest importance, which could not with propriety be neglected, he was at length rewarded for his anxiety and trouble by seeing a large dark round spot enter upon the disc of light."

The "business of the highest importance" was undoubtedly divine service, the transit having taken place on a Sunday. Most modern astronomers of Horrox's profession would, no doubt, have considered the claims of science paramount on an occasion like this. Horrox, in accordance with the feeling of his day, judged otherwise, and when all the circumstances of the case are taken into account, his sacrifice on behalf of what he esteemed a higher duty, must be regarded as an act of extraordinary heroism. He had, it is true, almost convinced himself that the transit could not occur until the afternoon, but even this anticipation was a proof of courageous reliance on his own judgment, being founded on his correction of Kepler's Rudolphine tables, according to the data supplied by which it should have occurred at 8.8 A.M. The phenomenon was also observed by Crabtree, but less perfectly, owing to the cloudy state of the atmosphere at Manchester. A letter from Crabtree on the subject to another north-country astronomer, Gascoigne, contains the remarkable expression, "I do believe there are as rare inventions as Galileo's telescope yet undiscovered."

Horrox did not remain at Hoole much above six months after this great achievement. In July, 1640, we find him again at Toxteth, which he never afterwards left. He must, accordingly, have resigned his curacy, on what account is unknown, as is also the precise nature of his subsequent avocations. We only gather from his correspondence that his affairs were in a very unsettled state, that the duration of his stay at Toxteth was uncertain, and that he was continually called from home. From his complaints of the impossibility of prosecuting his astronomical researches, one would almost surmise that his occupation was nocturnal, especially as he found time for the observations on the tides already referred to. His sustained enthusiasm for astronomy, as well as the generosity of his temper, is touchingly shown in a letter congratulating his friend Crabtree on the success of some observations reported by him: "Your letter alone," he says, "has enough and more than enough to transport beyond all bounds a soul more master of itself than mine. My emotion and gladness are such as you will more easily understand than I express." After several postponements, he eventually fixes January 4, 1641, for a visit to Broughton, but the intention was frustrated by his sudden death on the morning of the preceding day. We learn this from an endorsement by Crabtree, who gives no particulars respecting the cause of death, and who himself, according to Dr. Wallis, only survived his friend for an extremely short period.

We are indebted to Crabtree for the preservation of Horrox's extant papers, those only having escaped destruction which were obtained by him after the writer's death. Of the remainder, part were destroyed during the Civil Wars; part carried to Ireland by Horrox's brother Jonas, who appears to have shared his scientific tastes, and there lost; another portion, after having aided in the compilation of Jeremiah Shakerley's astronomical tables, was destroyed in the great fire of 1666. Crabtree's MSS., happily including the autograph of the "Venus in Sole visa," were purchased after his death by Dr. Worthington, of Emmanuel College, subsequently Vicar of Hackney, and a copy of the "Venus," lent by him to the astronomer Hartlib, having found its way into the hands of Hevelius, was published by the latter in 1662. The Royal Society, just instituted in England, immediately took cognizance of the remainder of the MSS., and having obtained these from Dr. Worthington, placed them in the hands of Dr. Wallis, Professor of Geometry at Oxford, whose Latin translation was ultimately published in 1674. By a judicious arrangement of his materials he was enabled to digest these into a perfect treatise, to which he gave the title of "Astronomia Kepleriana Defensa et Promota." To this he added a translation of the scientific portion of

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* Continued from p. 117.

Horrox's letters to Crabtree, to which we are indebted for most of our scanty biographical information. An inspection of the originals, should these have been preserved, would probably contribute much to clear up doubtful points, and to complete our conception of Horrox's intellectual character. The main outlines of the latter, however, are sufficiently apparent. They comprise a marvellous patience and persistency, combined with wide-reaching activity, a philosophical faculty for generalisation, ambition, enthusiasm, and self-confidence. The versatility of his attainments is attested by the composition of his "Venus" in Latin, by the quotations in his letters from Horace and Juvenal, and by his reference to Raleigh's "History of the World." Of his restless energy and fertility of resource we have proof in the promptitude with which, when debarred from his favourite pursuit, he turns to the investigation of the tides. His grasp of general principles is displayed, among other passages, by a remarkable one in which he speaks of the possibility of illustrating the elliptic orbits of the planets by terrestrial analogies. "To which method of confirmation Kepler is always partial, and most justly, inasmuch as Nature throughout the universe is One, and the general harmony of creation causes the lesser things to be examples of the greater, as the revolution of the moon around the earth is an emblem or imitation of that of the stars around the sun." We have already had occasion to appreciate his enthusiasm; and the self-reliance usually associated with enthusiasm is powerfully evinced in another letter exhorting Crabtree to undertake, in conjunction with him, the preparation of a new set of astronomical tables. From some expressions in this it may be conjectured that he felt hurt at the ignorant comments of his neighbours, and his resentment against his false guide Lansberg, which occasionally transgresses the limits of what would be considered courtesy at the present day, is another indication of a sensitive spirit. When we add to these traits the self-denial manifested on occasion of the transit, and in the temporary renunciation of his astronomical researches in deference to the claims, as seems probable, of his family, we must recognise in Horrox no mere man of science, but a distinct individuality of singular force and attractiveness. His precise place in the scientific world must be left to astronomers to determine; it requires, however, no special knowledge of the science to apprehend that the obscure youth who, under every disadvantage, was able to correct Kepler, might, if only he could have continued at Cambridge, very probably have rivalled him. In him England lost the promise of an astronomer of the first class, which loss, like many a similar one, would have remained absolutely unknown, but for the fortunate conjunction of his name with a phenomenon of regular recurrence and universal interest. If the commemoration of his great achievement cannot be equally universal, it should at least transcend merely local limits. Local patriotism has done its part well; an appropriate memorial has been erected in the church at Hoole, and we are exceedingly indebted to Mr. Whatton for his intelligent memoir and valuable translation of the "Venus in Sole visa." More, however, is demanded, and it would redound to the credit of Horrox's countrymen if, on the December day of 1874, when English watchers scan the skies of another hemisphere for the transit of Venus, Englishmen at home were found dedicating a national monument to the first observer of the phenomenon in this.

JAGOR'S "PHILIPPINE ISLANDS"

Reisen in den Philippinen, von F. Jagor. Mit zahlreichen Abbildungen und einer Karte. (Berlin: Weidmannsche Buchhandlung, 1873.)

THE increasing importance which the Philippines are assuming in both English and American commerce, the comparative insufficiency of the information we possess concerning them, and the beauty and productiveness of nearly the whole region, amply justify the ardour with which the author of this volume has devoted himself to a thorough exploration of the group, and an exhaustive study of every feature of interest appertaining to its component islands and their population. In this very interesting and acceptable work he has given to the world the results of his observation and inquiries, and of these it may be said that, while in point of extent and variety they are sufficiently comprehensive to embrace within their limits every subject of interest or of practical importance to which we should expect to find a place assigned in a book of travels having any pretensions to completeness, they bear the evident impress of the patient, laborious research, and the careful examination and weighing of facts, for which his countrymen are famous.

M. Jagor can hardly be said to be a recent traveller in these islands. His journey through them was made in the years 1859 and 1860, but unforeseen circumstances put a sudden stop to it; and though fully intending to resume it at a later day, that purpose has not yet been accomplished. Although it must be admitted, therefore, that his work does not make its appearance with all that absolute freshness about it to which we are accustomed in these days of ocean steam-navigation, the apparently long interval which has elapsed since his visit has been profitably turned to account by him in the careful study of an immense mass of materials accumulated by himself during his stay, or which he obtained through the Spanish Colonial Minister, or found in the great national libraries of London and Berlin, including a few bulky monkish chronicles, the perusal of which last was a work both long and tedious. In the vast labour incident to the extraction from these various sources of their most important and most interesting details, he has been sustained by a conviction that his subject was worthy of it. He has felt, as he tells us, that few countries in the whole world are so little known or so seldom visited as the Philippines, while none present more agreeable attractions for the traveller, or have been more profusely endowed by the hand of Nature, or contain a larger store of neglected treasure for the natural historian. So strong and so abiding is his faith on this last point, he gravely assures his readers, that even poor travellers would amply cover the cost of their journey by the sale of their collections. Without going so far as to endorse this suggestion in its full and entire significance, it is nevertheless true that the descriptions here given constitute, in the aggregate, a picture of marvellous natural wealth, of which it is on many accounts desirable that modern enterprise should have full and trustworthy information.

The travels recorded in this volume extended through the greater portion, certainly the most important and interesting, of the Spanish Philippines. Manila was the starting point. The author first made a short excursion northwards, thence into the province of Bulacan, and returning to Manila ascended the river Pasig, at the mouth of which it stands, to the great lake of Bay, crossing which he made several journeys into the province of Laguna. Returning thence to Manila, he crossed its magnificent bay, spacious enough to hold all the navies of the world, and proceeding by sea along the deeply indented southern coast of the great island of Luzon, and traversing the Straits of San Bernardino, landed at Albay, the chief town of the large insular province of the same name. From this point he made an excursion into the extreme southern districts of the island, visiting the great volcano of Balusan on his way, and returning to Albay, started thence in a north-westerly direction on a journey through southern Camarines. On this journey many natural features of the highest interest engaged his attention, and notably the great volcanoes of Mayon and Yriga, the Bateo Lake, and the remarkable siliceous wells near Tibi, with the great flat cones called the "white" and the "red," between which they lie—the whole of this district presenting one of the finest examples of calcareous depositions, in various states of advancement, in the whole world. Returning westwards to his main route, he reached Meroce Caceres, near the confines of northern and southern Camarines, and from this point made a considerable digression eastwards, for the purpose of visiting the vast volcano Ysarog, of which, and of the inhabitants of the region, he has given a full and highly interesting description. Again returning to his main route, he arrived at Cabusao on the Bay of San Miguel, and from this point, partly by land and partly by coasting, he explored about forty miles of the eastern coast of this portion of North Camarines, making occasional journeys inland where the prospect of reward seemed to invite attention. Returning to Albay, he embarked at that place for the next important island in this remarkable archipelago, Samar. There he landed at the north-eastern point, and crossing in a south-westerly direction to its western coast, coasted some twenty or thirty miles southwards to Carthalogan. From this place he traversed the centre of the island, and descending the river Ulut, reached the eastern side. He next coasted to its south-eastern extremity, and thence returned westwards, landing at Tacloban, the chief town of the closely adjacent island of Leyte, on which he made a journey many miles to the south. He then traversed the narrow Straits of San Francisco, which separate Samar and Leyte, visiting the ancient rock sepulchres in which the inhabitants of Bisay and some other localities interred the remains of their heroes and their elders. Continuing his return journey by sea, he again reached Manila, after having visited some minor islands, and obtained interesting information relative to them.

It would be vain to attempt, within the narrow limits of space available for our present purpose, anything like a substantial account of the innumerable matters of interest, with which M. Jagor's book deals. The mere enumeration of them would very considerably extend the proportions of an ordinary review, and there are many, very many, which present attractions of the highest order for the geographer, the geologist, the ethnologist, the naturalist, and others who interest themselves in certain special branches of modern science. All that can be done is to indicate a few of the more striking portions of the work, by which its character and completeness may be judged of, referring to the work itself—which we venture to think would well repay translation—those specially interested in its subject.

In his first chapter, the author makes some remarks on the situations of the group, and describes a few amusing circumstances which resulted from the ignorance of Magellan and his followers, of the difference of time depending upon difference of longitude. Such was the injudicious commercial policy of the Spaniards in those dependencies, that the intercourse between them and the Mother Country "was limited to the conveyance of officials and ecclesiastics, and their ordinary necessaries—provisions, wine, and other beverages (Caldos), and, a few French romances excepted, some very dull books—histories of Saints and other similar matters." As regards the aspect of Manila, despite the glowing descriptions of it given by many travellers, the author experienced considerable disappointment; his first impressions being received at a most unfavourable moment, since he landed towards the close of the dry season. The account he gives of the state of society in Manila and its suburbs is anything but inviting. "Life in the city proper can scarcely be agreeable: pride, envy, place-hunting, caste-hatred, are the order of the day. The Spaniards deem themselves superior to their Creoles, who, in their turn, reproach them with coming to the colony only to eat their fill. The same hatred and the same grudge exist between the whites and the half-castes." It appears that cock-fighting is the great pastime of the population. The social, political, and commercial condition of the colony is fully developed in the first four chapters of the book, and in connection with this part of the subject the author ventures on a few reflections on the future of the Philippines. He says:—"Now that the Eastern shores of the Pacific are at length becoming populated, and with unparalleled rapidity are advancing towards their great future, the Philippines can no longer remain in the exclusion which has hitherto been their lot; because, for the western coast of America, there is certainly no tropical Asiatic country so favourably situated; while as regards Australia, it is only in certain relations that Dutch India can dispute precedence with them. Their trade with China, on the contrary, whose staple-market Manila originally was, as also that with the western countries of Asia lying nearer to the ports of the Atlantic, they must for ever renounce."

The fifth chapter is devoted to a very clear and comprehensive exposition of the geography and the meteorology of the Archipelago, the political divisions of the Islands, their various populations, and the languages spoken in them.

On his first journey into the province of Bulacan, the author was much struck with the fertility of the soil, a subject upon which he has a good deal to say, as also upon the contrivances used for fishing. There, too, as in other portions of his route, he became familiar with the ways of Spanish priests, and formed his experiences of native hospitality, besides learning something of the system of wholesale plunder which is carried on almost with impunity, on sea and on land, in this as in all other portions of the islands, where it is likely to pay. It appears, from the author's statements, that piracy is frequent on the coast, and that the country is likewise exposed to gangs of lawless marauders, against whom the Government is almost powerless, while the people are generally deprived of firearms, or, when provided with them, don't know how to use them. Occasionally they make descents upon the land, plundering wherever they go, often accompanying their rapacity with deeds of violence, even murder, and constantly carrying away their victims as prisoners.

Of the land and sea journeys of M. Jagor, generally, it may be said that they are full of incident, and that he never allows anything to escape his notice which may appear to him to be likely to have interest in the eyes of Europeans. From volcanic eruptions to the many odd incidents that presented themselves to him on his journey, nothing is unworthy of his attention, nor beyond his graphic power. His style is at once quiet, simple, and effective, and will delight every reader of German, by the ease with which it portrays the grandest or the most simple objects. He is always deeply impressed with the grandeur of the scenery through which his path lies, heightened as it often is by the beauty and luxuriance of tropical vegetation, and the majesty of primæval forests which extend their dense masses to the sea-margin. The natural productions of the country—animal, mineral, and vegetable—are the subject of copious mention; and in connection with this part of the subject he has been at great pains to examine for himself, and put on record, the industrial and Governmental conditions under which all this mineral and other wealth is, or rather is not, made available for commerce. This is remarkably seen in his chapters on Manila hemp, and on the Government tobacco monopoly.

One of the most curious and interesting portions of the whole book is the twentieth chapter, which describes some remarkable antiquities in the narrow San Francisco strait, a locality whose picturesqueness the author extols, questioning much "whether the ocean anywhere laves a spot of such rich and peculiar beauty." The substance of this chapter, together with a few other portions of the work, has already appeared in Bastian and Hartmann's "Zeitschrift fur Ethnologie." The remains referred to are certain ancient sarcophagi found in cavities in a series of marble-like rocks situated near the eastern entrance to the straits, and in a few other remarkable localities. These rocks rise out at sea to a height of a hundred feet. Their summits are dome-shaped, and their bases are much worn by the action of the sea. In these cavities the ancient Pintados, a race of tattooed Indians, and some other natives of the Archipelago, deposited the remains of their wives and elders as before adverted to. They placed them in carefully closed coffins along with the objects which in life they deemed most precious. Slaves were sacrificed at their burial, in order that they might not be without attendants in the next world. These spots were regarded with superstitious awe by the natives, who believed them to be haunted. A young Spanish clergyman led an expedition to some of the caves, and after some religious ceremonies, wrecked the coffins, and turned their contents into the sea. The superstition still lingers about the rocks, although much weakened. The author had some difficulty in finding men resolute enough to accompany him on an expedition having a somewhat different object in view, that of bringing away some of the relics. He succeeded, however, and the trophies were deposited by him in the Zoological Museum of Berlin University.

Profs. Roth and Virchow have contributed to the scientific portion of the book—the former dealing minutely with the geology of the group, the latter with its ancient and its more recent inhabitants. A copious appendix contains articles treating of the Islands under every possible aspect—historical, antiquarian, commercial, and governmental. The book is handsomely got up, and is printed in Roman characters, now getting more and more into use in Germany, and it is enriched with numerous admirably executed engravings, in various styles, from drawings made by the author on the spot, or obtained by him during his journey. A beautifully executed map is added, and the whole volume may be said to be an important and valuable contribution to the literature of its class.

MILLER'S ROMANCE OF ASTRONOMY

The Romance of Astronomy. By R. Kalley Miller, M.A. (Macmillan & Co., 1873.)

It is in days of strongly marked utilitarianism, when so much is brought into the market that was never intended to go there, and so much of what is there is unfortunately rated at its marketable value only, that corresponding efforts should be made, by those who have the welfare of society at heart, to maintain the due balance of the human intellect by the cultivation of its imaginative faculty. It is here that poetry affords the noblest aid; and even the profusion of modern fiction may be looked upon by the philanthropist with less regret; if only moderately sensible and well-guided, it may lend important assistance in obviating that degeneration which would be the sure result of undue and excessive mental development in any one direction.

The work now before us, a curious little book with a curious title, may in this view of things not be without its value. It is a reprint and enlargement of some popular lectures which appeared in the *Light Blue*; and the author tells us that his object "has not been so much to instruct as to entertain, and possibly in some cases to inspire a taste which might lead to the further prosecution of a most fascinating study; and this will be his apology for passing over many important parts of the subject, and simply selecting a few points here and there which seem to afford scope for striking or amusing amplification." And in pursuance of this design, he brings before us a series of speculations as to the possible condition of other worlds, where fancy is allowed as full a range as the most romantic of readers can desire. As an amusing instance of his peculiar vein, the following passage may be cited: "The part of the moon which appears bright to us must have any moisture which it may contain dried up by his (the sun's) vertical beams; while on the other, or dark side, the ground must be frozen hard to the depth of several feet, the mountains covered with glaciers, and the seas blocked up with icebergs. At the very margin between the two hemispheres there will be a narrow temperate zone, which will of course move round the moon, as the latter turns round its axis and presents its different faces successively to the sun; and the only way in which we can see that life could be supported with comfort at the moon (supposing the atmospherical difficulty surmounted) would be by moving constantly round it, so as to keep always in this temperate zone. A queer Noah's Ark-like sight it would be to see the whole inhabitants of the moon, side by side, in a huge procession extending from pole to pole, and hurrying quickly round it at the rate of ten miles an hour—some riding, some driving, and some travelling in slow railway trains; beasts, wild and tame, galloping by their side, and all the birds of heaven flying along over their heads!" The chapter, too, on Astrology, is of a very diverting character, and above all, Zadkiel's horoscope of the heir-apparent to the British throne.

In the face of such an avowal as the author has made, anything like rigidity of criticism would be out of place: but we cannot help expressing regret that his always pleasing and often beautifully written descriptions should occasionally require the support of a more accurate statement of facts. We have so much respect for his ability, and admiration more especially for the high tone of his principle, as to hope that the book may reach a second edition. but in that case we should hope for the removal of several blemishes which it might seem invidious to point out, but which will be obvious to the scientific reader.

T. W. W.

OUR BOOK SHELF

Proceedings of the Berwickshire Naturalists' Club, Vol. vi. No. 4.

THIS is, we believe, the oldest field-club in existence, and has all along been one of the most efficient and most prosperous so far as numbers and funds are concerned. Its publications, moreover, are already numerous, and contain much valuable material for the natural history, archæology, and antiquities of Berwickshire. There must be already a vast amount of material shut up in the transactions of the now numerous local societies, of the greatest value in reference to the natural history of this country and to students of biology generally, but almost inaccessible except to the members of the various societies. It is a pity that some means could not be devised for bringing the most important contributions to local natural history, in its widest sense, together in some systematised form, so that they could be readily referred to and made available to students at large. Sir Walter Elliot refers to this point in his able address on Provincial Scientific Societies, and it is to be hoped that the Committee appointed by the British Association will give it their consideration. Prefixed to the Proceedings before us is the President's, the Rev. F. R. Simpson's, address, which is wholly occupied with an interesting account of the various meetings of the club during the summer of 1872. For this society is purely a field club, meeting only during the summer months, to explore some of the rich vales of Berwickshire or stretch their limbs over some of the bonny Cheviot fells, gathering rich stores of varied knowledge, and finding a glorious appetite for the substantial dinner which usually winds up the meetings. One of the longest and most interesting papers is by one of the secretaries, Mr. James Hardy, "On Langleyford Vale and the Cheviots," being a sort of survey of the district between Wooler and the base of Cheviot, and containing a wonderful amount of information on the geology, botany, zoology, and especially the prehistoric antiquities of the district. Mr. Hardy also contributes some valuable entomological lists to this part of the Proceedings, and various antiquarian papers; while Mr. Robert Hislop has a list of the rarer Coleoptera occurring chiefly in the parish of Nenthorn. Sir Walter Elliot contributes a list of the diurnal birds of prey hitherto found within the club's limits. There are many other valuable papers including a memoir of the late Dr. William Baird, F.R.S., one of the founders of this old society, appended to which is a list of his many writings. There are two very well executed plates of flint implements and a sculptured stone, and a fine portrait of the Club's late Secretary, Mr. George Tate.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Dr. Bastian's Turnip-Cheese Experiments

IN a former communication* I gave an account of a series of experiments by Dr. Bastian, in which it was established that, "by following his directions, infusions can be prepared which are not deprived by an ebullition of from five to ten minutes of the faculty of undergoing those chemical changes which are characterised by the presence of swarms of Bacteria, and that the development of these organisms can proceed with the greatest activity in hermetically sealed glass vessels, from which almost the whole of the air has been expelled by boiling."

In the first paragraph of that paper I adverted to the importance in every experimental inquiry of defining as completely as possible the method by which any given result can be attained. With this consideration in view, I now propose to give an account of additional experiments, made chiefly for the purpose of elucidating the influence of slight variations of temperature on the result. To guard against the possibility of mistake, it may not be unadvisable to remark that, whereas in the experiments previously reported upon, I took no part, excepting as a witness, I am exclusively answerable for those now to be recorded.

Certain particulars in Dr. Bastian's method have been objected to as possible sources of uncertainty. Thus it has been suggested that when a flask, of which the neck has been drawn out to a capillary orifice, is boiled even for ten minutes over a lamp, it is not certain that the whole of the liquid contained in it is heated to the temperature of boiling; and again, that when the lamp is withdrawn in the act of closing the capillary orifice, germs may enter from without. Although I do not attach much importance to either of these objections, I have modified Dr. Bastian's method, so as to render them inapplicable. The modification, however, applies exclusively to the mode of heating the hermetically sealed flasks. As regards the preparation of the liquid, I have in no respects departed from his instructions.

The liquid is prepared by simmering slices of peeled turnip in a beaker containing about a pint of distilled water. The acid infusion thus obtained, is, if necessary, concentrated by evaporation until it possesses a specific gravity of from 1018 to 1020. It is then filtered and neutralised with sodic carbonate. A little Cheddar cheese is rubbed up with a few drachms of the liquid in a mortar, and the mixture strained through calico. By adding the strained product to the rest of the infusion a turbid liquid is obtained, in every drop of which particles of cheese can be detected by the microscope, although there are scarcely any of a sufficient size to be distinguished by the naked eye.

In the first four sets of experiments retorts were used, in the others flasks. In either case they were charged with the liquid of which the preparation has just been described (their necks having been previously drawn out), boiled over a spirit lamp, and sealed hermetically by directing the flame of the gas blowpipe on the orifice at the same moment that the lamp was withdrawn. The experiments of the first two sets may be regarded merely as more exact repetitions of the former ones. Their results are confirmatory of those previously obtained. In the others the flasks were subjected to the temperature of ebullition under pressures exceeding that of the atmosphere. Although the excess of temperature in no case exceeded two degrees and a half, it will be seen that it exercised a decided influence on the results.

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* See NATURE, vol. viii. p. 180.

The pressures employed varied from one-tenth of an inch to three inches of mercury. According to * Wüllner's table, founded on those of Régnault and Magnus, an excess of 27·63 mill. over the normal pressure (760 mill.), determines an increase of 1° C. in the temperature of boiling, so that here 0°·924 C. corresponds to one inch of pressure. Similarly we have 0°·88 for the second inch, and 0°·873 for the third inch. In other words 100°·92 C. is the temperature of ebullition at one inch, 101°·81 at two inches, 102°·68 at three inches. In describing the experiments I use the expression "turnip-cheese" liquid to denote the neutral infusion of turnip with cheese of which the preparation has been given above; and in recording the results the words barren and pregnant are employed to express the absence or presence of living Bacteria. In any liquid which has been kept five days at the temperature of fermentation there is no difficulty in determining in which of these two conditions it is, for if Bacteria are present at all they are present in such numbers that every field is crowded with them. Bodies which appear to be dead Bacteria are met with here and there in every specimen. They are as numerous in liquids examined immediately after prolonged boiling as in others. They are probably derived from the cheese.

The retorts or flasks were examined after periods varying from three to six days, during which they were kept in the warm chamber at 32° C. Each was tested by observing that when the point of the blow-pipe flame was directed on the neck of the flask the softened part was first drawn in and then gave way with a loud crack.

With these preliminary observations I proceed to give an account of the experiments.

March 1.—Two retorts were charged with turnip-cheese liquid of which the specific gravity was 1017·2, each retort receiving 25 c.c. One was immersed in boiling water in a saucepan for an hour and then placed in the warm chamber: the other was placed in the chamber at once, *i.e.* immediately after it was boiled and closed hermetically. Both were examined on the 4th. The first was barren, the second pregnant.

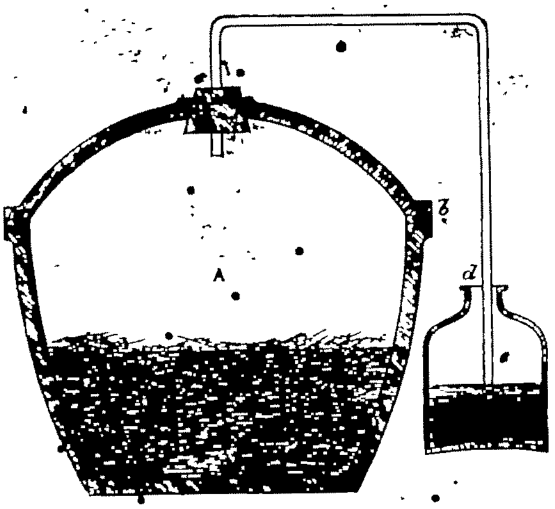
March 4.—Nine retorts were charged with cheese-turnip liquid, sp. gr. 1020. Each contained 35 c.c. After having been boiled and closed hermetically, eight of the retorts were successively subjected in couples to the temperature of boiling water in a digester.

The construction of the digester was such that during the ebullition, which in each case was continued for 15 minutes, steam escaped through various narrow openings. The heating of the retorts was accomplished in four processes, each couple of retorts being heated separately and the valve differently weighted in different cases. Thus in three ebullitions the weights employed were severally 2 lbs., 4 lbs., and 6 lbs., while in the fourth, no weight was added to that of the valve itself. The experiment was planned in this way in order that the influence of pressure might be observed, but in carrying out, it was at once observed that with this view the method was a futile one, for steam escaped so readily in each experiment through the valve that there could be no doubt that all the retorts were in reality subjected to the same temperature, *i.e.* to the ordinary temperature of ebullition. The ninth retort was treated in the same way as the others with this exception, that the ebullition was continued for an hour, the valve remaining open.

The liquids were examined March 10. Of the eight which were immersed in boiling water for 15 minutes, all were pregnant. The ninth retort was barren.

March 13.—Six retorts were charged with turnip-cheese liquid, sp. gr. 1018, after which they were boiled and closed hermetically as before. The retorts were immersed in boiling water as previously, but the experiment was so planned that the pressure under which ebullition took place could be increased at will. The arrangement of the apparatus will be best understood from the diagram. A is a strong iron pot (or digester), the lid of which fits it by a grooved joint *b*. To render the joint air-tight, the groove is filled with white lead before fitting on the lid, which is then tightly wedged into its place. The valve having been removed, the orifice of the digester is stopped by a vulcanite plug, [illegible], through which the long tube passes. This forms a perfect joint; for the greater the internal pressure, the more tightly it fits. The end of the tube dips into mercury contained in a large bottle, *c*, and is retained in its place by a holder at *d*, not shown in the diagram. The pot is half filled with water containing a quantity of hay, among which the flasks are arranged. It is supported on a triangle and heated by a Bunsen's burner, the mercury bottle being raised on blocks until the end of the tube dips to about the right depth under the surface of the metal. A more exact adjustment is attained by means of the holder already mentioned.

In this apparatus the six retorts were subjected to the temperature of ebullition under various pressures, viz., two under a pressure of three inches, two under a pressure of four inches, and two under a pressure of two inches. The period of heating in each case was fifteen minutes. All of the flasks were placed, after heating, in the warm chamber at 32° C., and were examined March 18. All were barren.



March 22.—Four retorts were charged with turnip-cheese liquid, sp. gr. 1019, each receiving 50 c.c. Of these two were heated in the apparatus at the temperature of ebullition under a pressure of one inch, the other two under a pressure of half an inch. Of each couple one was accidentally broken. Both the remaining ones were examined March 27, and found to be barren.

May 7.—Nine flasks were charged with turnip-cheese liquid, sp. gr. 1019, each flask containing 50 c.c. Of these, four were subjected to the temperature of water boiling under pressure of one inch of mercury for thirty minutes, and four for the same period to the temperature of ebullition under one-tenth of an inch. They were examined May 12. Of each set one contained Bacteria, the rest being barren. The liquid in the other flask, which was simply boiled, and closed while boiling, had an offensive smell, and contained much scum.

May 22.—Thirteen flasks were charged with turnip-cheese liquid, sp. gr. 1019·5. Of these six were subjected to the temperature of ebullition at three inches, and six to the ordinary temperature of boiling water. The remaining flask, after having been closed hermetically in ebullition in the same way as the rest, was placed with them in the warm chamber. All the flasks were examined May 26. All the six flasks of the first set were barren. Of the six flasks of the second set four were barren, the others contained innumerable living Bacteria. The liquid in the other flask was offensive, and contained masses of bacterial scum.

After the examination several of the flasks containing barren liquids, particularly those which had been heated under pressure, were replaced in the warm chamber. Some of them were simply closed hermetically (the liquid having been taken for examination by means of a freshly drawn out capillary tube), others were closed after the introduction of a drop of distilled water. On opening these flasks several days afterwards, it was found that those which had been impregnated by the addition of the distilled water were full of Bacteria, the others remaining barren. This was done to show that the liquid, although deprived of its power of germination, is as capable as before of supporting the life of Bacteria.

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* Wüllner, Lehrbuch der Experimentalphysik, B. III., p. 359

The results of the preceding experiments may be summed up as follows:—In sixteen experiments the liquids were subjected to the temperature of boiling at the normal pressure; of these, eight were heated for 15 minutes, and all bred Bacteria; six were heated for 30 minutes, of which two bred Bacteria; two for an hour, both of which were barren.

Of ten subjected to the temperature of ebullition at pressures not exceeding one inch, eight were barren. Both the liquids which were found to be pregnant had been heated for 30 minutes, one under a pressure of one-tenth of an inch, the other of one inch.

In the twelve experiments in which the liquids were heated under pressures exceeding one inch, all were barren, although half of them were subjected to that temperature for only 15 minutes.

It is unnecessary for me to draw any inferences from the preceding experiments; it may not, however, be superfluous to point out that, although all the flasks heated above 101° C. remained sterile, this fact affords no ground for concluding that any definite relation exists between that precise temperature and the destruction of the germinating power of the liquid in question. All that has been shown is that the *chance* that such a liquid will breed Bacteria is diminished either by slightly increasing the temperature to which it is heated, or increasing the duration of the heating. Thus it appears to me quite probable that if a sufficiently large number of flasks were heated even to 102° C., some of them would still be found to be pregnant.

University Coll., London, June 7 J. BURDON SANDERSON

Fertilisation of the Pansy.—Ground Ivy

THERE is one further point in the structure of *Viola tricolor* which is not mentioned by Mr. Bennett or by Mr. Hart, but which seems to confirm the theory of the former gentleman that *V. tricolor*, as distinguished from most other Violas, is fertilised by a small insect such as Thrips instead of by the proboscis of larger insects.

Before I saw Mr. Bennett's paper, my attention had been called by Miss Dowson to the fact that whereas in the Sweet and Dog Violets, the circle of anthers presses close to the style all round, there is in *V. tricolor* an opening between the two appendaged stamens. The use of this opening will evidently be to allow the small creature to enter in and crawl down the stamen to the nectary at the end of the appendage. This structure may be also seen in *V. cornuta*, which seems to be fertilised in the same way. In *V. tricolor* the opening is exactly opposite to the black streak, or guide post, as Mr. Bennett has termed it. In *V. cornuta*, although this black mark is not so evident, there is a distinct triangular mark pointing downwards exposed by the opening of the stamens. On each side of the style are two sets of hairs, looking like "whiskers" to the scull-like crest of the style, on which lots of pollen rest. The small insect on entering the flower can hardly help crawling into the cavity at the top of the stigma, for the entrance to the flower is almost completely blocked up by it. On emerging from it it would crawl over the top, which Mr Hart mentions as seen in *tricolor*, and which I also find in *cornuta*, be guided through the hole by the triangular mark, and so find his way to the nectary. On emerging, covered with pollen, and entering its next flower, it will again be deluded into the *cul-de-sac* in which the stigmatic surface is, where it will deposit its pollen. The details of the structure of the appendaged stamens, as contrasted with those of other Violas, fully bear out this view.

As regards the English translation for the German *bestauben*, I would suggest to Mr. Hart that "pollenate" is an impossible word; *pollen, pollinis*, must give the verb to "pollinate," as *fulmen, fulminis* gives *fulminate*. But there is a great advantage in a word which speaks for itself, and, if the word "be-pollen" offends scientific ears (Mr. Hart does not tell us why), would the literal translation of the German "to be-dust" be offensive? If not, I think it would tell its own tale. The word "empollen" seems justified by *embalm*, but the prefix generally means to place in or convert into, as in *enthral, emprison, embed*. Hence it would at least be ambiguous.

The form of Ground Ivy mentioned by your correspondent S.S.D. grows here abundantly in several spots, seeds freely, and is remarkable for having a much shorter style in proportion to the tube of the corolla than the common form in which the style and stigmas protrude from the tube. F. E. KITCHENER

Rugby, June 15

Mr. Kitchener having been kind enough to send me the above letter, I may, perhaps, be allowed to add a few additional notes. Since writing the former paper I have had the opportunity of examining three other species of *Viola*, *V. calcarata*, *elatior*, and *lactea*, all of which present a remarkable contrast to *V. tricolor* in a very curious point of structure. In *V. tricolor* the stigma is brought into close contact with the lowest petal by a very peculiar "knee" in the style, the effect of which is so completely to close up the central cavity of the flower as to render it extremely difficult for any large insect to insert its proboscis into the spur. In all the three species above-named, which I believe to be fertilised by bees, the style is nearly straight, so as to leave a considerable gap between the stigma and lower petal, quite large enough for the insertion of the proboscis of a bee. In none of these is there the least indication of the black triangular streak on the style which I take to serve, in *V. tricolor*, the purpose of guiding the Thrips to the nectary. The ring of anthers is also perfectly closed, as described by Miss Dowson in the case of the Dog and Sweet Violet, there being no opening for the admission of the small insect, as in the pansy. A striking difference in the form of the stigma also favours the same conclusion as to the mode of fertilisation.

ALFRED W. BENNETT

ON THE ORIGIN AND METAMORPHOSES OF INSECTS*

VI.

THE metamorphoses of insects have always seemed to me one of the greatest difficulties of the Darwinian theory. In most cases, the development of the individual reproduces to a certain extent that of the race; but the motionless, imbecile pupa cannot represent a mature form. No one, so far as I know, has yet attempted to explain, in accordance with Mr. Darwin's views, a life history, such as that of a butterfly, in which the mouth is first mandibulate and then suctorial. A clue to the difficulty may, I think, be found in the distinction between developmental and adaptive changes; to which I have called attention in a previous article. The larvæ of insects are by no means mere stages in the development of the perfect animal. On the contrary, they are subject to the influence of natural selection, and undergo changes which have reference entirely to their own requirements and condition. It is evident, then, that while the embryonic development of an animal in the egg may be an epitome of its specific history, this is by no means the case with species in which the immature forms have a separate and independent existence. If an animal when young pursues one mode of life, and lives on one kind of food, which subsequently, either from its own growth in size and strength, or from any change of season, alters its habits or food, however slightly, it immediately becomes subject to the action of new forces: natural selection affects it in two different and, it may be, very distinct manners, gradually leading to differences which may become so great as to involve an intermediate period of change and quiescence.

There are, however, peculiar difficulties in those cases in which, as among the Lepidoptera, the same species is mandibulate as a larva, and suctorial as an imago. From this point of view *Campodea* and the Collembola (*Podura*, &c.) are peculiarly interesting. There are among insects three principal types of mouth—first, the mandibulate; secondly, the suctorial; and thirdly, that of *Campodea* and the Collembola generally, in which the mandibles and maxillæ are retracted, but, though far from strong, have some freedom of motion, and can be used for biting and chewing soft substances. This type is intermediate between the other two. Assuming that certain representatives of such a type found themselves in circumstances which made a suctorial mouth advantageous, those individuals would be favoured by natural selection in which the mandibles and maxillæ were best calculated to pierce or prick, and their power of lateral motion would tend to fall into abeyance; while, on the other hand, if powerful masticatory jaws were an advantage, the opposite process would take place.

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* Continued from p. 109.

There is yet a third possibility—namely, that during the first portion of life, the power of mastication should be an advantage, and during the second that of suction, or *vice versâ*. A certain kind of food might abound at one season and fail at another, might be suitable for the animal at one age and not at another: now in such cases we should have two forces acting successively on each individual, and tending to modify the organisation of the mouth in different directions. It will not be denied that the ten thousand variations in the mouth-parts of insects have special reference to the mode of life, and are of some advantage to the species in which they occur. Hence no believer in natural selection can doubt the possibility of the three cases above suggested, the last of which seems to explain the possible origin of species which are mandibulate in one period of life and not in another. The change from the one condition to the other would no doubt take place contemporaneously with a change of skin. At such times we know that, even when there is no change to form, the temporary softness of the organs precludes the insect from feeding for a time, as, for instance, is the case with the silkworm. When, however, any considerable change was involved, this period of fasting would be prolonged, and would lead to the existence of a third condition, that of the pupa, intermediate between the other two. Since other changes are more conspicuous than those relating to the mouth, we are apt to associate the existence of a pupa-state with the acquisition of wings: but the case of the Orthoptera (grasshoppers, &c.) is sufficient proof that the development of wings is perfectly compatible with continuous activity; so that in reality the necessity for rest is much more intimately connected with the change in the constitution of the mouth, although in many cases no doubt the result is accompanied by changes in the legs, and in the internal organisation. An originally mandibulate mouth, however, like that of a beetle, could not, I think, be modified into a suctorial organ like that of a bug or a gnat, because the intermediate stages would necessarily be injurious. Neither, on the other hand, for the same reasons, could the mouth of the Hemiptera be modified into a mandibulate type like that of the Coleoptera. But in *Campodea* and the Collembola we have a type of animal closely resembling certain larvæ which occur both in the mandibulate and suctorial series of insects, and possessing a mouth neither distinctly mandibulate, nor distinctly suctorial, but constituted on a peculiar type capable of modification in either direction by gradual change, without loss of utility.

In discussing this subject it is necessary also to take into consideration the nature and origin of wings. Whence are they derived? why are there normally two pairs? and why are they attached to the meso- and metathorax? These questions are not less difficult than interesting. It has been suggested, and I think with justice, that the wings of insects originally served for aquatic and respiratory purposes. From the various modes by which respiration is effected among the different groups of aquatic insects, there are strong reasons for concluding that the original insect stock was, like *Campodea* (Pl 3, Fig. 5), a land animal. But in aquatic insects there is a tendency to effect the purification of the air through the delicate membraneous covering of more or less leaf-like expansions of the skin. In the larva of *Chloeon* (Pl. 4, Fig. 1), for instance, that singularly resembles *Campodea* (Pl. 3, Fig. 5), several of the segments are provided with such foliaceous expansions; which, moreover, are in constant agitation, the muscles of which, in several remarkable points, resemble those of the true wings. It is true that in *Chloeon* the vibration of the so-called branchiæ is scarcely, if at all, utilised for the purpose of locomotion; the branchiæ are, in fact, placed too far back to act efficiently. The situation of these branchiæ differs in different groups; indeed, it seems probable that originally there were a pair on each segment. In such a case, those branchiæ, situated near the centre of the body, neither too much in front nor too far back, would serve the most efficiently as propellers. The same causes which determined the position of the legs would affect the wings also. Thus a division of labour would be effected; the branchiæ on the posterior segments of the thorax would be devoted to locomotion; those on the abdomen to respiration. This would tend to increase the development of the thoracic segments, already somewhat enlarged to receive the muscles of the legs.

That wings may be of use to insects under water is proved by the very interesting case of *Polynema natans*, which I discovered in 1862, and which uses its wings to swim with. This, however, is a rare case; and it is possible that the principal use of the wings was, primordially, to enable the mature forms to pass from pond to pond, thus securing fresh habitats and avoiding in-and-in breeding. If so, the development of wings would tend to be relegated to a late period of life; and by the tendency to the inheritance of characters at corresponding ages, to which Mr. Darwin has called attention,* the development of wings would be associated with the maturity of the insect. Thus the late acquisition of wings in the Insecta generally, seems to be itself an indication of their descent from a stock which was at one period aquatic in its habits, and which probably resembled the present larvæ of *Chloeon* in form, but had thoracic as well as abdominal branchiæ.

If these views are correct, the genus *Campodea* must be regarded as a form of remarkable interest, since it is the living representative of a primæval type from which not only the Collembola and Thysanura, but the other great orders of insects have all derived their origin.

Finally, from the subject of metamorphoses we pass naturally to that most remarkable phenomenon which is known as the "Alternation of Generations:" for the first systematic view of which we are indebted to my eminent friend Prof. Steenstrup.

I have always felt it very difficult to understand why any species should have been created in this double character; nor, so far as I am aware, has any explanation of the fact yet been attempted. Yet insects offer, in the metamorphoses which they go through, a phenomenon not altogether dissimilar, and give a clue to the manner in which alternation of generations may have originated.

The caterpillar owes its difference from the butterfly to the early stage at which it leaves the egg; but its actual form is mainly due to the influence of the conditions in which it lives. If the caterpillar, instead of changing into one butterfly, produced several butterflies, we should have an instance of alternation of generations. Until lately, however, we knew of no such case; each larva produced one imago, and that not by generation but by development. It has long been known, indeed, that there are some species in which certain individuals remain always apterous, while others acquire wings. Many entomologists, however, regard these abnormal individuals as perfect, though wingless insects; and therefore, though these cases appear to me to deserve more attention than they have yet received, I shall not found any argument on them.

Recently, however, Prof. Wagner of Kazan, has discovered that, among certain small gnats, the larvæ do not themselves directly produce the perfect insect, but give rise to other larvæ, which undergo metamorphoses of the usual character, and eventually become gnats. His observations have been confirmed, as regards this main fact, by other naturalists; and there can, I think, be no doubt that they are, in the main, correct.

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* Origin of Species, 4th ed. pp. 14 and 97.

Here, then, we have a distinct case of alternation of generations, as characterised by Steenstrup. Probably other cases will be discovered in which insects undeniably in the larval state will be found to be fertile. Nay, it seems to me possible, if not probable, that some larvæ which do not now breed, in the course of ages may come to do so.

If this idea is correct, it shows how the remarkable phenomenon known as alternation of generations may have originated. At any rate, we find among insects every mode of development; from simple growth on the one hand, to well-marked alternation on the other. In the wingless species of Orthoptera there is little difference, excepting in size, between the young larva and the perfect insect. The growth is as simple and gradual as in any other animal; and the creature goes through nothing which would, in ordinary language, be called a metamorphosis. In the majority of Orthoptera the presence of wings produces a marked difference between the larva and the imago. The habits, however, are nearly the same throughout life, and consequently the action of external circumstances affects the larva in the same manner as the perfect insect.

This is not the case with the Ephemeridæ. The larvæ do not live under the same conditions as the perfect insects; external forces accordingly affect them in a different manner; and we have seen that they pass through some changes which bear no reference to the form of the perfect insect: these changes, however, are for the most part very gradual. The caterpillars of Lepidoptera have even more extensive changes to undergo; the mouth of the larva, for instance, is remarkably unlike that of the perfect insect. A change in this organ, however, could hardly take place while the insect was still growing fast, and consequently feeding voraciously. Nor, even if the change could be thus effected, would the mouth, in its intermediate stages, be in any way fitted for biting and chewing leaves. The same reasoning applies also to the digestive organs. Hence the caterpillar undergoes little, if any, change, except in size, and the metamorphosis is concentrated, so to say, into the last two moults. The changes then become so rapid and extensive, that the intermediate period is necessarily one of quiescence.

Owing to the fact that the organs connected with the reproduction of the species come to maturity at a late period, larvæ are generally incapable of breeding. There are, however, some flies which have viviparous larvæ, and thus offer a typical case of alternation of generations, owing to the early period of leaving the egg, and the action in many cases of external circumstances on the larva different from those which affect the mature form.

Thus, then, we find among insects every gradation, from the case of simple growth to that of alternation of generations; and we see how from the single fact of the early period at which certain animals quit the egg, we can account for their metamorphoses and for the still more remarkable phenomenon that, among many of the lower animals, the species is represented by two very different forms. We may even, from the same considerations, see reason to conclude that this phenomenon may in the course of ages become still more common than it is at present. As long, however, as the external organs arrive at their mature form before the internal generative organs are fully developed, we have cases of metamorphosis; but if the reverse is the case, then alternation of generations often results.

The same considerations throw much light on the remarkable fact, that in alternation of generations the reproduction is, as a general rule, agamic in the one form. This results from the fact that reproduction by distinct sexes requires the perfection both of the external and internal organs; and if the phenomenon arise, as has just been suggested, from the fact that the internal organs arrive at maturity before the external ones, reproduction will result in those species only which have the power of agamic multiplication.

Moreover it is evident that we have in the animal kingdom two kinds of dimorphism.

This term has usually been applied to those cases in which animals or plants present themselves at maturity under two different forms. The different forms of ants and bees afford us familiar instances among animals; and among plants the remarkable case of the genus *Primula* has recently been worked out with his usual ability by Mr. Darwin. Even more recently he has made known to us the still more remarkable phenomenon afforded by the genus *Linum*, in which there are three distinct forms, and which therefore offers an instance of polymorphism.*

The other kind of dimorphism or polymorphism differs from the first in resulting from the differentiating action of external circumstances, not on the mature, but on the young individual. The different forms, therefore, stand towards one another in a relation of succession. In the first case the chain of being divides at the extremity; in the other it is composed of dissimilar links. Many cases of dimorphism under this second form have been described under the name of alternation of generations.

The term, however, has met with much opposition, and is clearly inapplicable to the differences exhibited by insects in different periods of their life. Strictly speaking the phenomena are very frequently not alternate, and, in the opinion of many eminent naturalists, they are not cases of generation at all.†

In order, then, to have some name for these remarkable phenomena, and to distinguish them from those cases in which the *mature* animal or plant is represented by two or more different forms, I think it would be convenient to retain for these latter exclusively the terms dimorphism and polymorphism; and those cases in which animals or plants pass through a succession of different forms might be distinguished by the name of dieidism or polyeidism.

The conclusions, then, which I think we may draw from the preceding and other considerations are:—

1. That the occurrence of metamorphoses arises from the immaturity of the condition in which some animals quit the egg.

2. That the form of the insect larva whenever it departs from the original vermiform type, depends in great measure on the conditions in which it lives. The external forces acting upon it are different from those which affect the mature form; and thus changes are produced in the young which have reference to its immediate wants, rather than to its final form.

3. That metamorphoses may therefore be divided into two kinds, developmental and adaptational.

4. The apparent abruptness of the changes which insects undergo arises in great measure from the hardness of their skin, which admits no gradual alteration of form, and which is itself necessary in order to afford sufficient support to the muscles.

5. The immobility of the pupa or chrysalis depends on the rapidity of the changes going on in it.

6. Although the majority of insects go through three well-marked stages after leaving the egg, still a large number arrive at maturity through a somewhat indefinite number of slight changes.

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* Of course all animals in which the sexes are distinct are in one sense dimorphic.

† "There is no such thing as a true case of 'alternation of generations in the animal kingdom, there is only an alternation of true generation with the totally distinct process of gemmation or fission"—*Huxley on Animal Individuality*, Ann. and Mag. of Nat. Hist., June 1852.

7. When the external organs arrive at this final form before the organs of reproduction are matured, these changes are known as metamorphoses; when, on the contrary, the organs of reproduction are functionally perfect before the external organs, or when the creature has the power of budding, then the phenomenon is known as alternation of generations.

8. Thus, then, it appears probable that these remarkable phenomena may have arisen from the simple circumstance that certain animals leave the egg at a very early stage of development, and that the external forces acting on the young are different from those which affect the mature animal.

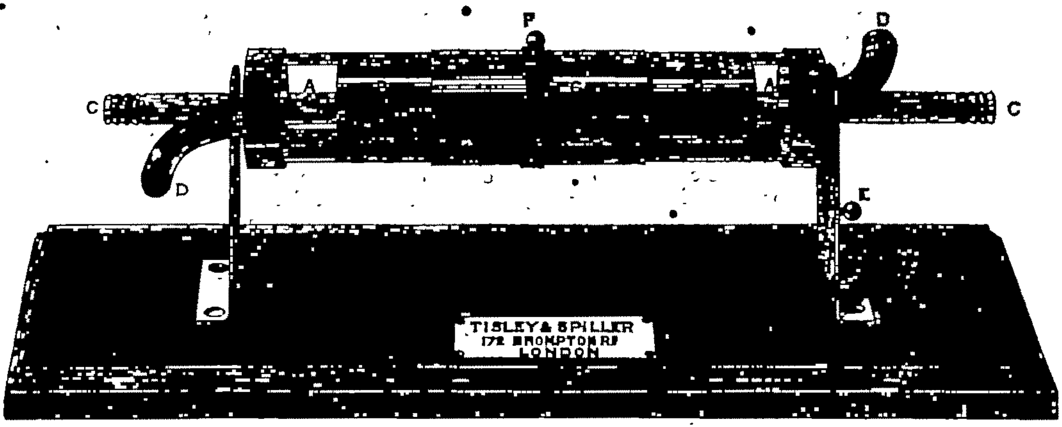
JOHN LUBBOCK

(*To be continued.*)

ON AN IMPROVED FORM OF OZONE GENERATOR

A SHORT description of an improved form of ozone generator, exhibited at the last meeting of the Chemical Society, may perhaps be interesting to the readers of NATURE.

Probably no apparatus hitherto introduced, for the production of ozone by electric induction, has in its working given universal satisfaction. The original form of "Siemens' tube" has many disadvantages, amongst which the chief are, the extremely fragile nature of the two glass tubes, especially when sealed together by the blowpipe, and the fact that if the apparatus be worked for any length of time it becomes heated, thereby causing a diminution in the quantity of ozone obtained. The arrangement of a number of glass plates coated on alternate sides with tin foil, and enclosed in a box, known as "Beane's" instrument, possesses,—especially if used as it is intended it should be with a large and powerful coil—this latter disadvantage to a considerable extent. Sir Benjamin Brodie, during his researches upon ozone, used a modification of "Siemens' tube," which in a great degree overcame this difficulty. Two glass tubes closed at one end, and of such diameter that one was capable of sliding within the other, were fixed together in that way, the junction being effected either by the blowpipe or by means of paraffin, thus leaving a small annular space between them through which the oxygen or other gas to be ozonised could circulate; tin foil coatings were dispensed with altogether, the inner tube being filled with water, and the whole apparatus stood in a vessel of water, wires in conection with an induction coil being placed in the interior tube, and also in the outer vessel: this water could be kept cool by ice, and thus any heat produced during the time it was in use was successfully neutralised. Such an apparatus works exceedingly well but requires delicate handling, and is not perhaps very well adapted for having other pieces of apparatus attached to it.



New Ozone Generator.

This new instrument is an improved modification of the above, but permits of a continuous stream of water of any required temperature being maintained through it; and further, the annular space which in the case of glass tubes is often very irregular, causing thereby an unequal electrical discharge, is made as true as possible, and the result is a more uniform conversion of the gas into ozone. The apparatus as at present made will be better understood by the following description referring to the accompanying diagram:—A A is a piece of glass tube of a little more than one inch in diameter, and of as uniform a bore as can be obtained. On each end of this tube is placed a brass cap, bored with two holes, and coated internally with shellac; in the interior of this glass tube and of a diameter scarcely less than that of the tube itself, but not quite so long, is placed a thin hollow brass box, B B, with its surface made as true as possible by turning in a lathe; this brass box is placed concentrically with the outer tube and is completely coated on its exterior surface with tin, the tin being acted upon to the smallest extent by the ozone. This hollow box communicates with the exterior of the apparatus by means of the tubes C C passing through the centre of the caps. It is intended that a current of water shall be kept circulating through the interior of this box, the water being brought into direct contact with its sides by means of a small spiral placed within it, the box being of a slightly less diameter than the glass tube, a small annular space will remain between the two, and through this space the gas to be ozonised is passed by means of the tubes D D; the box itself is made one of the electrified surfaces, and a strip of tin foil G, fixed to the outside of the glass tube, forms the other; two binding screws, E and F, serve to make the necessary connections with an induction coil.

The production of ozone by this apparatus is exceedingly regular and constant. No quantitative estimations with iodide of potassium and sulphurous acid have as yet been made with regard to the amount of ozone obtained, but an approximate experiment upon the quantity of indigo bleached in a given time, seems to indicate that this amount is quite equal to, if not rather in excess of, that obtained when the ordinary apparatus is used. This instrument possesses also some minor advantages; it is not so easily broken, other pieces of apparatus are very readily attached to it, and at the same time its cost is less. There appears to be no reason why larger forms should not be manufactured upon the same principle. These instruments are made by Messrs. Tisley and Spiller, of Brompton.

THOS. WILLS

THE LAW OF STORMS DEVELOPED*

II.

IT has been asserted lately that the Gulf Stream has no influence upon storms; that they have no tendency to run toward it or to run upon it; and that what geographers and seamen have always said about the Gulf Stream as a "weather-breeder" and "storm-king" is absurd. I think it can be demonstrated that this well-known popular belief is not absurd.

It is an observation as old as Aristotle, that the storms of the middle latitudes in the Northern Hemisphere advance from west to east. This is obviously partly due to the fact that the winds on their eastern sides are southerly, that they come from the equatorial regions, and hence are highly charged with aqueous vapour. This vapour is absolutely essential to the sustenance of the storm. Moreover, the law of storms requires that the southerly winds should enter the storm-vortex on the eastern side, and as this is the side on which the greatest quantity of vapour is found, and the side of greatest condensation, of the greatest evolution of latent heat, hence of the greatest aerial rarefaction and barometric fall, to this side the heavier air from the west will push as into a great hollow. Thus do we actually find that all storms, formed west of the Gulf Stream, are actually propagated toward it. It may be argued from the above facts that the anti-trade winds are thus maintained by storms incessantly making the circuit of the globe within the temperate zone. But in reality, instead of being the effect of storm influence, the anti-trades are originated by independent solar agency, as are the trades, and they are potential and causal in producing the eastward progression of all cyclones. It must be conceded that the pressure of vast aërial currents does serve to force the meteor along with them as the river-eddy is carried down stream with the water-current; otherwise it is impossible to explain the westward progression of tropical hurricanes. While yet in the band of easterly trade-winds the storm will invariably work its way or be propagated toward *the most humid region*, unless mechanically borne in another direction by the great atmospheric current in which it is often embedded as an eddy in a river. The cyclone-tracks over all the oceans lie in the central bands of the great ocean-currents of high temperature and great evaporation, and the band of cyclonic violence is often beautifully coterminous with the sharply-marked blue-tinted edge of the Gulf Stream. Thus, in the Pacific, the Loochoo Islands lie just in the path of the Kuro Siwo, the great Pacific Gulf Stream of the Japanese, and are visited by the most fearful typhoons; but the Bonin Islands, in the same parallel, but on the extreme margin of the Kuro Siwo, have very mild and moderate storms.* "If a storm commences anywhere in the vicinity of the Gulf Stream, it naturally tends towards that stream, because," as Loomis says, "here is the greatest amount of vapour to be precipitated, and when a storm has once encountered the Gulf Stream, it continues to follow that stream in its progress eastward." Vessels and Japanese junks, dismasted in gales off the Asiatic coast, have been drifted for many days in the current of the Kuro Siwo, to the coast of California, just as West-Indian beans, cocoa-nuts, and vegetables, have been drifted to Iceland, Greenland, and Spitzbergen, on the extension of the Gulf Stream. According to all meteorological observations of the tracks of storms, we are warranted in believing that cyclones and hurricanes do, as a matter of fact and of atmospheric law, run on the hot currents of the sea as naturally as the watercourse clings to its bed. Practical seamen, though unable to

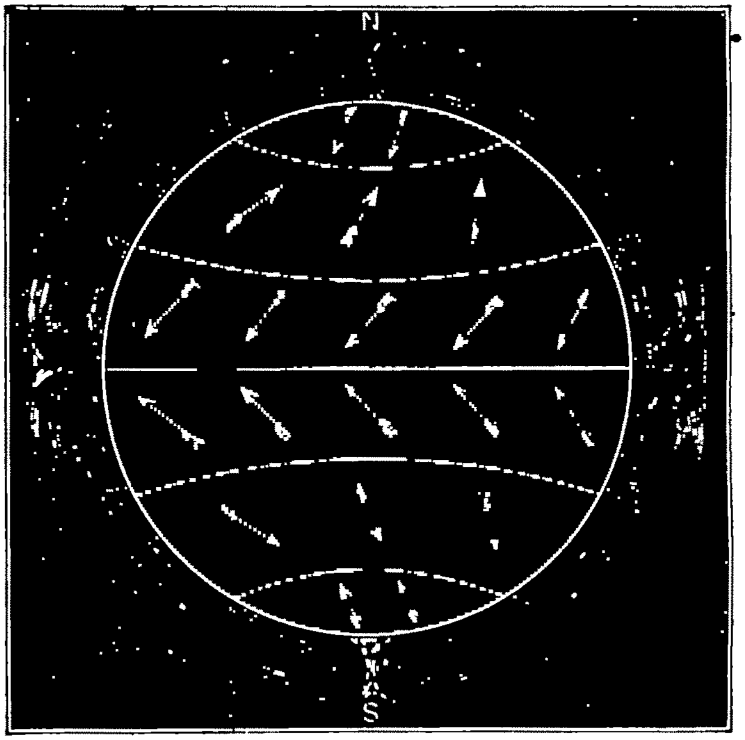


FIG. 3.—The Atmospheric Movements.

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* Continued from p. 124.

* See Redfield's Report on Pacific Cyclones.

explain the fact, are always on the look-out for these furious gales when sailing on the axial lines of the Gulf Stream, on the hot Mozambique current (the Gulf Stream of the Indian Ocean), and on the dark superheated waters of the Kuro Siwo of the Pacific.

So dangerous and disastrous are the storms which course along the Gulf Stream that sailors avoid it, and the American Sailing Directions and those of the British Admiralty advise all vessels, sailing from the West Indies to New York or Liverpool, to beware of taking advantage of its current, although it would help them along from three to four miles an hour. Close observation has traced these storms continuously from the Florida coast to New York, through Redfield's labours, and thence to England, through the records of the Cunard steamships, and thousands of detached observations.

We have now reached a point where we can properly and intelligently consider a question that has always baffled meteorologists—the origin of cyclones. The diagnosis of the phenomenon necessarily precedes its explanation. This subject has engrossed many minds, and various have been the ingenious devices for unravelling its mystery. Mr. Redfield—the father of storm physics—in his modesty and diffidence, so distrusted himself and in his day so keenly felt the need of a more enlarged induction of facts, that he has scarcely left us his opinion. The theories of other writers have all long since been abandoned by themselves or suffered to drop from the notice of the scientific world as evidently incapable of explaining the phenomena of cyclones. This has been the fate of them all, unless possibly we except the theory advanced by the great meteorologist, M. Dové, of Berlin. Briefly stated, the latter hypothesis is this (at least in its application to West Indian hurricanes), viz., that "they owe their origin to the intrusion of the upper counter trade-wind into the lower trade-wind current" (Dové's "Law of Storms," p. 264).

Without pausing here to examine this theory upon its merits and upon the facts, we hasten to mention a different hypothesis advanced, nearly two years ago, as a substitute for that of M. Dové, and as affording an entirely original and satisfactory explanation of the origin of cyclones.

The hypothesis was likewise based upon the agency of the trade-winds, but in a manner wholly different from that elaborated by the German meteorologist. In the original paper in which my views were published, the following statement was made:—"It can be demonstrated that the origin of cyclones is found in the tendency of the south-east trade-winds to invade the territory of the north-east trades, by sweeping over the equator into our hemisphere."

The hypothesis advanced, in lieu of another seemingly less satisfactory, claimed to rest upon observations conducted in the very region most notorious for the generation of cyclones.

To test this, we need only to examine the Atlantic trade winds.

Theoretically, physical geography has generally represented the motions of the atmosphere somewhat as is represented in the accompanying diagram (Fig. 3) of the winds, as projected by Prof. William Ferrel, of Cambridge. The elaborate pages of Prof. Coffin, in his invaluable volume on the "Winds of the Northern Hemisphere," as deduced from myriads of observations, show that the graphic illustration furnished by the diagram is approximately correct.

The region of the trade winds, it will be seen, more than covers the torrid zone of the earth, and at all the seasons of the year overlaps both the northern and southern tropics. While this is theoretically true, and is usually put forth as a fact, it must be accompanied with one or two important qualifications and additions.

Let us see what these are. The well-known oscillation or swinging of the belts of winds to and fro on the meridians, which is kept up in never-ceasing response to the apparent annual motion of the sun as he crosses and recrosses the equator, must ever underlie the conception we form of the trade winds and be perpetually present to the mind's eye. This oscillation has never yet received the popular attention it needs. The sun traverses (apparently) an arc of 23½° on either side of the line; and we might, *a priori*, suppose that the thermal or meteorological equator, the thermal or meteorological Tropics of Cancer and Capricorn, and all those phenomena which lie between them and beyond them, move over an arc of as many degrees as they traverse. Such an inference, however, is not borne out by observation, and we propose to confine ourselves strictly to what may be proved by observation. It is clear that the trade-wind belt does traverse or vibrate over a wider zone than any physicist has yet assigned to it, which is not more than ten degrees of latitude north and south respectively of the Tropic of Cancer and that of Capricorn. These winds, when first experienced by Spanish sailors, gave, to that portion of the Atlantic over which they blew, the name *el Golfo de las Damas* (the Ladies' Sea) because they rendered navigation so easy that a girl might take the helm. But, "gentle" as they are, they have a wide sweep, and, in the summer of the Northern Hemisphere, extend far beyond the Tropic of Cancer. They have often been distinctly felt at Madeira and the Azores (near the 40th parallel) in summer, and it is highly reasonable to suppose that they then fully reach the latitude of 40° N. The equatorial side of the north-east trade-wind belt, of course, vibrates with the sun. In summer it stretches along between the 10th and 12th parallels of north latitude, verging in August on the 13th parallel, and, according to one writer, occasionally the north-east trades at that season do not extend south of the 15th parallel of north latitude. Dampier, "the prince of navigation," as the English call him, gives the direction of the wind in the summer months, between the equator and 12° north, as south-south-east, south-south-west, and south-west.

The equatorial side of the north-east trade-wind belt in winter approaches very nearly to the equator, and may be located in January at least as far south as the latitude of 2° north.

The freshest trade-winds in the North Atlantic are generally found between the parallels of 10° and 25°, and by long protracted experiment in seamanship they have been found to have an average propelling power, when the wind is taken just abaft the beam, of about six knots an hour. But, of course, the northern boundary of the south-east trade-wind likewise varies and vibrates with the seasons. So also, and under the same condition, does the southern boundary of this trade vary and vibrate with the seasons. Its normal and mean position is a little south of the parallel of 25° south, but in the winter of our hemisphere it is pushed much farther south, and in the vicinity of 35° south latitude. The charts of Captain Wilkes give easterly winds for the east coast of Australia, and also for the south coast of Africa. Sir John Herschel, speaking from knowledge gained by his long residence at the Cape of Good Hope, tells us that there "the south-easterly winds which sweep over the Southern Ocean, infringing upon the long range of rocks which terminates in the Table Mountain, is thrown up by them, makes a clean sweep over the flat table-land which forms the summit of that mountain (about 3,850 ft. high), and thence plunges down with the violence of a cataract" ("Meteorology," p. 96).

From these high southern latitudes, we must conceive the motion of the south-east trades, extending northward in summer to the neighbourhood of the parallel of 10°.

T. B. MAURY

(*To be continued.*)

THE CORONAL ATMOSPHERE OF THE SUN*

II.

WHEN the subject is a phenomenon so complex as that of the corona, it is necessary to bring to bear upon it various methods of study. This is why I have thought it indispensable to consider the corona from the triple standpoint of its aspect, the analysis of its light, and its polariscopic manifestations. Let us discuss these varied observations.

Let us first of all see what can be learned from an examination of the corona during the first instants of totality. We have seen that the general structure of the corona persisted throughout the duration of totality. We cannot, then, admit here any effect of diffraction engendered at the surface of the lunar screen by the rays grazing the edges of that screen Let us revert to the geometric circumstances of a total eclipse. At the moment when totality is produced, the disc of the moon is tangent, at one point, to that of the sun, and edges off gradually more and more to the opposite point. Diffraction will be produced, then, under physical conditions the most different, at various points of the lunar limb, and an aureole due to that cause will reveal, by its dissymmetry, such a diversity of conditions. Moreover, an aureole of this kind will present a continually varying aspect during the various phases of totality. Unsymmetrical at the outset, it will be modified with the movement of the moon, and will tend to assume the same form all round our satellite, when the disc of the latter is equidistant from that of the sun. Finally, from that point this aureole will pass through the same phases inversely until the reappearance of the sun.

However, nothing like this was produced at Shoolor. The general structure of the corona remained the same throughout the continuance of totality.†

It is unnecessary to dwell on the hypothesis of an aureole produced by a lunar atmosphere. We know now that if a gaseous layer exists on the surface of our satellite, it must be of so small extent that the grand phenomenon of a corona could not be produced by it.

Our own atmosphere cannot be adduced as the cause of the phenomenon, though it is evident that it plays an important part in the particular aspects which the corona may present at different stations, according to the state of the sky at these stations. It acts as a modifying, but not as a producing cause.

Let us pass, meanwhile, to the spectroscopic observations. The corona presents the hydrogen lines throughout all its visible extent; in certain parts as far as to 12′ or 15′ in height. This observation is certain. The precision of the spectroscopic scales, the experience we have had in such determinations, and the care which was taken in the last observation to compare the lines of the corona with those of a protuberance, of which they are only a prolongation, leaves no doubt as to this point.

But if the corona presents the hydrogen lines, we must ask this testing question—Is this light emitted or reflected? The constitution of the coronal spectrum will afford us an answer.

If the light of the corona is reflected, this light can only have a solar origin. It proceeds from the photosphere and the chromosphere, and its spectrum ought to be that of the sun, that is, a luminous ground with obscure lines. But such is not the constitution of the coronal spectrum; that presents to us the hydrogen lines standing in strong relief on the ground; after the green line (1474) this is the most striking manifestation in the phenomenon. We must conclude that the coronal medium is self-lighted, in great part at least, and that it contains incandescent hydrogen. This first point is conclusively established. But is it to be inferred from this that the whole of the light of the corona is emitted light? Evidently not; and on this point a delicate observation in spectrum analysis and polarisation may inform us. In fact, the spectrum of the corona presented to me, besides these bright lines, many obscure lines of the solar spectrum, the line D, and some in the green. This fact proves the presence of reflected solar light. We may ask why the principal Fraunhofer lines are reduced to the line D. It should be remarked that the coronal spectrum, not being very luminous, is especially perceptible in its central part, and that, in this part, the lines C, F, &c., are replaced by the bright lines. In these conditions the line D alone remains important; thus it is on it I have directed all my attention. As to the finer lines, they were much more difficult to discern, a fact very easily explained by the very large opening I was obliged to give to the slit of the spectroscope.

The proof of the existence of the Fraunhofer lines in the spectrum of the corona is a work of delicacy; it was not obtained by the other observers. This fact is explained partly by the great purity of the sky at Shoolor, partly by the power of my instrument. I have no doubt that the observation will be confirmed by astronomers who work under conditions equally favourable.

The presence of reflected solar light in the spectrum of the corona is of great importance; it shows the double origin of this coronal light; it explains observations of polarisation which appeared irreconcilable;* but above all, it enables us to understand how the solar light forming in some sort the ground of the spectrum of the corona, this spectrum may be considered continuous; and we know that hitherto this circumstance has been the great obstacle which prevented the corona from being regarded as entirely gaseous. The phenomena of polarisation presented by the corona are for the most part those of radial polarisation, which shows that reflection takes place chiefly in the corona, and that that which may be produced in our atmosphere is only secondary. Polarisation then agrees here with my observation of the Fraunhofer lines; but in order that the agreement may be complete, it is necessary that the polariscopic analysis, like the spectral analysis, should show that the light of the corona is only partially reflected. This is precisely what happened. We have seen, in fact, that near the limb of the moon, where the coronal light is brightest, polarisation appears less pronounced than at a certain distance. The reason is, that in the inferior regions emission is so strong that it conceals reflection, and the latter appears, with its peculiar characteristics, only in the layers where it is able to assume a certain relative importance.

Thus the two analyses, spectral and polariscopic, fairly interpreted, agree as to the double origin of the coronal light, and all the observations unite in demonstrating the existence of this circumsolar medium. This medium is distinguished both by its temperature and by its density from the chromosphere, of which the limit, moreover, is perfectly distinct, as is shown in all the drawings of the protuberances and of the chromosphere. There is thus a necessity for giving it a name: I propose that of "coronal envelope" or "coronal atmosphere," to remind us that the luminous phenomena of the corona owe to it their origin.

The density of the coronal atmosphere must be excessively rare. In fact, it is known that the spectrum of the chromosphere in its superior parts is that of a hydrogen medium successively rarified; but as the coronal medium, according to the indications of the spectrum, ought to be even infinitely less dense, we see how rare this medium must be. This conclusion is further corroborated by astronomical observations. Science has recorded the passage of comets as only some minutes' distance from the surface of the sun; these bodies must have traversed the coronal atmosphere, and yet, notwithstanding the lightness of their mass, they did not fall into the sun.

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* Continued from p. 127.

† It is quite evident that this constancy of aspect only agrees with points of general structure sufficiently distant from the sun not to be influenced by variations of light resulting from the displacement of the moon, relatively to the low and very luminous regions of the chromosphere.

* If we consult the history of eclipses we shall see that observers have often obtained contrary results, which has been the means of casting a kind of discredit on this kind of observation. But if these observations are considered in view of the double nature of the light of the corona, and of the effects of our atmosphere, we shall be able to remove most of the difficulties,

I shall add here, as to the constitution of the coronal atmosphere, a few ideas which do not rigorously flow from my observations, but which appear to me very probable, but upon which the future must pronounce.

I said, *à propos* of the observations in the telescope, that the corona was shown at Shoolor with a form almost square, and that it was distinguished by gigantic dahlia-like petals. It is a fact that in each eclipse the figure of the corona has often varied; it has exhibited the most eccentric appearances. I have no hesitation in saying that this medium, now incontestably recognised, and which I propose to name the "coronal atmosphere," very probably does not represent the whole of the aureole which is seen during total eclipses. It is quite credible that portions of the rings or trains of the cosmical matter then become visible and thus tend to complicate the figure of the corona. It belongs to future eclipses to instruct us on this point. But with regard to the coronal medium itself, there is no doubt that it presents singular forms, which convey but little idea of an atmosphere in equilibrium. Moreover, I am inclined to admit that these appearances are produced by trains of very luminous and dense matter from the superior layers ploughing this troubled medium. The protuberant jets, which carry the hydrogen to such great heights, must have a peculiar influence upon this coronal medium, whose density is quite comparable to that of the cometary media.

It is, then, very probable that the coronal atmosphere, like the chromosphere, is very much agitated, and that it changes its shape very rapidly, which will explain how it presents different appearances every time it has been observed.

To repeat: I have been able to establish at Shoolor, by trustworthy and consistent observations, that the solar corona presents the optical characteristics of incandescent hydrogen gas, that this very rare medium extends to very variable distances from the sun, from half a radius of the sun to about double that at certain points; but I give these figures only as results of an observation, not as definitive. It is quite certain, moreover, that the height of the corona must be necessarily variable.

This result seems to be a considerable advance in the general problem of the corona. If our foreign rivals have not obtained a result so decisive* as those of the French mission, I believe it must be attributed to the altogether exceptional purity of the sky in the station which I chose with such pains, and also to the combined optical arrangements which gave to the luminous phenomena which it was the object to catch, an exceptional power.†

JANSSEN

CHRONOMETER TESTS

THE following, which has been sent us by the Scientific Editor of *Harper's Weekly*, shows with what minuteness the scientific work of this country is studied in America, and what a critical audience we have on the other side of the water:—One of the most important services that astronomy has rendered to mankind consists in the contributions it has made to the progress of navigation, and the increased security of life and property. In this field England has always taken the lead, and the efforts of Mr. Hartnup at Liverpool are a worthy continuation of the labours of Flamstead, Bradley, and Airy. While the Greenwich Observatory has caused a great improvement in the general standard of the chronometers bought for the use of the Government vessels, Mr. Hartnup has sought to effect a similar reform for the mercantile marine. He has insisted on the vital importance to ship-masters, as well as to owners and insurance companies, of the careful determination of the rates of their chronometers as affected by temperature. The makers of these instruments and the astronomers who use them carefully have always known that which captains of vessels have been very slow to profit by—*i.e.* that the chronometers are, when made, so adjusted that they keep perfect time at two temperatures, such as 55° and 85° F., while between these limits they gain, and beyond them they lose, on the true time. It is rare that this variation in the chronometer rate can be safely overlooked by a careful navigator, though it is frequently done by those whose vessels do not carry a precious burden of 1,000 or 2,000 souls. The only excuse for this neglect is the positive assurance of the maker that the chronometer is perfectly reliable—an assurance that is often fortified by very deceitful figures. The difficulty and expense of a searching investigation into the errors to which every chronometer is liable have long been supposed by the trade to stand in the way of the introduction of such chronometers only as were of approved reliability. In order to obviate the difficulty as far as possible, the Liverpool Observatory has been constructed by Mr. Hartnup specially for the purpose of studying the rates of the chronometers that may be sent thither by captains sailing from that port. The expense of the examination given to such chronometers is comparatively trifling; and the number of chronometers submitted to him has annually increased, until by reason of the recent regulations at that port the number of examinations has amounted to between 1,000 and 2,000 annually, the same instruments having been repeatedly submitted to him. The process pursued by Mr. Hartnup consists in exposing each chronometer for a week to a uniform temperature of 55°, and determining its rate each day; it is then for another week exposed to a temperature of 70°, and then to one of 85°; the next week it is returned to the temperature of 70°, and the last or fifth week it is exposed to the temperature of 55°, as at first. By means of general laws regulating the rates of chronometers it is now possible to determine what the rate will be at other temperatures than the three above mentioned, and knowing these, the navigator is able to apply the proper correction to his time-keeper so exactly that he need never mistake his position upon the ocean.

The records of the Liverpool Observatory for the past year show—1. That the rates of about 10 per cent. of the chronometers tested (those of the mercantile marine very generally have the ordinary compensation balance) are so irregular as to render the instruments entirely unfit for nautical purposes. 2. The error of adjustment for temperature of the remaining 90 per cent. is often so erroneous as to produce a change of daily rate of many seconds, when the temperature varies but little from either of the two standard points of 55° and 85°, or thereabouts. 3. That the best made and most carefully adjusted instruments gain, on the average, daily six-tenths of a second more at a temperature of 70° than at 55° or 85°. 4. That those that have the same rate at 55° and 70°, or at 70° and 85°, lose when exposed to temperatures beyond these limits at the rate of 1·5 seconds daily for a change of 15° in temperature. 5. That when the connection between temperature and daily rate has been well determined, it will remain constant in good instruments for a long time, which need in general to be examined only once in one, two, or three years.

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* M. Respighi, at Poodookotah, made observations purely spectroscopic which confirm mine; only he found the height of the corona much less, which appears to me to be due to the more feeble luminous power of his instrument.

† This paper contains only an analysis of my observations. I have not been able to refer in detail to those of other observers. I may cite, however, the important remarks of Mr. Lockyer on the structure of the corona, the photographs of Colonel Tennant, the polariscopic observations made at Jafna, those of Capt. Fyers, M. Oudemans, and others.

The vital importance of this subject to the interests of safe, speedy navigation, will be impressed upon everyone by the disaster that befell the *Atlantic*, consequent upon being some twenty miles (or ninety seconds of time) out in her reckoning.

NOTES

LAST Thursday the gentlemen already named by us were elected Fellows of the Royal Society.

THE Baly Medal for physiological research has been awarded to Dr. Sharpey.

A PORTION of the collection made by the naturalist D'Albertis in New Guinea, and referred to in our Notes last week, has already arrived in England, and at the meeting of the Zoological Society, on Tuesday, June 17, Mr. Sclater, F.R.S., announced that among other valuable species, it contained both male and female specimens of a previously unknown Bird of Paradise of the Epimachine division, with a peculiarly long and curved beak, which he proposed to name *Drepanephorus albertisi*, after its discoverer.

A PROJECT has been set on foot by Colonel Grant, so well known from his African travels, to form a loan exhibition of skulls and horns of hollow-horned animals, in order that by observation and comparison of a large number of characteristic specimens, facts may be obtained regarding the form, sexual characters, and locality of each particular species. It is proposed to have as many as from twenty to fifty specimens of each species, so as to be able to form groups representing every stage in the life of each, as also to show the varieties of species in different localities. When from three to five thousand specimens of the one hundred and fifty existing species has been promised, means will be taken to secure the most suitable place in London for their exhibition.

ARRANGEMENTS have been made, under the sanction of Dr. Whewell's friends and executors, for the publication of a life of the late Master of Trinity, with selections from his correspondence and remains. The literary and scientific remains and correspondence will be edited by Mr. Todhunter, Lecturer, and ormerly Fellow of St John's College, Cambridge. The account of Dr. Whewell's college and university career will be written by Mr. W. G. Clark, Senior Fellow of Trinity College, Cambridge. Some of the most distinguished of Dr. Whewell's friends, to whom application has been privately made, have kindly placed their papers at the disposal of the editors, and expressed their approbation of the proposed work. The editors now ask in a more public manner for the loan of letters or other materials which will assist them in their labours. Mr. J. L Hammond, Fellow of Trinity, as the surviving executor under Dr. Whewell's will, has undertaken to receive, on behalf of the editors, any documents that may be intrusted to them, all of which will be catalogued and carefully preserved, and returned within such limits of time as may be prescribed.

A CONFERENCE took place on Saturday, in promotion of a project to which we have already alluded as the "Trades Guild of Learning," for extending the advantages of university education to the working and middle classes of this country. It is proposed that local organisations shall be formed in various towns, and put into communication with a central guild, for the purpose of defraying the cost of the attendance of duly authorised lecturers sent from the Universities of Oxford and Cambridge, to conduct classes and deliver lectures on subjects, such, for example, as Political Economy, English Literature, Force and Motion, Astronomy, Physical Geography, &c. Technical education is to form a leading department of the scheme, and it appears that Nottingham, Derby, and Leicester have already made arrangements and fixed dates for receiving the lectures, and that the authorities of both Universities, but that of Cambridge especially, have given cordial encouragement to the idea. Saturday's conference was very fairly attended by representative working men in the capacity of delegates from societies more or less numerous and powerful, and the whole day from eleven in the morning until seven in the evening was occupied in the discussion of the project. Mr. Samuel Morley, M.P., presided for the first few hours, and was succeeded in the chair by Mr. Mundella, M.P. With them were the Rev. H. Solly, Mr. James Stuart, M.A., Hon. Sec. to the Syndicate, who is actively engaged in furthering the scheme in connection with the Universities, Mr. Webster, Q.C., and other gentlemen, and a few ladies. It was agreed that women should not be excluded from the advantages of the guild.

ON June 7 a meeting was held of the Druitt Testimonial Committee, at which it was reported that a handsome silver cup, along with 1215*l*., was to be presented to Dr. Druitt, who is still in India.

THE subscribers to the Children's Hospital, Bristol, have resolved to admit female practitioners to the medical staff of the hospital.

THE following, in alphabetical order, have passed first-class in Natural Science at St. John's College, Cambridge:—Clough, Jukes-Browne, Koch, Marshall, Sollas. Of the above, Marshall has been elected to a Foundation Scholarship, Clough, Jukes-Browne, Koch, Sollas (scholar 1872) have been awarded exhibitions.

IN the last issued Part of the *Birds of Europe*, which has just appeared, the name of Mr. Sharpe is no longer associated with that of Mr. Dresser as co-editor. The former of these two gentlemen has been compelled, on account of his many duties at the British Museum, to retire from his connection with the work which he was so instrumental in organising, and Mr. Dresser is now sole editor. The Viscount Walden, F.R.S., President of the Zoological Society, has relieved him of part of his considerable task, by undertaking to write most of the synonyms of the future parts, which will be sufficient guarantee for its accuracy and exhaustiveness.

THE concluding Part of Dr. W. L. Buller's *Birds of New Zealand* has just been issued. The genus *Apteryx*, the last discussed, and most interesting in the avifauna of these islands, is divided into four species at least, of which *A. haasti* closely resembles *A. oweni*, except in size, being considerably larger. The author also considers that the evidence, as far as it goes, is in favour of *A. haasti* differing from *A. maxima* of M. Jules Verreaux, which he thinks represents another species as large as a full-grown turkey. The Introduction contains several interesting supplementary notes; further facts are given in favour of the Quail Hawk (*Hieracidea novæ-Zealandiæ*) being distinct from the Sparrow Hawk (*H. brunnea*); the validity of *Platycercus alpinus*, as a species, is established; the Huia bird (*Heteralocha acutirostris*) is placed among the Starlings, close to *Creadion* instead of with the *Upupidæ*, and *Tribonyx mortieri* is included in the New Zealand fauna. There are seven excellent plates, and a supplementary series is promised.

THE recent changes which took place in French policy have deprived science of an active and able leader in M. Jules Simon, who was sparing no trouble to promote new inquiries and restore French science to its pristine activity. His immediate successor has had no time to make any show of his intentions. M. Batbie seems to feel inclined to accept the inheritance of M. Jules Simon, as far as it relates to the *Facultés* (the equivalent of the several English Universities). It is supposed on good grounds that all the schemes of M. Jules Simon for building a new Faculté of Sciences on the back part of the Luxembourg will not be interfered with by the sudden presidential change. It remains to be ascertained what will be the working of the new system on the courses of lectures delivered by unofficial men of science.

M. LEVERRIER has entered on his new office of Director of the French National Observatory. The Observatory Board has decided on his formal proposition that they shall co-operate with the Bureau des Longitudes for taking a new measure of the French arc from Dunkerque to Oran *viâ* Spain. Commander Perrier will be the chief geodesist for that most important survey.

M. WOLF has taken a series of magnificent photographs with Leon Foucault's siderostat during the last partial eclipse. He was then testing the photographic process which he intends using in Japan on the next Transit of Venus. The Japan Embassy was present at the operations and exhibited a great deal of truly scientific curiosity.

M. THIERS is now busy studying geology for the purpose of writing an essay on the destiny of mankind. He will take an anti-Darwinian view of the question. M. Daubree is his teacher for geology. He was taught in astronomy ten years ago by M. Leverrier, and in Natural Philosophy by M. Mascart, lecturer at the Collége de France.

M. BARTHELEMY SAINT-HILAIRE has already resumed his work of translating Aristotle and commenting upon it. The volumes now in hand relate to scientific subjects.

MESSRS. MACMILLAN & Co. will shortly publish the "Elements of Embryology," by Michael Foster, F.R.S., Prelector in Physiology at Trinity Coll. Cambridge, and F. M. Balfour, Scholar of Trinity College, Cambridge.

THE French Academy has named a commission to prepare a list of candidates for the place of Foreign Associate, vacant by the death of Baron Liebig. The commission is composed of MM. de Quatrefages, Liouville, Morin, Becquerel, Dumas, Chevreul, and Milne-Edwards.

TURIN possesses an Industrial Museum, which, though it has been established only a few years, is, according to *L'Institut*, one of the most complete in Europe, second only to the Conservatoire des Arts et Métiers de Paris. The value of this establishment has just been increased by the publication of a monthly periodical entitled *Annals of the Italian Industrial Museum*. The Director of the Museum is M. Codazza, and the Conservator, Mr. W. T. Jervis.

M. PAUL BROCA contributes to the *Revue Scientifique* an account of some researches he made about twelve years ago for the purpose of ascertaining the influence of education on the development of the brain. He took as his subject 20 attendants and 18 pupils of the hospital of Bicêtre, the average age of the former being 39½ years, and the average height 1·643 metre; the average age of the latter 26½ years, and the average height 1·689 metre. Notwithstanding the great advantage of the former in the matter of age—for it has been ascertained that the mean weight of the brain increases up to 40 years—the measurements made by M. Broca were very considerably in favour of the pupils, who had undergone a long training before being admitted to the hospital, and some of whom have since had a distinguished career. We can only here give the differences between the various measurements of the two groups of heads, the + denoting the excess (in millimetres) in favour of the hospital pupils. Antero-posterior diameter—Maximum + 4·89, inial + 5·87; transverse diameter + 2·91; cephalic cephalometric index − ·55; inio-frontal curve—total + 9·90, anterior part + 9·25, posterior part + 0·65: horizontal curve—total + 16·06, anterior part + 10·90, posterior part + 5·16; transverse curve—bi-auricular + 13·90, supra-auricular 11·70. M. Broca thinks that the results of the measurements prove, in the first place, that mental culture and intellectual work increase the volume of the brain, and in the second place that the increase takes place principally in the frontal lobes, which are the seat of the highest faculties of intelligence. Very important conclusions in favour of the spread of the higher education may be drawn from these statistics.

WE are glad to see, from a pamphlet by Mr. Ellery (just elected F.R.S.), "Notes on the Climate of Victoria," that a beginning has been made to put into shape the multitude of statistics which have already been accumulated as to the climate of that country. With regard to the rainfall, we quote the following paragraph:—By selecting Melbourne as the locality in which the most extended series of observations have been obtained, we remark that in the years 1848, 1849, and in 1863, the rainfall was far above the average; in 1864, 1865, 1866, and 1870 it fell below the average, especially 1865, when it only reached 15·9 inches. In 1848 and 1849 extensive and destructive floods occurred, and again in 1863; in 1865 and 1866 the country suffered from a severe drought; and the year 1851, following the heavy rains of 1849, was also a dry one, although the amount of rainfall, if ever observed, cannot yet be ascertained. An opinion has often been expressed that there is a periodicity in the excessive rainfalls and droughts in Australia generally; but although the above results may give some slight grounds for this supposition, a far greater number of years' observations will be necessary from which to deduce any law of this kind.

THE United States Signal Corps has recently extended its series of observations in the form of a daily record of the surface and bottom temperature of the rivers and harbours upon which the several stations are situated. This, while of much interest in a meteorological point of view, is also of practical importance in connection with the subject of introducing useful food fishes into the rivers and lakes of the United States, as lately provided for by Congressional enactment. It is well known that the possibility of introducing salmon into any given stream will depend upon the relationship of its temperature during the summer and autumn to the particular species; some kinds, as the true salmon of the North Atlantic (*Salmo salar*), requiring a summer minimum of at least sixty to sixty-five degrees, while others will bear a higher temperature.

AN institution has been founded in Vienna by M. Anton M. Pallsc, which he calls a Rudolfinum, or Students' Home—a college of technical science for students of any nationality. It is now announced that this gentleman has arranged with the officers of the Rudolfinum to furnish free lodgings in that building to three hundred professors and teachers, of all nations and countries, who intend visiting the exhibition of 1873. The offer is made for the months of July, August, and September, and applies alike to the professors of royal academies and the teachers of any kind of public schools. Early application is to be made, giving in each instance the name, address, and teaching position of the applicant, locality of school or institution in which he is engaged, with the date and length of time of his desired occupancy of these free lodgings. The application is to be addressed to the administration of the Rudolfinum, 4, Moierhofgasse, Vienna.

THE principal paper in the last number (Vol. ii. No. 4) of the

"Proceedings of the Bath Natural History Society," is a long address by the president, the Rev. Leonard Blomefield, F.L.S., F.G.S., on "Local Biology," containing many valuable hints as to the objects which members of such societies ought to have in view, illustrated by many interesting facts and recent observations in natural history. He shows how valuable the field work of local scientific societies might be made when intelligently and judiciously conducted, not only in collecting facts as to local biology, but in helping to solve many of the most important problems which are at present occupying the attention of biologists. The main qualification for efficient work of this kind is an intelligent and sharp look-out. Mr. Blomefield concludes his paper by some remarks on the faunas of Bath and Somerset. We are glad to see the address has been printed separately, and we would recommend it to the attention of all local scientific societies. The two other scientific papers in this number are on "Devonian Fossils from the Sandstones on the N.E. of the Quantocks," by the Rev. H. H. Winwood, F.G.S., and "The Geographical Position of the Carboniferous Formation in Somersetshire, with Notes on possible Coal Areas in adjoining Districts in the South of England," by J. McMurtrie, F.G.S., the latter illustrated by a well-constructed map.

WE have received a wonderfully cheap pennyworth in the shape of a "Descriptive Guide to the Fossil Collection" of the Museum of the Leeds Philosophical and Literary Society. The pamphlet is interestingly written and well arranged, and contains a long and valuable list of useful books of reference on Palæontology.

THE Third Annual Report of the Devon and Exeter Albert Memorial Museum, Schools of Science and Art, and Free Library, is altogether a very satisfactory one. Great facilities are offered for scientific study and laboratory practice, and these appear to be largely taken advantage of. The number of individual students during the current session is 89, and the subjects at present taught in the school are Mathematics, Theoretical Mechanics, Physical Geography, Geology, Acoustics, Light and Heat, Vegetable Anatomy and Physiology, Systematic and Economic Botany, Magnetism and Electricity, and Inorganic Chemistry with laboratory practice. According to the library statistics, a very large increase during the past year has taken place in the number of scientific books sought for, both in the consulting and lending libraries.

THE following is the ephemeris of Tempel's Comet for the days named as, calculated by Mr. Hind for Greenwich midnight:—

		True R.A.	True N.P.D.	Log Δ
1873		h. m. s.	° ′ ″	
June	20	16 14 50·1	111 18 19	9·91982
	22	14 23·7	111 38 44	9·92589
	24	14 4·9	111 58 53	9·93106
	26	13 53·9	112 18 45	9·93711
	28	13 51·1	112 38 19	9·94342
	30	13 56·7	112 57 34	9·94996
July	2	14 10·8	113 16 31	9·95771
	4	14 33·6	113 35 10	9·96365
	6	16 15 5·3	113 53 30	9·97077

THE additions to the Zoological Society's Gardens during the past week include a black Iguana (*Metopoceros cornutus*) from San Domingo, presented by Mr. John Dutton; two golden Tench (*Tinca vulgaris*), presented by Lord Herbrand Russell; two black Kites (*Milvus migrans*), presented by Mr. H. F. Blissett; two starred Tortoises (*Testudo stellata*) from India, presented by Capt. C. S. Sturt; a smooth-headed Capuchin (*Cebus monachus*) from Brazil, presented by Mr. J. A. Horsford; a Rhesus Monkey (*Macacus erythraeus*) from India, presented by Mr. G. Cork; an Entellus Monkey (*Semnopithecus entellus*) from India; four Sturgeon (*Accipenser sturio*); two American Rice-birds (*Dolichonyx oryzivora*); five horned Lizards (*Phrynosoma cornutum*) from Texas, purchased; a Lion (*Felis leo*) from Africa; a Collared Mangabey (*Cercocebus collaris*), a Diana Monkey (*Cercopithecus diana*), and a Moustache Monkey (*C. cephus*) from W. Africa, on approval.

SCIENTIFIC SERIALS

THE *Journal of Botany* for May commences with a critical investigation, illustrated by a plate, by the editor, of the very common but badly understood Dock, *Rumex obtusifolius*, which is followed by two papers on the distribution of plants, Additions to the British lichen flora, by Rev. M. Crombie, and Additions to the flora of Berkshire, by James Britten. In this number is also the very useful annual list of the new species of phanerogamous plants described in periodicals published in Great Britain during the year 1872. The plate which now accompanies every number is a great addition to the value of this magazine.

In the June number the illustrated article is by Mr. W. P. Hiern, on *Physotrichia*, a new genus of Umbelliferous plants from Angola, from the Welwitschian collection. Mr. F. Townsend contributes a paper on a much controverted subject, some points relating to the morphology of *Carex* and other Monocotyledons. The short notes and queries are, as usual, not the least interesting portion of these two numbers.

Poggendorff's Annalen der Physik und Chemie, Supplement vol. vi., part. I. This number contains the first instalment of a series of researches on the volume constitution of solid substances; a lengthy paper in three parts, the first being introductory and theoretical, the second describing methods, and the third detailing results in the case of chlorides, bromides, and iodides.—Prof. Schwedoff of Odessa follows with an interesting paper, in which he establishes a correspondence between the propagation of electrical currents in thin conducting insulated plates and that of light rays in transparent media. A "ray of electricity" is represented by the line drawn from a pole to any given point of the body, and means simply the direction in which electrical "masses" (in the plate) are attracted to the pole or repelled from it. He shows that the intensity of such rays is inversionally proportional to distance from the pole; that they are reflected (it may be oftener than once), from the edges, the angles of reflection and incidence being equal; that they do not lose intensity by reflection, nor suffer change of sign. His theory and mode of experiment are illustrated by figures.—An article by Dr. Heinrich Schneebeli on bar-magnetism, contains a full and thorough investigation of magnetic moment in permanent bar-magnets, and more especially of the position of the magnetic pole; this is determined by two different methods which do not suppose a knowledge of the law of distribution of the magnetic fluid, and the results (closely agreeing), are applied in correction of the tangent galvanometer.—Carl Pape contributes a determination of the optical constants of blue vitriol, and Alexander Muller the first part of studies on chloride of iron solutions without change of aggregate state.—Among the extracted matter may be noted an article by Kohlrausch on the reduction of the Siemens unit of galvanic resistance to absolute measure, and one by Edlund on the nature of electricity, which has already appeared in English form.

THE *Monthly Microscopical Journal* commences with an article by Dr. R. L. Maddox on an Entozoon with ova, found encysted in the muscles of a sheep, which he calls *Cysticercus oviparicus*. Then comes a very valuable paper on the development of the face in the sturgeon, by Mr. Parker, F.R.S., which, if followed by the description of a few more type-forms, will render the development of that complicated portion of the body, the head, one of the most easily understood sections of the vertebrate body. Mr. Joseph Needham gives a concise *résumé* of the methods employed for cutting sections of animal tissues for microscopical examination, in which he strongly advocates the method of freezing as "the simplest and most elegant mode" of obtaining sections of yielding tissues. Assistant-Surgeon Woodward describes how that a $\frac{1}{10}$th objective, sent to him by Mr. Tolles to test, gave a balsam angle of less than 80°, whilst a second, a $\frac{1}{8}$th of peculiar construction, having four combinations instead of three, gave the high angle of more than 100° when fully closed, and so exceeding the extreme limit assigned as attainable by Mr. Wenham. Mr. H. Davis gives further facts in support of the originality of his theory respecting the survival of Rotifera after desiccation.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 8.—"Researches in Spectrum Analysis in connection with the Spectrum of the Sun."—No. II. By J. Norman Lockyer., F.R.S.

The observations in this paper are a continuation of those referred to in the previous communication bearing the same title. They deal (1) with the spectrum of chemical compounds, and (2) with the spectra of mechanical mixtures.

I. *Chemical Compounds.*

Several series of Salts were observed; these series may be divided into two:—1st, those in which the atomic weights varied in each series; 2nd, those in which the associated elements varied in each series. The following salts were mapped:—

$\mathrm{Pb\,F_2}$, $\mathrm{Pb\,Cl_2}$, $\mathrm{Pb\,Br_2}$, $\mathrm{Pb\,I_2}$; $\mathrm{Sr\,F_2}$, $\mathrm{Sr\,Cl_2}$, $\mathrm{Sr\,Br_2}$, $\mathrm{Sr\,I_2}$; $\mathrm{Ba\,F_2}$, $\mathrm{Ba\,Cl_2}$, $\mathrm{Ba\,Br_2}$, $\mathrm{Ba\,I_2}$; $\mathrm{Mg\,F_2}$, $\mathrm{Mg\,Cl_2}$, $\mathrm{Mg\,Br_2}$, $\mathrm{Mg\,I_2}$; $\mathrm{Na\,F}$, $\mathrm{Na\,Cl}$, $\mathrm{Na\,Br}$, $\mathrm{Na\,I}$.

The conditions of the experiments are described, the same aluminium cups, described in the first paper, were used, and the poles were arranged in such a manner that they could at will be surrounded with any gas or vapour. Hydrogen was used in some of these experiments; it was purified in the usual manner by drying, and freeing from traces of sulphuretted hydrogen, it was then passed over clean cut pieces of sodium, and admitted to the poles. An induction-spark from 5 one-pint Grove cells was used, the *circuit being without the Leyden jar.*

The lead compounds behaved (in air) as follows:—

The fluoride gave the eleven longest lines of the metal, but four were very faint.

The chloride gave nine lines; one of these is very short.

The bromide gave six lines, but one is a mere dot on the pole.

The iodide gave four lines distinctly and two as dots, one of which is scarcely visible.

It is pointed out that the decrease in length and number of lines follows the increase in the atomic weight of the non-metallic element, the lines dying out in the order of their length.

Barium was next experimented on, the same series of salts being used. A marked departure from the results obtained in the case of the lead compounds was observed especially in the case of the fluoride, its spectrum being much the simplest; in fact it consisted of only four lines. Strontium behaved like barium, and so did magnesium fluoride. This anomalous behaviour was found to be most probably due to the exceedingly refractory nature of these fluorides, all of them being quite infusible, and non-volatile in any spark that was used.

Sodic fluoride, sodic chloride, sodic bromide, and sodic iodide exhibited a behaviour exactly the reverse of that of lead, *i.e.* the iodide showed most of the metallic spectrum.

The difference between flame-spectra and those produced by a weak electric discharge are then discussed. Beads of the chlorides, &c., were heated in a Bunsen-gas flame; $\mathrm{BaI_2}$ gave a "structure" spectrum (since proved to be due to the oxide) and the line 5534·5, by very far the longest metallic line of barium; the bead fused. The bromide behaved like the iodide, and so did the chloride, except that its spectrum was more brilliant. Baric fluoride gave scarcely a trace of a spectrum, the oxide structure being scarcely visible, and 5534·5 very faint indeed. The strontium salts follow those of barium, 4607·5, the longest strontium line appearing in conjunction with an oxide spectrum. The strontic fluoride, however, refused to give any spectrum whatever. These results are compared with those obtained with the weak spark, and it is shown that the difference is one of degree; *e.g.* baric bromide gives 25 lines in the spark; these are the longest lines. In the flame it gives but one line; but this is the longest of all the barium lines, and indeed very far exceeds all the others in length. When the flame-spectra are compared with those produced by the low tension spark, the spectra of the metals in the combination are in the former case invariably more simple than in the latter, *so that only the very longest line or lines are left.*

Some experiments made by Mr. R. J. Friswell to determine the cause of the similarity of the spectra of the various salts of the same metal observed in air are then given, the conclusion being that the spectrum observed is really that of the oxide.

Kirchhoff and Bunsen's, Mitscherlich's, and Clifton and Roscoe's prior conclusions on the points investigated are stated at length; and it is shown that the observations recorded, taken in conjunction with the determination of the long and short lines of metallic vapours, are in favour of the views advanced by Mitscherlich, Clifton, and Roscoe. For while the spectra of the iodides, bromides, &c. of any element in air are the same as stated by Kirchhoff and Bunsen, the fact that this is *not* the spectra of the metal is established by the other *fact, that only the very longest lines of the metal are present, increased dissociation bringing in the other metallic lines in order of their length.*

The spectra have been mapped with the salt in hydrogen; here the spectra are different, as stated by Mitscherlich, and *the metallic lines are represented according to the volatility of the compound, only the very longest lines being visible in the case of the least volatile one.*

The following are the conclusions arrived at:—

1. A compound body has as definite a spectrum as a simple one; but while the spectrum of the latter consists of lines, the number and thickness of some of which increase with molecular approach, the spectrum of a compound consists in the main of channelled spaces and bands which increase in like manner. In short, the molecules of a simple body and of a compound one are affected in the same manner by their approach or recess, in so far as their spectra are concerned; *in other words, both spectra have their long and short lines*, the lines in the spectrum of the element being represented by bands or channelled lines in the spectrum of the compound; and in each case the greatest simplicity of the spectrum depends upon the greatest separation of molecules, and the greatest complexity (a continuous spectrum) upon their nearest approach.

2. The heat required to act upon a compound, so as to render its spectrum visible, dissociates the compound according to its volatility; the number of true metallic lines which thus appear is a measure of the dissociation, and doubtless as the metal lines increase in number the compound bands thin out.

Mitscherlich's observations, that the metalloids show the same structural spectra as the compound bodies is then referred to, and the question is asked whether the molecules of a metalloid do not in structure lie between those of elements on the one hand and compounds on the other.

These considerations are applied to solar and stellar spectra; the general appearance of the solar spectrum shows that in all probability there are no compounds in the sun.

Secchi's maps of a large number of stellar spectra are referred to as now indicating beyond all doubt the existence of compound vapours in the atmosphere of some stars; and it is suggested that the phenomena of variable stars may be due to a delicate state of equilibrium in the temperature of a star which now produces the great absorption of the compound and now that of the elemental molecules.

The second part of the paper deals with the mechanical mixtures. Maps of the spectra of alloys of the following percentages are given:—

Sn and Cd percentages of Cd 10·0, 5·0, 1·0, 0·15.
Pb and Zn ,, ,, Zn 10·0, 5·0, 1·0, 0·1.
Pb and Mg.................. Mg 10·0, 1·0, 0·1, 0·01.

It is pointed out that the lines die out in the order of their length as the percentage becomes less, the shortest lines disappearing first; and that although we have here the germs of a quantitative spectrum analysis, the method is a rough one only, but that further researches on a method which promises much greater accuracy are in progress.

The bearing of these results on our knowledge of the reversing layer of the sun's atmosphere is then discussed.

Mathematical Society, June 12.—Dr. Hirst, F.R.S., president, in the chair.—The following papers were read:—"Some general theorems relating to Vibrations," Hon. J. W. Strutt; "Invariant conditions of multiple concurrence of three conics," Mr. J. J. Walker; "On a new form of Biquaternion, being the ratio of two systems of forces," Prof. Clifford; "A further note on geodesic lines," Prof. Cayley.—A paper by Prof. Wolstenholme, "The locus of the point of concourse of tangents to an epicycloid, inclined to each other at a constant angle," was, in the author's absence, taken as read.—A conversation ensued on the subject of Prof. Clifford's paper, in which the president, Prof. Cayley, and Mr. S. Roberts took part.

Geological Society, May 28.—Prof. Ramsay, F.R.S., vice-president, in the chair. The following communications were read:—"The Glaciation of the northern part of the Lake-district," by J. Clifton Ward. The author stated the leading questions to be settled by his investigation of the northern part of the Lake-district as follows:—The fact of the glaciation of the district being granted, and of this he adduced abundant evidence, the questions that arose were whether the glaciating agent worked from north to south, whether it came from within or from without the district, and finally, whether the agent was floating ice, a system of local glaciers, or an unbroken ice-cap. As the result of his investigation he maintained that there is no evidence that a great ice-cap from the north ever swept over this district. The ice-scratches trending along the principal valleys, but sometimes crossing watersheds, indicate a great confluent glacier-sheet, at one time almost covering a great part of the district, the movement of which was determined by the principal water-shed of the Lake-district. In the part of the Lake-district under consideration the ice, during its increase, carried forward, from south to north, a great quantity of rocky material. There are no signs in the district of the occurrence of mild periods during the epoch of primary glaciation, but the author thought that the climate had probably become moderate before the great submergence of the land commenced. The author noticed the effect of the submergence upon the results of previous glacial action, and maintained that when the land had sunk 800 or 900 ft. there was a recurrence of cold, and boulders were transported by floating ice. Until the submergence reached 1,500 ft. there was no direct communication between the northern and southern halves of the Lake-district except by the straits of Dunmail Raise. From the directions which would be taken by the currents in the sea at this period, it would appear that boulders may then have been transported by floating ice in some of the same directions as they had previously been carried by glacier-ice. The extreme of submergence appeared to have been about 2,000 ft. The author further maintained that on the re-elevation of the district there was a second land-glaciation, affecting the higher valleys and clearing them of marine drift.—"Alluvial and Lacustrine Deposits and Alluvial Records of the Upper Indus Basin," by Frederic Drew. The author said that he felt the necessity for a careful classification of the phenomena of alluvial deposits, for the want of recognition of the different kinds was likely to lead to incorrect deductions; the classification he proposed was the following:—I. *Loosened material*, which consisted of disjointed rocks or loose angular stones, sometimes mixed up with mud, which had been separated and disintegrated, but since that had remained *unmoved*. II. *Taluses*, the substance of which had fallen by its own weight, and not been transported by streams. These were the great heaps of angular matter that were found at the foot of cliffs, with a slope generally of near 35°. A special form was the fan-talus, which occurred where the falling matter had either originated from, or collected to, one spot, from which again it spread, and made a partial cone of the same slope as the ordinary taluses. III. *Alluvial Fans.*—These were the fan-shaped extensions of alluvial or torrential matter that spread out from the mouths of gorges, where these debouched into a more open valley. They were in form cones of a low angle, commonly 5° or 10; they had accumulated by layer after layer on a cone-shaped surface, as shown by the radial sections exhibiting layers of a straight slope, and the chord sections showing curves, which were by the theory hyperbolas. Many complicated phenomena were produced by the denudation of these fans, and the production of secondary ones, some of which were illustrated by diagrams. IV. *Alluvium*, which was defined as a deposit which sloped down the direction of the valley of the stream which had made it, and did not appreciably slope or curve over in a direction at right angles to that. With regard to the country in question, there was evidence of a succession of three states:—1st, The cutting of the valleys. 2nd, The accumulation of alluvial matter. 3rd, The cutting down of the streams through that alluvial matter. Accumulation denotes an excess of supply of material from the rocks (by disintegration) over what can be carried away by the streams. Denudation, or the cutting down of the streams through their alluvium (the lowering of their beds), denotes a deficiency of supply of material from the rocks as compared with the transporting power of the streams. Hence the author inferred that the period of great accumulation of these alluvial deposits was one of great disintegration of rocks, one of intense frost; in other words, it was the Glacial period, and that the denudation occurred when the cold lessened, and there came to be a smaller supply of disintegrated material. The connection of various glacial phenomena with the alluvium, such as the one described above, was taken to corroborate the inference that the greater deposits were made during the Glacial epoch.

Geologists' Association, June 6.—Mr. Robert Etheridge, F.R.S., vice-president, in the chair.—"On Ammonite Zones in the Upper Chalk of Margate, Kent," by Mr. F. A. Bedwell. The author described, and showed by sections, the exact positions in the cliffs to the east and west of Margate, of fifteen large Ammonites, twelve of which lie between the Flagstaff and the Cliftonville Hotel, a space of about half a mile, and some of them exceed three feet in diameter. All these twelve are in a bed closely approximating to an exact parallel with a faint line of nodular flints which undulates over this part of the cliff and are at a constant distance of eight feet below that line. These facts indicate the following (1) The presence of an Ammonite zone, and of (2) a true sea floor. (3) The parallelism of this with the horizontal flints, and (4) that all the horizontal bands of flint must be assumed to have been aggregated before the chalk moved. Particulars were also given of three other beds of Ammonites, one to the west of Margate, another forty feet below that first mentioned, and a fourth at Pegwell Bay, at the top of the cliff near the landing-stage. The first and second were conjectured to be identical, and also the third and fourth. Specimens from the first and second beds were respectively identified by Mr. Etheridge as *A. leptophyllus* and *A. Lewesiensis*. Similar beds elsewhere were referred to, but details could only be given of one. This is to be seen at low water near the Black Rock at Brighton. A remarkable bed of continuous solid flint, three or four inches thick, occurs round and under the Isle of Thanet. Between the Foreland and Pegwell Bay it is in the upper part of the cliff, sinks below the shore at Pegwell Bay and Kingsgate, rises again to the west at the back of Margate Harbour but disappears immediately, appears again to the south, as pointed out by Mr. Whitaker in his Geology of the London Basin, at Cap Point near Walmer, and again at Shepherd's Well Station, 10 miles inland, where it is surmounted by the soft almost flintless chalk of Margate, and finally it was known throughout the island by the well diggers. This positive testimony of coincident and uniform flint aggregation over so large an area appeared to be an important fact in its bearing on the origin of flint. Mr. Bedwell stated that he had found the ammonites entirely by trusting to the zone of life theory insisted on by Mr. Caleb Evans in his paper on the Chalk (Geol. Assoc. 1870), and had failed to find them until he had selected the faint line of flints as a datum line and worked from that. He advised all young students of the chalk to examine a cliff in true horizons and not in a mere indiscriminate effort to make a large bag of specimens, to record carefully the exact chronological order of each fossil extracted by referring it to a datum line as suggested by Mr. Caleb Evans, to keep in mind the time which may have separated the life history of two fossils though only distant a few feet from each other, and to try to correlate two sections of chalk rather by the succession of zones of life in each cliff than by a mere comparison of indiscriminately collected fossils. The author in conclusion urged the importance of allowing Nature to teach her own independent lessons at the cliff side, of supplementing Nature by books rather than books by Nature, and pointed out how easy it was for those with little knowledge of details to be of service to science by simple observation and following to its end one single thread and one only, and then laying the results before scientific men, leaving them to estimate the value of the information.

Royal Horticultural Society, June 4.—Scientific Committee.—A. Smee, F.R.S., in the chair.—A fruit of *Anona reticulata* was sent produced in the gardens of Sir Walter Trevelyan at Wallington.—A letter was read from Prof. Westwood, stating that some grubs which had been submitted to him as having completely destroyed some bulbs, proved to belong to *Merodon clavipes*, a very rare insect in England, and in this case probably introduced.—A Pelargonium of the variety Cleopatra was exhibited from the Chiswick Garden. It had produced trusses of flowers of its proper pink as well as others of its ancestral scarlet.—Dr. Gilbert made some remarks on the proposed use of chalk mixed with coal in furnaces for horticultural purposes. He said it was quite certain the chalk could not supply any heat; on the contrary, its conversion into lime involved a considerable loss of heat in order to effect the change. What the chalk did was to absorb the heat and radiate it out again, and pieces of broken fire brick would probably answer the purpose equally well. The mixture of these substances simply, so to speak, diluted the coal.—A fine specimen of fasciated asparagus was shown from Mr. Macmullan. It has been produced two years running apparently from the same plant.

General Meeting.—Viscount Bury, M.P., president, in the chair.—The Rev. M. J. Berkeley stated that he had recently seen in Denbighshire nectarine trees, the flowers of which usually produced five carpels instead of one. He commented on the effects of the late frost on the potatoes at Chiswick. Some were very much injured, while others had escaped altogether, and in some instances of two stems to one root, one had been killed back and the other not touched.

Philadelphia

Academy of Natural Sciences, March 4.—Mr. Vaux, vice-president, in the chair.—Mr. Thomas Meehan exhibited a flower of *Bletia Tankervillia* (*Phaius grandiflora* of some authors), in which the dorsal sepal (or, as some authors contend, petal), had united with the column, and had been much retarded in its development accordingly. He said that he had several dozen of flowers produced in this way this winter, all, however, confined to separate spikes from those which bore the perfect flowers. It was usual to pass over these appearances as "monstrosities," but in truth the whole Orchid structure was little less than a monstrosity. He did not think as much had been made out of the changes of structure in orchids in the study of evolution, as might be, in consequence of the impression that these abnormal forms, as they were termed, were monstrosities, or the results of cultivation. There had been already on record accounts of changes in wild orchids more remarkable than many much dwelt on by many modern writers on development. He further remarked that, in examining closely the flowers of *Bletia Tankervillia* early in the morning, he found on the outside, at the base of the three exterior petals, a liquid exudation from a small gland. It was highly probable that these glands were rudimentary spurs, and that, if the course of nutrition which sustained the cohering power of an orchid could in any way be diverted before the final direction of form, each of these outer petals might take on some of the labellate character with its attendant spur, which gave such a peculiar appearance to so many orchidaceous plants.

March 18.—The president, Dr. Ruschenberger, in the chair. —"On the Occurrence of an Extinct Hog in America."—Prof. Leidy exhibited the fragment of a lower jaw of a pig which Prof. Hayden had picked up, together with many remains of extinct mammals, in the pliocene sands of the Niobrara River, Nebraska. The specimen he viewed as of recent character, and not as a true indigenous fossil. Prof. Leidy remarked that he had never seen any remains of the hog which he could confidently view as true American fossils.—Prof. Cope stated that Dr. Hayden handed to him for determination some bones on a fragment of the Green River shale of the Eocene of Wyoming. They indicated a species of Anourous Batrachian, but as the individuals were not fully developed, he was not prepared to identify the genus. They constituted the first indication of this order in time; those previously known from Europe and India being all of Miocene age.

Paris

Academy of Sciences, June 9.—M. de Quatrefages, president, in the chair.—M. Dupuy de Lome presented to the Academy, in the name of the Minister of Marine, the first number of the "Memorial of Marine Artillery" and its appendix, "The Artillery Remembrancer." These are published for the use of French naval officers, and contain an immense amount of information on the armament of foreign ships of war. Great space is devoted to English naval matters, and the Memorial is well worthy of the attention of our own naval authorities. —The following papers were read:—Researches on new propyl derivatives, No. 2, by M. A. Cahours. The glucinum, silicon, and boron compounds of propyl were described.—On normal and abnormal speech, by M. Bouillaud.—On the intervention of atmospheric nitrogen in the phenomena of vegetation, by M. P. P. Dehérain. The author described some experiments which showed that, in the presence of ammonia, glucose absorbs nitrogen from the air.—On the multiple causes which provoke the fall of lightning, by M. W. de Fonvielle.—On the theory of the spots and the dark nucleus of the sun, by M. E. Vicaire. The author replied to M. Faye's recent answer to him; he thinks that Respighi's observations quoted by M. Faye tend to support his views rather than those of that astronomer, *i.e.* that the absence of the chromosphere over the spots is due to a cessation of the emission of the gases of which it is composed, and not to their being swallowed up by a cyclone.—Researches in spectrum analysis in relation to the spectrum of the sun, by Mr. J. N. Lockyer. This was a letter to M. Dumas giving an account of the author's late paper read before the Royal Society.—An answer to M. Raynaud's late note on the resistance-maxima of magnetic coils, by M. Th. du Moncel.—On the relation between electric and capillary phenomena, by M. G. Lippmann.—On the boiling points and molecular volumes of the chlorinated isomers of the ethylic series, by M. G. Hinrichs.—On ethylacetylene formed by synthesis, and on its identity with crotonylene, by M. L. Prunier. The author has synthesised this body by passing equal volumes of ethylene and acetylene through a porcelain tube heated to dull redness.—On the synthesis of phenyl-allyl, by M. Chojnacki. The author obtained this body by acting on a mixture of equal weights of benzine and iodide, or bromide of allyl, with $\frac{1}{8}$th of its weight of powdered zinc.—On the combinations of titanic chloride with the ethers, by M. Demarçay.—On phenyl-cyanine, by Mr. T. L. Phipson.—Note on M. Mène's paper on the preparation of ammonic sulphate from nitrogenous waste, by M. L'Hote.—On the estimation of phosphoric acid in manures, coprolites, and fossil phosphates, by M. Ch. Mène.—Mineralogical note on the dibasic plumbic sulphate of l'Ariége, by M. E. Jannettaz.—On the affinities of *Etheostomata* (Agassiz), by M. L. Vaillant.—Magnetic observations, by M. Diamilla-Muller.—Spectroscopic researches on the fumerolles of the eruption of Vesuvius of April 1872, and on the actual state of that volcano, by M. L. Palmieri. This was a very short extract from a letter, the only points being that thallium and boric acid are found in the sublimates from these vents, and that since the eruption the mountain has exhibited a state of abnormal quietude.

DIARY

THURSDAY, June 19.

Royal Society, at 8.30.—On the Fossil Mammals of Australia, Part IX. Family Macropodidae: Prof. Owen, C.B.—On the Nature and Physiological Action of the Poison of Naja Tripudicans, and other Indian Venomous Snakes: Dr. Fayrer and Dr. Brunton.—Researches in Circular Solar Spectra Applied to Test Residuary Aberration in Microscopes and Telescopes: Dr. Royston-Pigott.—On the Structure and Development of the Skull in the Pig (*sus scrofa*): W. K. Parker.—Results of the Comparisons of the Standards of Length of England, Austria, Spain, United States, Cape of Good Hope, &c.: Lieut.-Col. Clarke.—On Comparative Vegetable Chromatology: H. C. Sorby.

Society of Antiquaries, at 8.30.—On Further Excavations at Silchester: Rev. J. G. Joyce.

Linnean Society, at 8.

Chemical Society, at 8.—On the Influence of Pressure upon Fermentations Part II.: Horace Brown.—Researches on the Action of the Copper-Zinc Couple on Organic Bodies, III., and on Normal and Iso-Propyl Iodides: Dr. J. H. Gladstone and A. Tribe.—On Cymenes from different sources optically considered: Dr. J. H. Gladstone.—On the Action of Bromine on Alizarine: W. H. Perkin.—On some Decompositions and Oxidation Products of Morphine and Codeine Derivatives: E. L. Mayer and Dr. C. R. A. Wright.—On the Decomposition of Tricalcic Phosphate by Water: R. Warington.—On a new Tellurium Mineral, with Notes on a Systematic Mineralogical Nomenclature: J. B. Hannay.—Communications from the Laboratory of the London Institution, No. XII.,—On New Derivatives of Cresol: Dr. H. E. Armstrong and C. L. Field.

Numismatic Society, at 7.—Anniversary.

FRIDAY, June 20.

Medical Microscopical Society, at 8.—The Pathological Relations of Diptheria and Croup: Jabez Hogg.

MONDAY, June 23.

Geographical Society, at 8.30.

WEDNESDAY, June 25.

Society of Arts, at 4.—Anniversary.

Geological Society, at 8.—On Six Lake-basins in Argyllshire: His Grace the Duke of Argyll, K.T., F.R.S., President.—Description of the Skull of a Dentigerous Bird (*Odontopteryx toliapicus*, Owen), from the London Clay of Sheppey: Prof. Richard Owen, F.R.S.—Contribution to the Anatomy of *Hypsilophodon Foxii*, Huxley: J. W. Hulke, F.R.S.—On the Glacial Phenomena of the Long Island, or Outer Hebrides: James Geikie.—On Fossil Corals from the Eocene Formation of the West Indies: Prof. P. Martin Duncan, F.R.S.—Note on the Lignite-deposit of Lal-Lal, Victoria, Australia: R. Etheridge, Jun.

THURSDAY, June 26.

Society of Antiquaries, at 8.30.

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THURSDAY, JUNE 26, 1873

THE ENDOWMENT OF RESEARCH

I.

THERE are not wanting signs that ere long the whole question of the present condition of research in this country, and of its amelioration, will undergo a complete discussion. Those who are best acquainted with this condition, and the position occupied by England at the present moment in the Science of the world, will be the first to acknowledge the importance of general attention being directed to the subject.

When the matter comes to be considered by minds free from the trammels alike of tradition and of prejudice, it will doubtless be found strange that such a fundamental question should have waited so long before it should have asserted itself; on the other hand, it is perfectly clear that many who are even now considering it have utterly failed to grasp it as it will have to be grasped.

This lack of clearness in the appreciation of the vast bearings of the question is quite pardonable, and is, doubtless, to a large extent, the natural consequence of the manner in which physical science has been added on to the older knowledge. It would seem, however, that a mere statement of a few fundamental positions should clear the view. These positions, most fortunately, are rapidly asserting themselves.

First, we have the generally acknowledged fact that a nation's progress depends upon its Science. Science, in fact, is the engine which must be as ever active in peace as the cannon's mouth is in war, and a nation may just as safely neglect one as the other.

This brings us to the second position. Does England as a nation pay as much heed to the one as the other? or as much as other nations? To ask this question is to answer it. England as a nation does next to nothing for this peace armament, and on all hands it is acknowledged that the nation's progress from this point of view is in great danger, because the decline of research in England, not only relatively, but absolutely, is so decided, that it is already a matter of history. We have long ago in these pages referred to Dr. Frankland's evidence on this point; he is the acknowledged head of chemical science in this country and should surely know; and other men who cultivate other sciences have expressed the same opinions with regard to them.

To what then is this decline to be attributed? The reply to this question brings us to the third point. There is absolutely no career for the student of Science, as such, in this country. True scientific research is absolutely unencouraged and unpaid. The original investigator is of course the man here intended, not the man who turns Science into a means of livelihood, however honourable, either as a teacher or a manufacturer.

There can be no doubt that to this state of things our present condition is to be ascribed, and this point is, according to us, the key of the whole position. A glance at the condition of things in France and Germany will strengthen our view. Why was Germany till lately the acknowledged leader in all matters connected with the advancement of knowledge? Because there were no such brilliant and highly paid careers open there as here to those who choose politics, the bench, the bar, or commerce, in preference to Science. And what is happening there at present? a decline visible not alone to the far-sighted, because Germany is getting rich as England has long been rich. Why is France now endowing research on a large scale, and even proposing that the most successful students in her magnificent Polytechnic School should be allowed to advance Science as State servants? Because in France there is a government instructed enough to acknowledge that a decline of investigation may bring evil to the State, and that it is the duty of the State to guard against this condition of things at all cost, this condition till lately, there as here, being that outside of the State service, and outside of the professoriate, no means of existence are provided for a student of Science; hence men of the most excellent promise are yearly lost to research, which undoubtedly also is the case with us.

What course then does it behove us to pursue in this country, in order that Science may take up its true position in our midst?

Here again opinion is rapidly forming itself. It is obvious to all who have thought about the matter, that it is absolutely indispensable that an employment, necessary for the public good, which is neglected to the State's detriment because in itself it does not bring in a livelihood, should be artificially supported, and artificially supported at the public expense. It would be quite justifiable, both from an economical and also a political point of view, to provide for the needs of knowledge out of the taxation of the country; because the taxpayer gets back his *quid pro quo* for the taxes he pays in the form of the amelioration of the conditions of living, as he gets it back in the form of security and good government.

It will probably be a considerable time before this truth is brought home to the public mind so completely as to render possible any large grant of national income for this purpose; but there are not wanting indications that statesmen of all parties are awakening to its reality, which in point of fact has long been conceded in principle. Still, such a source of support for Science to any very large extent must appear, even to the most sanguine, a thing of the future.

The area of knowledge will probably, in the future, increase beyond the means of any artificial support less than the national one; but perhaps it cannot be said that this state of things exists at present.

What, then, are we to do in the mean time? Have we no means which are at hand and immediately available, which may suffice to support the present claims of knowledge, without drawing too extensively upon the long-suffering or the intelligence of the taxpayer?

We have the means, if we will only employ them—nay more, some of them are now, for the most part, lying idle—of not only supplying all the needs of the physical and other sciences, but of supplying them magnificently. To mention no other sources of supply there is the Patent Fund, and the endowments of the colleges of the old Universities.

As to the Patent Fund, it is not too much to say that a large part has been derived from the application of the abstract truths of physical science to the requirements of ordinary life, and that therefore the needs of physical science would be properly provided for out of it.

As to the College Endowments, whichever way we look at them, either as private bequests, as they are at length ceasing to be regarded, or as public funds, the conclusion is the same: their proper destination is the support of learning and Science.

If we look upon them as private bequests, and interpret the wills of founders and benefactors on the usual *ci-près* principle, we should be right in devoting to investigation of facts at first hand the funds which were left by the far-seeing men of the time of the revival of letters for the support of book-learning, which at that time occupied the place of modern Science. That they so regarded the aim of these bequests is shown, amongst other things, very remarkably by the universal annexation to the enjoyment of them of the condition of residence within the Universities. When the whole, or the major part, of the materials of investigation was enshrined in libraries, to insist that a man should remain where libraries were was to insist that he should remain in his workshop.

If on the other hand we are to regard these endowments as public funds, as is now generally agreed, is it right that such public funds should be consumed either in educating those who are practically as well able to pay for their own education as those who now receive a similar one at, say London University, an institution which is not aided by the State; or in supplying a life-maintenance to a considerable body of able young men, in return for passing a good examination at the outset of life?

It is well known that the ordinary Fellow of a college does not dream for a moment that he has any duties towards knowledge or Science. He regards the public money which he enjoys as a portion in a freehold estate, to enable him to tide over the uncertain years which come at the commencement of the ordinary professional career, the brilliant rewards of which we have shown to be the great cause of the decline of Science in this country, because they enable the practical life to outbid in attractiveness the laborious but most necessary pursuit of truth.

CHAUVEAU'S ANATOMY OF DOMESTICATED ANIMALS

The Comparative Anatomy of the Domesticated Animals. By A. Chauveau. Translated and edited by G. Flemming, Vet.-Surg. R.E. (J. and A. Churchill.)

FOR a long time there has been a great want felt by veterinary surgeons of a first-class work on the anatomy of the horse and other domestic animals, to be to them as valuable and trustworthy a book of reference as Quain and Sharpey's Anatomy is to the student of human anatomy. This feeling has induced Mr. Flemming to undertake the very arduous and considerable task of translating from the French the generally esteemed "Traité d'Anatomie Comparée des animaux domestiques" of M. Chauveau. The high position held by the Veterinary School of Lyons, and the great scientific reputation of its Professor, are sufficient guarantee for the excellence and accuracy of the original work before us, so that it will be unnecessary to enter into a detailed criticism of it: it will therefore be our chief duty to consider the manner in which the translation has been performed.

There are, however, one or two points to which we should like to draw attention in the work itself. First respecting the nomenclature of the lobes of the liver in the horse, Prof. Chauveau, as do most of the authors on the same subject, incorrectly calls the Caudate lobe the Spigelian. This error was clearly pointed out by Prof. Flower in his Hunterian Lectures last year, when he conclusively proved that the free, ear-shaped lobe, which is situated to the right of the vena portæ in the horse, rhinoceros and tapir, is the caudate and not the spigelian lobe. This last is represented by a long attached transverse ridge of hepatic tissue, situated further to the left. Again, it is not clear why the protometra is said to be incorrectly termed the *uterus mascululinus*, for it is certainly not a gland in the ordinary sense of the word, and is as certainly the rudiment of the duct which developes into the uterus in the female. In the paragraph on the small horny plates, called "chesnuts," found on the lower third of the inner face of the forearm and at the upper extremity of the inner face of the metatarsal bone of the horse, the author remarks that "In solipeds, the chesnut is the representative of the thumb." That such is the case is, to say the least, extremely doubtful particularly in any member of the class Ungulata; and from the fact that in the rhinoceros and tapir the second digit is perfectly developed, these epidermic appendages would be most probably larger in them than in Horse, if they represented the pollex and hallux; however they are altogether absent. That these horny plates in the fore-limb are situated above the carpus, is likewise not in harmony with their representing the thumbs.

Respecting the translation, which considering the size of the volume, must have been a very serious undertaking, the reader will, in the majority of cases, learn as correctly and as easily as from the original French. A perusal of several portions of the work seems to indicate that the translation has been performed by more than a single hand, for in some portions it is not so good as in others, and different words are employed to express the same one in the original. If there is any fault to find, it is one which may be considered by some to be rather an advantage than not, namely, that the rendering is too literal. A verbatim translation is in some cases not capable of giving the full force of the author's meaning in scientific as well as in other subjects, each language having an idiomatic phraseology of its own. For instance, the middle of the diaphragm may be correctly termed in French "le centre phrénique," but it is more than perplexing to comprehend at first sight what is meant by "the phrenic centre." The cavities of the heart (*les poches*) are not called "pouches" by English anatomists, and the colon is sacculated (*bosselé*), not "bosselated;" this latter word is not to be found in some, perhaps not in any standard dictionaries. The stylo-glossus muscle does not "respond" (*il repond*) but corresponds "with the mylo-hyoid outwardly and the genio-glossus inwardly." The large colon of the horse is said to be fixed by adherence to the "cross of the cæcum;" we do not know what the cross of the cæcum is, but the angle or bend (*crosse*) can be easily understood; in other places this word is correctly translated. Several minor errors in which nouns are rendered as adjectives and sentences are incomplete, will be no doubt corrected in a second edition.

Mr. Flemming has made some modifications in the general plan of the work, which will decidedly render it more useful to English readers. The descriptions of the anatomy of the ruminants, as well as those of the cat, dog, and birds, are in small type, so that it is not at all difficult, by omitting all but the large type, to study the bones, muscles, and nerves of the horse, without having to sift these out from the much larger mass of information respecting the other animals, as has to be done in the French edition. He has also added many notes, which in most cases bear on practical points in veterinary art; and he has omitted, wisely we think, the paragraphs of the original, which have reference to the dromedary and rabbit. Several of the unnecessary illustrations of human dissections, which can be found in many other works on the subject, have been omitted, and they have been replaced to advantage by others which further illustrate that of the horse, and also the recent advances in our knowledge of the structure of the tissues of the animal body.

Students of human anatomy are too apt to think that anthropotomy is the only subject of the kind which has been worked out thoroughly and in detail, but a glance at the book before us will soon remove that impression; and we are convinced that no one who has made any progress in a medical education could more profitably employ an occasional spare hour, than by a perusal of parts of this translation by Mr. Flemming of M. Chauveau's most excellent treatise.

RECENT ARITHMETICS

Arithmetic in Theory and Practice. By J. Brook-Smith, M.A., LL.B. (Macmillan, 1872.)

A Treatise on Arithmetic. By J. Hamblin Smith, M.A. (University Press, Cambridge, 1872.)

Figures made easy. A First Arithmetic Book. By Lewis Hensley, M.A. (Clarendon Press Series, Oxford, 1872.)

Notes on Arithmetic and Algebra. By the Rev. S. E. Williams, M.A. (Cambridge: J. Hall and Son, 1872.)

MOST persons engaged in tuition have often this critical question proposed to them, "Whose arithmetic do you recommend?" and as almost every teacher of mathematics fancies he has something new or varied to say on the subjects he has long taught, many rush into print, and thus submit their claims to consideration to a wider circle than that they have hitherto addressed. "As many arithmetics as teachers of the science," is perhaps as true a doctrine as that which applies to men and their opinions, certainly the writing of treatises on the subject has not of late years got into disfavour with the body referred to, and a second edition of De Morgan's Arithmetical Books, would show a considerable increase in number of authors if brought down to the present date. Every year sends forth a heap of candidates for the public favour. On the whole perhaps arithmetic has been very fairly treated; most of the treatises that have come under our own eyes have possessed something to recommend them. We have grouped together for our present consideration some of the most recent works on the science. Without doubt the first book on our list is entitled to the place of honour; it is, we think, the best work that has appeared for some years, the only work claiming to be ranked on the same high platform with it, being the "Arithmetic Theoretical and Practical," by W. H. Girdlestone, M.A. (Rivingtons, 1870): the two have much in common. In this treatise the leading propositions are discussed and reasoned out in a lucid and accurate manner; the fundamental principles are clearly stated, and there is a valuable collection of examination papers for the student to try his powers upon. The writer is a disciple of De Morgan, to whom, as well as to other eminent writers on Arithmetic, he acknowledges his indebtedness. The book is quite up to approved modern standards, as it gives contracted methods of work, and treats of the metric system, and of the application of per-centages. It needs no further commendation, and after stating that it is a good *practical* work, we advise a student in want of a good treatise, to get this, and make it part and parcel of his mental furniture. The "get-up" of the book, its external dress, its inner garniture, is not merely neat but positively elegant, and possibly indicates the high interest the author takes in the subject upon which he has written so well.

Mr. Hamblin Smith's work calls for no special comment: the ability with which the author has written on other subjects will doubtless induce many to purchase the book. It is hard to write anything new on so hackneyed a theme, and there are few who have been able to raise the treatment of it above the ordinary fair orthodox level. We believe it to be a sound book, but it could have been dispensed with (especially with our first considered work in the field) except as it serves to fill up a niche in a connected series of text-books. The writer in this case also aims at teaching not so much *rules* as *principles*, and he rightly treats the so-called *rule of three* by the rational method now so generally adopted. The book may be recommended as a school-book, and this is probably the object the writer had in view. There is a copious collection of examination papers, which occupies nearly one-sixth of the whole work.

The third work on our list is concerned with much lower ground than the two former; it is written for mere infants, so to speak, in the science—it is an A B C: the receiving vessels are small and their capacity consequently for acquiring such new ideas as are presented to them at the outset of their inquiries also small; our author, with the ability only acquired by careful thought and experience, prepares right food, and not too much of that, for each lesson. In forty lessons the pupil is carried from "first notions of counting" to "division of fractions." With careful oral teaching we believe the book to be well adapted for the end aimed at. It is printed in the effective style of the "Clarendon Press" Series, and is further recommended by its cheapness.

The "Notes" presuppose a general knowledge of the subject, and give for the most part no explanation of the rules. The book is intended to act more as a "refresher" than as an "instructor," yet in the addition, multiplication, and division of recurring decimals, together with the history of the calendar, the author has gone into a little more detail. To these "Notes" have been subsequently added some useful "Notes on Algebra." For the object aimed at the book is very fairly adapted. Some few further notes which will readily occur to the majority of teachers can be easily furnished to pupils using the "Notes" for insertion, in addition to the printed ones.

We have not tested the accuracy of the solutions given in the works we have here examined.

OUR BOOK SHELF

Official Guide-book to the Brighton Aquarium. By W. Saville Kent, F.L.S., F.Z.S. (Brighton, 1873, price 6*d.*)

THE Brighton Aquarium is without doubt the largest and most extensive of the buildings which have been erected of late years for the exhibition of aquatic animals. It also possesses the advantages of being at the seaside, and at the same time conveniently placed for access to the multitude of sight-seers. Though a large sum of money was spent upon its construction, we have been informed that good dividends are paid to the shareholders, and it would seem that the institution shows every symptom of favourable progress. In our eyes the issue of the present guide-book is a very welcome proof that Science will not be entirely neglected in the endeavours to attain material prosperity. Mr. Saville Kent's guide-book is drawn up with a strictly scientific method, but at the same time a large amount of popular information is given in it, and it is well adapted for the purpose for which it is intended.

The higher vertebrata of the Brighton Aquarium are at present but few in number, consisting only of porpoises, representing the order *Cetacea*, and the common seal, exemplifying the marine section of the Carnivora, and it is not likely that the representatives of these orders will be much increased in number. But the class of fishes is, on the other hand, very well represented, the Brighton Institution containing the best living series of these animals that has ever yet been brought together, and one that, as our weekly record of its progress shows, is continually increasing both in number and in variety. Mr. Kent's guide-book furnishes the visitor with a short account of the principal facts that are known concerning the life-history of each of these fishes, and cannot fail to add greatly to the instruction to be derived from a visit to the Aquarium. After the fishes, which certainly form the leading feature in the Brighton establishment, and consequently the principal topic in the guide-book, Mr. Kent turns to the Invertebrate division of the animal kingdom, and gives a general sketch of the five groups into which it is now usually separated, and of their principal representatives in the Aquarium. This portion of the guide-book, we think, requires further development, and will doubtless receive it in a future edition. We also beg leave to suggest that a few illustrations in the way of woodcuts would be a valuable addition to the handbook, and would, moreover, be likely to assist very materially in extending its sale. The only illustration in the edition now before us is the ground-plan of the building, given as a frontispiece to the work, and showing the arrangements of the different tanks and rooms. Figures of some of the more remarkable inhabitants of the tanks would, in our opinion, render Mr. Kent's book more attractive to the general visitors, and more useful to the scientific student.

Chemistry for Schools. By C. Haughton Gill. With 100 illustrations. Second edition. (London: Edward Stanford, 6 and 7, Charing Cross, 1873.)

MR. GILL's little manual is intended either for private study or for class-teaching, and has special reference to the requirements of those who have to learn the small modicum of chemistry required for the matriculation examination of the University of London. He has indicated the chapters necessary for the latter by a †, an act which we cannot at all approve. Surely, if even so light an examination as the one in question has to be undertaken in what may be to some a distasteful study, it is better to know too much than too little, and Mr. Gill's little book is not such a very dreadful treatise that one need be afraid of reading it through. If the examinations are to mean nothing more than the "getting up" of a set of special chapters written for the purpose, they had better by far be abandoned at once. With this exception we have little fault to find. Great care has been taken in arranging and systematising the work, though this has been pushed rather far—the word "acid," for instance, being almost banished. The great merit of the book is, however, to be found in the very admirably-selected questions placed at the end of each chapter: we feel sure that any-one conscientiously endeavouring to understand and work these out would learn more, and that more thoroughly, than he would by a vast amount of desultory reading and rambling through of larger works. We would say to any candidate for the London matriculation, "Let him neglect Mr. Gill's advice about the marked chapters, and work conscientiously through the book."

Report of the Rugby School Natural History Society for the Year 1872. (Rugby: Billington, 1873.)

WE are sorry that the first words of this Report are words of complaint at the small number of real workers among the numerous members of this society; some of the Sections we regret very much to be told, are either deserted or inactive. We hope no such complaint will be called for next year, and that the new regulation as to membership may be of service as a stimulus to work among the younger associates; by this new rule the number of members is henceforward limited to 15, for the purpose of making election to membership a real distinction. To judge from the number and value of the papers in the Report, there are, after all, not a few really good workers among the members. Of the various selected papers and reports one-half are by members who were actual pupils of the school at the time they were written. B. R. Wise's paper "On the Earliness of the Season" (1872), shows the possession of a power of observation which, if carefully cultivated, ought to produce good results. The same may be said of A. G. Burchardt's paper on "The Work of the Anatomical Section," which contains an account of some of the animals found in the Rugby district, and some very useful directions on the preservation of specimens. E. J. Taylor's account of "A Visit to Norway" is interesting, and shows the author can make use of his eyes. L. Maxwell's essay on "Spectrum Analysis," well deserves the Society's Prize, which was awarded to it: the author shows that he possesses a clear idea of the nature of Spectrum Analysis, the principles on which it is based, and the many valuable purposes it is calculated to serve. It is accompanied by some rough but intelligible drawings of various absorption spectra. The second prize was awarded to an intelligent paper by H. N. Hutchinson on "Motive Power," in which the author describes and illustrates various substitutes for coal as generators of motive power, including an ingenious flux motor, or tidal engine. Among other interesting papers we would mention the valuable observations on *Hippocampus brevirostris*, by the Rev. T. N. Hutchinson; and some very curious facts as to protective mimicry in spiders, communicated by the Rev. C. W. Penny. From the Astronomical Report, by Mr. Wilson, we learn that a large amount of good work is being done, especially in solar observation. Appended to the report are Messrs. Lockyer and Seabroke's paper "On a New Method of Viewing the Chromosphere;" and a report on the November Meteors, by L. Maxwell. The Meteorological Observations seem to have been regularly and carefully taken, though we hope there will be more to report in the

Zoological Section as the result of the present year's work; the anatomical department of this section has, however, made a fair start under the direction of the late member, Mr. A. G. Burchardt. W. B. Lewis's Report of the Geological Section, with accompanying plates, shows there has been some activity in this department. A. F. Buxton's Entomological Report consists of a complete list of the Lepidoptera which have been noticed within eight miles of the School Close. Under Mr. Kitchener's, the President's, guidance, some good work has been done in the Botanical Section, though the workers seem to be few. Appended to the report of this section is an abstract of two papers by Mr. Kitchener on a Pelerian form of *Linaria vulgaris*. On the whole, the Report of this Society's work for 1872, is one of which there is no reason to be ashamed, and we hope that each year will add to the number of those who take an active part in the work. From many scientific societies it is not advisable nor often expedient to exclude non-workers, but in such societies connected with schools, it should be insisted on that every member be an active worker: only thus can they completely serve the purpose for which they are established.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Dr. Sanderson's Experiments and Archebiosis

THE letter by Dr. Sanderson, in last week's NATURE, contains an interesting and important confirmation of my experiments, which I was very glad to see. There are two or three points, however, which seem to require some comment.

In the first place the flasks and retorts after exposure to the heat were kept only from three to six days, before they were submitted to examination in order to ascertain whether fermentation had or had not taken place. But in cases in which fluids are exposed to heat for a long time, or are exposed to higher temperatures, the process of fermentation is almost invariably delayed and also modified in intensity. It must not therefore be supposed that fermentation would not have taken place at all in certain of Dr. Sanderson's flasks, simply because it had not occurred within four, five, or six days.

Secondly, Dr. Sanderson thinks his present experiments enable him to say that the particular fluid with which he experimented is not prone to undergo fermentation within six days, after it has been heated to a temperature of 100·92° C. I would ask Dr. Sanderson, however, whether he has been careful to observe the precise temperature attained by an infusion boiling rapidly in a flask from which the steam can find exit only through a capillary orifice—as in the experiments which we performed together?

Thirdly, I think it very desirable that Dr. Sanderson should state definitely to the scientific world what precise meaning he wishes to convey by his emphasized use of the word "*chance*" in the concluding paragraph of his letter. There seems a little ambiguity in his use of the word, which is the more to be regretted, since it occurs in the statement of an inference—where freedom from all possibility of misconception is so eminently desirable.

H. CHARLTON BASTIAN

University College, June 23

Spectrum of Nitrogen

IN a letter to NATURE (April 17th), Mr. Stearn throws some doubts on the accuracy of my experiments regarding the spectrum of nitrogen. I shall take the earliest opportunity of repeating and completing my experiments, and hope then to bring the question to a satisfactory close. As, however, some time may elapse before I can resume work, I wish to say now a few words in answer to Mr. Stearn's letter.

Before all, I wish to state clearly in what way the correctness of the opinion I profess with regard to the band-spectrum of nitrogen would be affected by an error introduced into my experiments. The unexpected result of an experiment of mine, together with a remark which Plucker makes in one of his papers, suggested to me the idea that the so-called band-spectrum of nitrogen might be that of the oxides of nitrogen. I was confirmed in this idea soon afterwards by a remark of Angstrom in his recent paper on double spectra (*Comptes Rendus*, August 17, 1871, but which was omitted in the English translation), by which he calls attention to the close resemblance of this band spectrum with the spectrum of metallic oxides. I have described in my paper the experiment just mentioned. A rather narrow tube showed, when exhausted, the lines of nitrogen; as soon as the air entered the bands appeared. The remark of Plucker alludes to the fact that a tube filled with oxides of nitrogen showed the bands with unusual brilliancy. In order to test the accuracy of this opinion I intended to make a crucial experiment by taking care to remove every trace of oxygen. I used for this purpose, at the suggestion of Dr. Balfour Stewart, small pieces of sodium heated in the vacuum tubes The sodium was fused several times in succession in order to free it from impurities. When the nitrogen was thus treated it always showed a line spectrum, the lines of which seemed to coincide with those of the known line spectrum of nitrogen when measured with the instrument at my disposal. It seems now that I have been too hasty in assuming that this apparent coincidence was a real one. While passing through London a few weeks ago, Dr Huggins was kind enough to allow me the use of his spectroscope in order to compare, under his supervision, the spectrum of my tubes with the real line spectrum of nitrogen. I then found that, although my tube shows a line which is very near the principal double line of nitrogen, the spectrum is not that of nitrogen. I am at present unable to say what is the origin of this spectrum; but I do not think that its formation can be brought forward as a proof that the band spectrum is not due to oxides of nitrogen. On the contrary, it rather shows that an impurity which has no effect on the spectrum of air, will have one when all the oxygen is removed, and that a change has therefore probably taken place in the conducting power of the gas which gives out the spectrum.

I do not quite see the real object of Mr. Stearn's letter. If he merely wishes to say that the proof brought forward by me is insufficient, and that the question must still remain an open one. I confess I have nothing to say against it. If he, however, wishes to convey the idea that nitrogen has really a double spectrum, I do not think his argument is a correct one.

I will not trespass any longer upon your space, but I may, I think, fairly ask your readers to suspend their judgment until I have completed my experiments.

Heidelberg, May 30

ARTHUR SCHUSTER

Ground Ivy

WITH respect to the question started in the number for June 12 of this journal as to the Ground Ivy, it may be said that in *Glechoma*, as also in *Origanum vulgare*, *Thymus serpyllum* and *vulgaris*, and *Mentha vulgaris*, specimens having flowers with small corolla and undeveloped anthers are very common, I think as common as specimens having flowers with large corolla and the two sexes developed. Also of *Mentha aquatica* and *Prunella vulgaris* specimens with smaller corolla and only pistils developed are found, but much more rarely than those of the other form.

I have attempted in my work to give an explanation of the origin of the second form of the above-mentioned Labiatæ, as follows:—

The species named are distinguished from our other Labiatæ by the coincidence of the following three peculiarities:—

1. By an abundance of honey, and in consequence of that by an abundance of insects visiting and cross-fertilising them.[*]

2. In the hermaphrodite flowers, by a stigma so far overtopping the anthers and developed so long after the anthers that self-fertilisation is impossible, or nearly so.

3. By a great variability in the size of the corolla in the hermaphrodite flowers of different specimens.

Now when the flowers on different stems of the same species differ in the size of their corolla, it is evident *a priori*, and ascertained by direct observation, that generally those with the largest corolla are the first seen and visited by insects flying near them, those with the smallest corolla the last. The latter, always the flowers last visited, are fertilised exclusively by the pollen of previously-visited flowers, consequently produce their pollen in vain; and since the non-production of useless organs is always an advantage to every organic being, varieties of the smallest flowered form must be favoured in the struggle for existence, when ceasing to develop their useless anthers. Thus of the smallest-flowered form, varieties with atrophied anthers of necessity remained at last the only survivors.

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[*] For instance, I found *Thymus serpyllum* visited by 7 species of Apidæ, 3 species of *Sphegidæ*, 14 species of Diptera, and 6 species of Lepidoptera, *Glechoma* visited by 21 species of Apidæ, 8 species of Diptera, and 3 species of Lepidoptera.

Lippstadt, June 17 H. MÜLLER

ALL the flowers of the ground ivy (*Nepeta Glechoma*) that I have this season examined, from this neighbourhood, have been of the stamenless form described by your correspondent "S. S. D." While spending a few days at Bath, I could find none but hermaphrodite flowers. At Hertford I found both forms, but a preponderance of hermaphrodites. These seem always more or less protandrous, and spontaneous self-pollenation is further prevented by the unequal lengths of the style and stamens.

Kilderry, Co. Donegal W. E. HART

Lotus corniculatus

MR. W. E. HART (NATURE, June 12) is quite right in correcting me on the subject of the fertilisation of *Lotus corniculatus*. It is the outer whorl of stamens, those opposite the calyx teeth, which continue to grow after the others, and which have their filaments dilated at the top so as to thrust the pollen out of the long sharp tube of the keel. I should scarcely have thought it necessary to acknowledge his courteous correction, if it were not for the following question and answer: How is it, then, that the pollen of the inner and shorter whorl of stamens, which discharge their pollen at the same time as the outer whorl, gets pushed out by the filaments of the outer whorl, since the anthers of the inner whorl lie below the summits of the filaments of the outer whorl? The answer is curious: In the early bud, before the anther cells begin to open, the inner whorl is obviously shorter than the outer whorl, so that the anthers of the former lie in a regular row entirely below the anthers of the latter, apparently for the convenience of close packing in the narrow closed flower. As the anther cells begin to open, which is just before the flower opens, the stamens of the inner whorl grow and approach very nearly in height to the stamens of the outer whorl; and as they shed their pollen from the summit of the anthers, their pollen comes out above the dilated tops of the filaments of the outer whorl, so that it can be pushed forwards by those filaments along with the pollen of their own anthers. The filaments of the inner whorl then wither and become comparatively short, while those of the outer whorl continue to grow, dilate, and stiffen, so as to do the work for all the pollen of both whorls. In the mature opened flower the difference between the two whorls becomes more marked than ever. If I am right, Mr. Hart's detection of my blunder leads to the notice of a curious instance of economy of space and of mechanism. T. H. FARRER

Abinger, Surrey, June 21

The Secchi and Respighi Methods

IN the number of NATURE for June 12, p. 136, I see that you notice the results obtained in the last eclipse with the use of the spectroscope for determining the first entrance of the moon or planet. There seems, however, to be some confusion in the report. You say that I propose Respighi's method for first contact, and my own for the last. This is not the case. I propose the common Respighi method as useful for obtaining a *first warning* of the entrance of the planet on the chromosphere. This is the only use I think it possible to make of it. But the real entrance must be obtained by my method, in which one sees the disc of the sun as with a common glass, and the line of the chromosphere tangent to it, can be seen broken at the instant of contact, as the ring of Venus is broken at its exit from the solar disc.

You say also (page 131, col. 1) that it is difficult to obtain a perfect adjustment on account of the inequality of the driving-clock. If you say so for the common spectroscopic method, I agree perfectly with you, because the edge of the disc cannot be seen; but with my method this difficulty does not exist. It is not more difficult to keep the sun's disc tangent to the chromospheric line, than to keep it tangent to a common wire; the clock can help, but it is not necessary to have it in perfect order; even with common handles one can obtain it. The reason is that the solar disc being perfectly visible, one is greatly helped by the edge of the sun itself, while in common methods the edge of the sun is not seen.

Rome, June 16 P. A. SECCHI

P.S.—More on this will be found in the *Memorie del. Soc. degli Spettroscopisti Ital.*

Gassendi and the Doctrine of Natural Selection

NO one having yet replied to the question in Mr. Monro's letter (see NATURE, vol. vii. p. 402), I venture to hope that you will give me space for a few remarks on Gassendi's physical philosophy, and more especially on that part of it germane to the subject discussed by Mr. Monro.

The apparent implication of the question referred to is, that anticipations of natural selection are to be found in Gassendi's writings. Allowing to the term its utmost latitude of meaning, this does not appear to me to be the case. In his historical sketch of the various views which poets and philosophers have held as to the origin of things, Gassendi gives the theory of Empedokles at some length, including the passage on the *βουγενῆ ἀνδρόπρωρα* which Mr. Monro quotes in his letter. But Gassendi has no word of approval for the theory; he classes it with other Greek cosmogonies, such as those of Anaximander, Pythagoras, &c., and with the Chinese and Hindu cosmogonies as "fabulares sententias philosophorum," not less fabulous indeed than the poetic fictions of Prometheus, Deukalion, and Kadmus. Here, too, as well as in other parts of his works, Gassendi blames philosophers for ascribing to the action of natural laws effects which he regards as direct results of the Divine power.

Before giving a brief summary of Gassendi's own views, I will premise that it is not easy to discover them with exactitude. His works are very voluminous, both the Lyons edition of 1658, and the Florence edition of 1728, occupying six bulky and closely printed folio volumes. Even the abridgment made by his disciple Bernier fills seven vols. 12mo. Ordinary histories of philosophy give for the most part a very meagre account of the French forerunner of Locke; and more comprehensive works, like those of Tennemann, Buhle, and De Gerando, deal with Gassendi as a psychologist and a moralist rather than as a physicist. Even Dr. Whewell, from whom, as the historian of the inductive sciences, more might have been expected, makes but a few cursory references to the philosopher who was one of the earliest and most pronounced followers of the Baconian method, and who, as De Gerando says, "enseignant les mêmes principes (as Bacon) les a surtout enseignés par son exemple." The work which, as far as I have seen, gives the most complete account of Gassendi as a physical philosopher is Schaller's "Geschichte der Naturphilosophie von Baco bis auf unsere Zeit." This writer takes Bacon, Hobbes, and Gassendi as the typical philosophers of the empirical or *a posteriori* school of natural philosophy. He devotes about one hundred pages to the exposition of Gassendi's physical doctrines, and concludes with an elaborate criticism of his atomic theory. The intrinsic obstacles to a precise appreciation of Gassendi's views are more serious. Not far removed from the age of scholasticism he exhibits, in a modified degree, two of the distinctive features of the schoolmen, their pedantic erudition, and their commentatorial spirit. The wealth of quotation with which his pages are burdened rather than adorned has laid him open to the charge "de laisser étouffer ses propres idées sous le poids des citations empruntées aux anciens." He better deserves the second than the first clause of Gibbon's epigrammatic eulogy: "Le meilleur philosophe des littérateurs, et le meilleur littérateur des philosophes." A work largely imbued with the commentatorial spirit, as the *Syntagma Philosophicum* is, is always more valuable as a history of philosophic opinion than as a source of new philosophic thought. Again Gassendi's bent of mind, coupled with the exigencies of his position as a Church dignitary, seems to me to have precluded his holding opinions of a very decided and novel character. True or not, the reason he is said to have given for adopting the atomism of Epicurus rather than the Cartesian theory of vortices is somewhat characteristic; "Chimera for chimera I cannot help feeling some partiality for that which is two thousand years older than the other."

In his views as to the origin of things, Gassendi is at once an atomist and a special creationist. One experiences a certain sense of incongruity in noticing the way in which, while following the Biblical narrative for the main outlines of his doctrine, he fills in the details from Atomism. In the beginning there was a chaos in which the Deity had intermingled in manifold confusion atoms, molecules, *corpuscula insectilia*, or *minima naturalia* (a phrase borrowed from Lucretius) of every kind, celestial and terrestrial, organic and inorganic, animal and vegetal. Upon these atoms had been impressed peculiar motions and affinities. At the creation of the world, as the creative fiats in their turn went forth, the potential motions and affinities of each species of atom became kinetic, and by the concourse of atoms, similarly endowed, the successive stages of creation were accomplished. There is so much resemblance between Gassendi's account of the appearance of the different animal forms, and the Miltonic narrative of the time when "the grassy sods now calved," that the question suggests itself whether the "Paradise Lost," which appeared in 1667, might not have been influenced by the *Syntagma Philosophicum*, its predecessor by some twenty years? From the side of Atomism Gassendi seeks to explain the Divine cessation from labour after the six stages of creation. Besides the atoms which, when endowed with kinetic energy, gave rise to the primordial plants and animals, there remained others in which their characteristic motions and affinities still continued potential, and which had been subject to distribution only. These account on the one hand for the seminal reproduction of plants and animals, and on the other for the phenomena of so-called spontaneous generation. On this view, as may be supposed, spontaneous generation presents few difficulties to Gassendi. He needs but the hypothesis of the endurance from the creation of the atoms special to any peculiar form of life. Then, when their potential motions and affinities become kinetic, they must of necessity issue in the forms of life which by their concourse they were destined to produce. Two points are worthy of notice in this connection—Gassendi's definition of spontaneous generation, and his list of animals produced spontaneously. Spontaneous generation is not generation "sine seminibus" (germs), but "sine parentibus." Amongst his "animalia sponte nascentia" are enumerated "mures, vermes, ranæ, muscæ, aliaque insecta."

In a theory such as this is there no evolution, no selection. The atoms themselves are unchangeable, and so are the specific characters of the aggregates which they build up. Plants and animals, as they now are, are but copies of the primitive forms, be they produced by gamogenesis or spontaneously. The natural conditions also by which floral and faunal habitats and distribution are regulated, Gassendi seems to regard as having been fixed once for all at the creation. Reading "Deus" for "Natura," Virgil's lines express Gassendi's views on this point—

> "Continuo has leges, æternaque fœdera certis
> Imposuit Natura locis"—(Geo I., vv. 60, 61)

There is a sort of superficial resemblance between Gassendi's atoms and Mr. Spencer's "physiological units," but with capital points of difference. In both theories the molecules of each species of plant and animal have distinctive characteristics, and an inherent power of arranging themselves in the form of the organism to which they appertain. But while Gassendi's atoms are simple and indivisible, as one of their synonymes, *corpuscula insectilia*, connotes, Mr. Spencer's physiological units are complex. While Gassendi's atoms are specific creations and endowed with unalterable properties, Mr. Spencer's physiological units are themselves the products of evolution, and are perpetually undergoing adaptation to equilibrate the action of forces internal and external.

I am inclined to suspect that Maupertuis may have, in the main, borrowed the atomic theory contained in the "Système de la Nature" from Gassendi. The materialism which led Maupertuis to make perception a fundamental property of his atoms is, however, all his own; at any rate it is not Gassendi's

In Physics as in Ethics, the nearest affinity of the philosophy of Gassendi is to that of Epicurus. It is Epicurianism modernised, and modified so as not to clash, openly at least, with Christianity and with the dogmas of the current theology. By his want of originality he was led to base his philosophy on an already established system, and by his adoption of Bacon's method he was attracted to Epicurus, for that philosopher and his school were the sole ancient representatives of the new *a posteriori* philosophy. De Gerando thinks that an additional link between Gassendi and Epicurus existed in the similarity of their views on the physical doctrines of a vacuum and of atoms. But it seems at least as probable that the French philosopher adopted these conceptions from the Greek, as that he reached them by his own independent thought. While, however, he was essentially an Epicurean, Gassendi was careful not to commit himself to any doctrines which might cause his orthodoxy to be questioned; in fact, he more than once clearly expresses this determination.

"How far back can traces of the great theory of Darwin and Spencer be discovered?" As I showed in my letter on Maupertuis, in NATURE, vol. vii. p. 402, the doctrine is discoverable in that writer; but De Maillet, with whom Mr. Spencer begins his historical sketch, is a quarter of a century earlier than Maupertuis. My examination of Gassendi leads me to the conclusion that the doctrine of Natural Selection is not to be found in his works, and further that his views, as far as I understand them, effectually preclude his holding the theory under any form.

W. H. BREWER

P.S.—On looking back over what I have written, I find that I have omitted to point out the different attitudes of Gassendi towards the two distinct portions of his cosmological views. When he is borrowing from the Mosaic account of the creation, all his assertions are positive, for here we have "quod Fides et Sacræ Literæ docent." When, however, he is borrowing from Atomism his views take a hypothetical form, and are introduced by the phrase "nihil vetat supponere."

Grace's Road, Camberwell

Care of Monkeys for their Dead

AS a supplement to the extract from James Forbes' "Oriental Memoirs," given by Dr. Gulliver in NATURE (vol. viii. page 103), the following incident, recorded by Capt. Johnson, deserves republication:—

"I was one of a party at Jeekarry, in the Bahar district; our tents were pitched in a large mango garden, and our horses were picqueted in the same garden at a little distance off. When we were at dinner, a Syce came to us complaining that some of the horses had broken loose in consequence of being frightened by monkeys (*i.e. Macacus Rhesus*) on the trees. . . As soon as dinner was over, I went out with my gun to drive them off, and I fired with small shot at one of them, which instantly ran down to the lowest branch of the tree, as if he were going to fly at me, stopped suddenly, and coolly put his paw to the part wounded, covered with blood, and held it out for me to see. I was so much hurt at the time that it has left an impression never to be effaced, and I have never since fired a gun at any of the tribe.

"Almost immediately on my return to the party, before I had fully described what had passed, a Syce came to inform us that the monkey was dead. We ordered the Syce to bring it to us, but by the time he returned, the other monkeys had carried the dead one off, and none of them could anywhere be seen."

G. J. R.

The Intellect of Porpoises

IN Prof. Huxley's admirable criticism of "Mr. Darwin's Critics,"* the following passage occurs:—"The brain of a porpoise is quite wonderful for its mass, and for the development of the cerebral convolutions. And yet, since we have ceased to credit the story of Arion, it is hard to believe that porpoises are much troubled with intellect."

I have no doubt that Prof. Huxley will agree with me in further concluding that "it is hard to believe" that the remarkably developed cerebral hemispheres of the porpoise with their deep and numerous convolutions perform no more exalted functions than the smooth pair of mere pimples that stand behind the olfactory ganglia of a cod-fish, and constitute the whole of his claim to a cerebrum proper.

The psychology of the porpoise (and also that of the dolphin and other cetaceans with similar brains) is thus a subject of primary interest to the student of cerebral physiology. As a contribution to the subject I offer the following fac[illegible]

Many years ago I made the voyage from Constantinople to London in a small schooner laden with box-wood, &c. The passage was very slow, occupying fully two months, including the whole of August, and parts of July and September. We were often becalmed, with porpoises playing about the ship. The sailors assured me that no sharks were in the neighbourhood while the porpoises were near, and accepting this generalisation I frequently plunged overboard and swam towards the porpoises. They usually surrounded me in a nearly circular shoal or company, and directed towards their unusual visitor an amount of attention which I may venture to dignify with the title of curiosity. Their respiratory necessities precluded any long-continued scrutiny, but after dashing upwards for their customary snort, they commonly resumed their investigations, sometimes approaching uncomfortably near and then darting off to the circumference of the attendant circle. I am not able to describe the expression on the *features* of a porpoise, but my recollection of that of the eyes of my swimming companions is very different from what I have since seen on the large vacant orbs of aquarium cod-fishes, &c.

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* *Contemporary Review*, 1871. Reprinted in "Critiques and Addresses."

I have not yet seen the porpoises in the Brighton Aquarium, but suspect that if they contrive to "make themselves at home" there, a careful study of their habits will remove some of the difficulty which Prof. Huxley experiences in believing in their intelligence.

W. MATTIEU WILLIAMS

Instinct

A DIFFICULTY occurred to me on reading Mr. Lewes's interesting and instructive article on "Instinct" in NATURE of April 10—and as no satisfactory answer offers itself to me, I venture to trouble you with it.

Wherein lies the difference in kind between the actions performed instinctively by animals for the preservation of themselves or their young, and those actions performed by plants with the same result?

For instance; the Ivy Linaria grows on an old wall; its flowers and the flower-stalks stand out for the sun and insects to visit the little "snap-dragon." But no sooner does the corolla fall, than the peduncle begins to curve inwards to the wall, and usually contrives to tuck its seed-vessel well into the brickwork again. We cannot say of such an action that there is "no alternative open to it;" and even if we do, it does not explain it to call it "impulsive," and yet one is not prepared to accept it as an instance of instinct. I shall be grateful for any elucidation.

M.

Grus vipio

I OBSERVE that in your report of the meeting of the Zoological Society on the 6th ult., in your issue of the 15th, it is stated, with reference to *Grus vipio* (*seu leucauchen*), that "no example of this fine species, so far as was known, had previously been brought alive to Europe." Last autumn, when going over the Zoological Gardens at Amsterdam with the superintendent, Mr. Hegt, I saw there a splendid pair of these birds, which had been purchased for 140*l*., and had bred the same spring, and reared successfully a fine young bird, about two-thirds grown when I saw it in September, destined, as I was informed by Mr. Hegt, for the Berlin Gardens. The collection of cranes at Amsterdam is exceedingly rich, far surpassing either London or Antwerp in this respect. It contained, when I saw it, fourteen out of the fifteen valid species of Grus, comprising, besides the above-mentioned, *G. vipio*, a splendid pair of *G. viridirostris*, a fine *G. leucogeranus*, *G. carunculatus*, *G. canadensis*, *G. Americana*, *G. torquata*, &c., the desideratum being *G. monacha*, of Japan.

W. A. FORBES

Culverlea, Winchester, June 2

ON THE SYNTHESIS OF MARSH-GAS AND FORMIC ACID, AND ON THE ELECTRIC DECOMPOSITION OF CARBONIC OXIDE *

IN connection with the investigation on the electric decomposition of carbonic-acid gas referred to in a previous communication to the Society, I was led to submit a mixture of hydrogen and carbonic-oxide gas to the action of electricity in the induction-tube, the mixed gases being circulated through the tube by means of an apparatus which I will not now describe. A contraction was soon observed to have taken place, which at the end of an hour amounted to 10 cub. centims. The rate of contraction steadily diminished, and during the fifth hour of the duration of the experiment amounted to only 2 cub. centims. The experiment was stopped, and the gas analyzed with the following results in two several analyses:—

I.		II.	
Carbonic oxide	61·65	Carbonic oxide	61·35
Hydrogen	32·16	Hydrogen	32·34
Marsh-gas	6·14	Marsh-gas	6·31
	100·00		100·00

A small quantity (about 2 per cent.) of nitrogen was also contained in the gas, together with a trace of oxygen, which have been omitted from the calculation.

The result of this reaction is expressed in the following equation:—

$$CO + 3H_2 = CH_4 + H_2O.$$

This fundamental experiment, which constitutes the basis of a new method of chemical synthesis, susceptible of the most varied applications, and of peculiar interest in reference to the explication of natural phenomena, was commenced by me on the 10th of January last at Oxford, in the laboratory of my friend and successor in the Chair of Chemistry, Prof. Odling; two analyses of the gas were completed, and the results attained in the course of a week from that date. In a similar experiment made with a mixture of hydrogen and carbonic-acid gas, a contraction also occurred, attended with the formation of water. The gas which resulted from the experiment was found to consist (after the absorption of carbonic acid) of hydrogen and carbonic oxide, together with a little marsh-gas. Traces of oxygen and nitrogen were also present. Minute drops, too, of an oily liquid appeared in the tube. This liquid, after the conclusion of the experiment, was dissolved in a small quantity of water. The solution was strongly acid and had a pungent taste. It reduced an alkaline solution of terchloride of gold and an ammoniacal solution of nitrate of silver. These reactions are the characteristic properties of formic acid, of which we may infer the synthesis to have been effected according to the equation

$$H_2 + CO_2 = H_2CO_2.$$

I may avail myself of the present opportunity to place on record the following important facts in reference to the action of electricity on carbonic-oxide gas.

When pure and dry carbonic oxide is circulated through the induction-tube, and there submitted to the action of electricity, a decomposition of the gas occurs, attended with a gradual and regular contraction, which, in the form assumed in my experiments, occurred at the regular rate of about 5 cub. centims. in an hour. Carbonic acid is formed, and simultaneously with its formation a solid deposit may be observed in the induction-tube. This deposit appears as a transparent film of a red-brown colour, lining the walls of the tube. It is perfectly soluble in water, which is strongly coloured by it. The solution has an intensely acid reaction.

The solid deposit in the tube, in the dry condition before it has been in contact with water, is an oxide of carbon. Samples, however, made in different experiments do not present precisely the same composition; but nevertheless they appear to belong to a certain limited number of forms which repeatedly occur, and may invariably be referred to the same general order or system. This system is, or appears to be, what I may term a homologous series of "oxycarbons," of which the unit of carbon with the weight 12 may be regarded as the first term, and of which the adjacent terms differ by an increment of carbonic oxide (CO) weighing 28, precisely as homologous series of hydrocarbons differ by the increment CH_2 with the weight 14. I have succeeded in identifying by analysis two at least of these substances, namely, the adjacent terms C_4O_3 and C_5O_4. From this point of view these peculiar bodies are members of a series of oxycarbons analogous in the oxycarbon system to the series of hydrocarbons of which the unit of carbon is the first and the unit of acetylene C_2H_2 is the second term, the oxycarbon C_4O_3 being represented in that series by the hydrocarbon crotonylene C_4H_6 and the oxycarbon C_5O_4 by the hydrocarbon valerylene C_5H_8.

THE LAW OF STORMS DEVELOPED *

III.

FROM the Cape of Good Hope, in a straight line toward the projecting eastern coasts of Brazil, mariners have found a peculiar streak of south-easterly winds.

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* A paper read at the Royal Society by Sir B. C. Brodie, Bart., D.C.L., F.R.S., late Waynflete Professor of Chemistry in the University of Oxford.

* Continued from p. 148.

Between the island of Tristan da Cunha and the Cape, and northward and westward to the island of Fernando Noronha, this streak of powerful winds, with which nothing in the trade-wind region of the North Atlantic can compare, has its atmospheric current as sharply marked as the dark blue and rapid current of the Gulf Stream in the Narrows of Bemini. It is, doubtless, the region or band of most intensely acting south-east trades, and is probably due to the peculiar configuration of the shores of the South Atlantic, and to the wall of the South American Andes. It is a well-known fact that the volcanic cone of Teneriffe, which lies in the zone of north-east trades, intercepts the wind and gives it a lateral deflection; so that, while the trades are blowing strongly on the north-east side of the island, on the opposite side there is a distinctly-marked and carefully-measured calm shadow. Now, the chain of the Andes endeavours to exert on the south-eastern trades just such an influence as is exerted by the Canary Islands on the north-east trades. This influence, in the former case, suffices to throw off from the Continent of South America a large body of the south-east trades, and to deflect it to the eastward, giving it the character of a south-south-west wind, and, at the same time, by forcing a greater or more concentrated body of air into the regions north-east of Brazil, imparting an increased velocity and violence to the air-current. It is, therefore, in the air-current that the homeward-bound vessel from the Cape of Good Hope aims to steer, because she is sure of being wafted happily and swiftly to her destination.

It has long been demonstrated by meteorologic observations, taken both at sea and on land, that there is very much *less* atmosphere in the Southern Hemisphere than in the northern, and for a long time physicists were at a loss to account for the difference. It has been, however, very satisfactorily explained by the eminent American mathematician, Ferrel, in his work on the "Motions of Fluids and Solids, relative to the Earth's Surface," where he proves at length, and states in detail (p. 39). "As there is much more land, with higher mountain ranges, in the Northern Hemisphere than in the southern, the resistances are greater, and consequently the eastward motion of the air, upon which the deflecting force depends, is much less; and the consequence is, that the more rapid motions of the Southern Hemisphere cause a greater depression there, and a *greater part of the atmosphere to be thrown into the Northern Hemisphere.*" It is, doubtless, to this tendency of the Southern Hemisphere to throw off much of its atmosphere north of the equator that we may attribute in part the superior force and power of the south-east trades, and their well-known ability to battle with the north-east trades, and drive them from their own territory, at least all summer, and even in winter, as far back across the line as 3° or 4° north latitude. Mr. Ferrel, speaking of the principle just enunciated, well says: "This also accounts for the mean position of the equatorial calm-belt being, in general, a little north of the equator. But, in the Pacific Ocean, where there is nearly as much water north of the equator as south (and the resistances are usually equal), its position nearly coincides with the equator." In other words, just as a bucket full of water revolving on a perpendicular axis would show a depression in the centre, and the fluid be thrown from all sides of its rim, the Southern Hemisphere throws its water and its atmosphere into the Northern hemisphere, all along the equator.

It is, therefore, a mathematical and mechanical certainty that there is an invasion of the north-east trade-wind belt from the south-east trades, and observation powerfully bears out the deduction of the mathematician. Ansted states in his cautiously-written "Physical Geography:"—"The southern trade-wind region is much larger than the northern in the Atlantic Ocean. In this sea, the south-east trades are fresher, and blow stronger, than the others, and often reach to the 10th or 15th parallel of north latitude; whereas the northern trade-wind seldom gets south of the equator, and usually ranges from 9° to 29° north latitude" (p. 253). It is not difficult to see how easily it happens that a very small atmospheric eddy found in the tropical Atlantic by the conflictory north-east and overleaping south-east trade-winds may soon become a hurricane of wide extent and of tremendous energy. All that is necessary, as we have before seen, is that an initial impulse of gyration be given to a body of air. The moment that this takes place by mechanical influence, and centrifugal force creates the smallest eddy or vortex, the surrounding air, already highly charged with moisture, begins the process of convergence and ascensional motion, followed rapidly by condensation aloft.

The storm-cylinder—the nucleus of the hurricane—originally very small, is instantly enlarged and expanded by the evolution of latent heat stored away in the vesicles of aqueous vapour. For some hours, as all observations show to be actually the case, the incipient cyclone scarcely moves, while gathering in its energies and laying tributes upon all contiguous regions. The process continues with momentarily increasing intensity, and, before the sun has made his daily circuit, the meteor is formed.

If it be asked along what parallels of latitude in our hemisphere this formation takes place, the intelligent reader will at once answer, Near the terrestrial circle of trade-wind interference. This, we have already seen, is in summer, from the 10th to the 12th parallels of north latitude.

This slender zone of debatable ground is the battle-field of the two opposing bands of the trades. There is really no need of observations to tell us as much. But millions of observations attest the fact. Every seaman knows it. Every meteorological writer tells the same story. You have only to examine physical charts from the time of Columbus and Magellan to this, to see the absolute unanimity of testimony, and to discover that the hypothesis now advanced, and the known facts of the case, are in perfect and minute accord.

If it be asked whether the origin and interest of the West-Indian gales is *solely* due to mechanical interference, the proper reply, it would appear, should be in the negative. As the south-east trade-wind comes laden with the vapour of the southern or water hemisphere, which Dové well called "the boiler" of the globe, it is met by the cold north-east trade from the northern, or land hemisphere. There must be a great difference in their temperatures, and consequently extensive condensation, which, by the reasoning of Mr. Clement Ley, would, of itself, explain the formation of the storm. That condensation greatly assists in producing or intensifying it, cannot be doubted. In the high latitudes, where the polar air-current is sometimes forced by barometric pressure into the southerly or equatorial current moving over the warm waters of the ocean, and thus heavily vapour-laden, the consequence is illustrated by such terrific and sudden tempests as that of the *Royal Charter*, distinctly proved by Admiral Fitzroy to have been generated between the opposite polar and equatorial currents off the coast of Wales.

But that the origin of great depression-systems is solely due to condensation can hardly be sustained, and seems entirely overthrown if we regard the single fact that, on the great equatorial belt—the belt of perennial precipitation—no hurricane or typhoon has ever been experienced by the mariner. It has long been, and is now, the almost universally accepted theory of meteorologists, that the reason no cyclones have ever been known to occur on the equator is, that there the earth's rotation exerts a deflecting influence on the winds, amounting to zero, and hence the formation of a whirl is impossible. This view is not satisfactory, because the nucleus of a depression once formed on the equator, there would be intro-moving masses of air proportioned in violence to the amount of the depression and the steepness of the barometric gradient down which they rush to reach the point of lowest barometer. The true reason that no great cyclone has ever been formed nearer the equator than the third parallels of latitude appears to be, that the equatorial belt is a belt of *non-interference.*

WEATHER-CHART OF GREAT BRITAIN, BEFORE "ROYAL CHARTER" STORM.
Full-feathered arrows show Polar current, half-feathered arrows show Equatorial current, dark-coloured surface not reported by vessels or land-observers.

ON THE ORIGIN AND METAMORPHOSES OF INSECTS*

VII.

ON THE ORIGIN OF INSECTS

"PERSONNE," says Carl Vogt, "en Europe au moins, n'ose plus soutenir la Création indépendante et de toutes pièces des espèces," and though this statement is perhaps not strictly correct, still it is no doubt true, that the Doctrine of Evolution, in some form or other, is accepted by most, if not by all, the greatest naturalists of Europe. Yet it is surprising how much, in spite of all that has been written, Mr. Darwin's views are still misunderstood. Thus Browning, in one of his recent poems, says :—

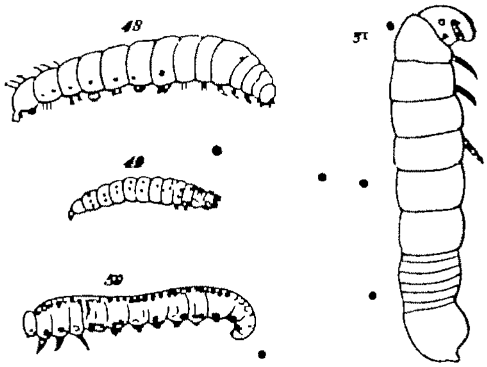

Fig. 48, Larva of Moth (*Agrotis suffusa*), after Packard. 49, Larva of Beetle (*Haltica*), after Westwood. 50, Larva of Sawfly (*Cimbex*), Brischke and Zaddach. Beob. ub d. arten. der Blatt und Holzwespen, Fig 8 51, Larva of Julus. Newport, Philos Transactions, 1841.

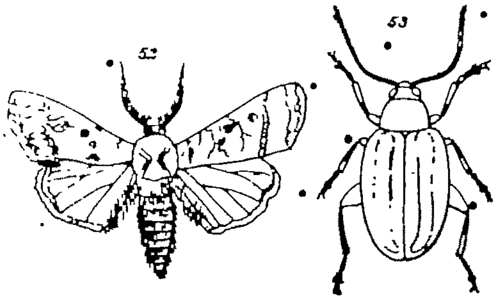

Fig. 52, Agrotis suffusa (after Packard). 53, Haltica (after Westwood).

> "That mass man sprang from was a jelly lump
> Once on a time ; he kept an after course
> Through fish and insect, reptile, bird, and beast,
> Till he attained to be an ape at last,
> Or last but one."†

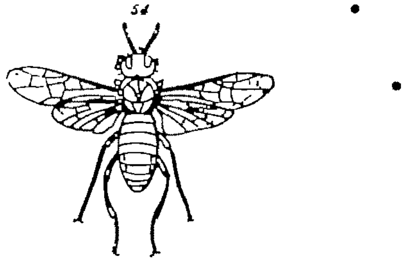

Fig. 54, Cimbex, Brischke and Zaddach, l.c. T. 2, Fig. 9.

Yet this is a theory which Mr. Darwin would entirely repudiate ; which is utterly inconsistent with his views. Whether fish and insect, reptile, bird, and beast, are derived from one original stock or not, they are certainly not links in one sequence. I do not, however, propose to discuss the question of Natural Selection, but I may observe that it is one thing to acknowledge that in Natural Selection, or the survival of the fittest, Mr. Darwin has called attention to a *vera causa*, has pointed out the true explanation of certain phenomena ; but it is quite another thing to maintain, that all animals are descended from one primordial source.

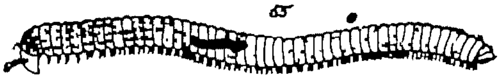

Fig. 55, Julus (after Gervais).

For my own part, I am satisfied that Natural Selection is a true cause, and that whatever may be the final result of our present inquiries—whether animated nature is derived from one ancestral source, or from many—the publication of the Origin of Species will not the less have constituted an epoch in the History of Biology. But, how far the present condition of living beings is due to that cause ; how far, on the other hand, the action of Natural Selection has been modified and checked by other natural laws—by the unalterability of types, by atavism, &c. ; how many types of life originally came into being ; and whether they arose simultaneously or successively,—these and many other similar questions remain unsolved, even if we admit the theory of Natural Selection. All this has indeed been clearly pointed out by Mr. Darwin himself, and would not need repetition but for the careless criticism by which in too many cases the true question has been obscured. Without, however, discussing the argument for and against Mr. Darwin's conclusions, we so often meet with travesties of it like that which I have just quoted, that it may be worth while to consider the stages through which some group, say for instance

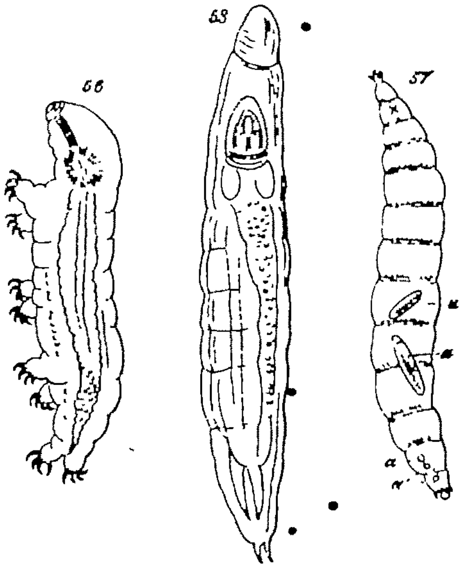

Fig. 56, Tardigrade (after Dujardin). 57, Larva of Cecidomyia (after Packard). 58, Lindia torulosa (after Dujardin).

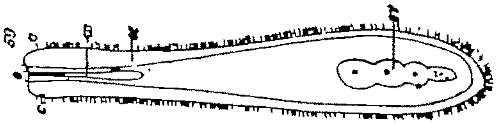

Fig. 59, Prorhynchus stagnalis.

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* Continued from p. 140.

† Prince Hohenstiel Schwangau, p. 68.

that of insects, have probably come to be what they are, assuming them to have developed under natural laws from simpler organisms. The question is one of great difficulty. It is hardly necessary to say that insects cannot have passed through all the lower forms of animal life, and the true line of their development would not at present be agreed upon by all naturalists. In this question embryology and development are perhaps our best guides. The various groups of Crustacea, for instance, greatly as they differ in their mature condition, are for the most part very similar when they quit the egg. Haeckel, in his "Naturliche Schopfungsgeschichte," gives a diagram which illustrates this very clearly.

In the case of insects, the gradual course of evolution through which the present condition of the group has been probably arrived at, has been discussed by Mr. Darwin, by Fritz Müller, Haeckel, Brauer, myself and others. At first sight the differences are indeed great between the various groups of insects. The stag beetle, the dragon fly, the moth, the bee, the ant, the gnat, the grasshopper—these and other less familiar types seem at first to have little indeed in common. They differ in size, in form, in colour, in habits, and modes of life. Yet the researches of entomologists, following the clue supplied by the illustrious Savigny, have shown, not only that while differing greatly in details, they are constructed on one common plan; but also that other groups, as for instance, Crustacea (Lobsters, Crabs, &c.) and Arachnida (Spiders and Mites), can be shown to be fundamentally similar. In Pl. 4 I have figured the larvæ of an Ephemera (Fig. 1), of a Meloë (Fig. 2), of a Dragon Fly (Fig. 3), of a Sitaris (Fig. 4), of a Campodea (Fig. 5), of a Dyticus (Fig. 6), of a Termite (Fig. 7), of a Stylops (Fig. 8), and of a Thrips (Fig. 9). All these larvæ possess many characters in common. The mature forms are represented in the corresponding figures of Plate 3, and it will at once be seen how considerably they differ from one another. The same fact is also illustrated in Figs. 48—55, where Figs. 48—51 represent the larval states of the mature forms represented in Figs. 52—55. Fig. 48 is the larva of a moth, *Agrotis suffusa* (Fig. 52); Fig. 49 of a beetle, *Haltica* (Fig. 53); Fig. 50 of a Saw Fly, *Cimbex* (Fig. 54); and Fig. 5[illegible] of a Centipede, *Julus* (Fig. 55).

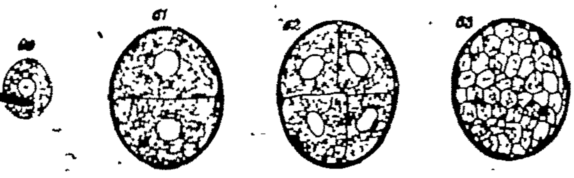

FIG. 60, Egg of Tardigrade, Kaufmann, Zeit. f. Wiss. Zool. 1851, Pl. 1; 61, Egg of Tardigrade after the yolk has subdivided 62, Egg of Tardigrade in the next stage 63, Egg of Tardigrade more advanced.

Thus then, although it can be demonstrated that perfect insects, however much they differ in appearance, are yet reducible to one type, the fact becomes much more evident if we compare the larvæ. M. Brauer[*] and I[†] have pointed out that two types of larvæ, which Packard has proposed to call the Eruciform and Leptiform, run through the principal groups of insects. This is obviously a fact of great importance: as all individual Meloës are derived from a form resembling Plate 2, Fig. 2, it is surely no rash hypothesis to suggest that the genus itself may be so.

Firstly, however, let me say a word as to the general Insect type. It may shortly be described as consisting of animals, possessing a head, with mouth-parts, eyes, and antennæ; a thorax made up of three segments, each with a pair of legs; and a many-segmented abdomen with anal appendages. Into the internal anatomy I will not now enter. It will be seen that Plate 4, Fig. 4, representing the larva of a small beetle named Sitaris, answers very well to this description. Many other Beetles are developed from larvæ closely resembling those of Meloë (Plate 4, Fig. 2), and Sitaris (Plate 4, Fig. 4); in fact—except those species the larvæ of which, as, for instance of the Weevils (Plate 2, Fig. 6), are internal feeders, and do not require legs—we may say that the Coleoptera generally are derived from larvæ of this type.

I will now pass to a second order, the Neuroptera. Plate 4, Fig. 1, represents the larva of Chloëon, a species the metamorphoses of which I described some years ago in the Linnean Transactions,[*] and it is obvious that in essential points it closely resembles the form which I have just described.

The Orthoptera, again, the order to which Grasshoppers, Crickets, Locusts, &c. belong, commence life in a similar condition, and the same may also be said of the Trichoptera.

From the difference in external form, and especially the large comparative size of the abdomen, the larvæ of Lepidoptera (Fig. 48), and of certain Hymenoptera, for instance, of Sirex (Fig. 14) and of the Saw Flies (Fig. 50), have generally been classed with the maggots of Flies, Bees, Weevils, &c., rather than with the more active form of larva just adverted to. This seems to me, as I have already pointed out,[†] to be a mistake. If we look, for instance, at the larva of Tenthredo we see the three thoracic segments well marked, and the three pairs of legs. The abdominal prolegs, indeed, give the larvæ a very different appearance to those of the preceding type, but in some respects remove them still further from the apod, vermiform, larvæ. The larvæ of other species belonging to this group, for instance of Lyda, have no abdominal prolegs, and well developed though short antennæ. The caterpillar type differs then in its general appearance owing to its greater clumsiness, but still essentially agrees with that already described.

No Dipterous larva, so far as I know, belongs truly to this type; in fact the early stages of the pupa in the Diptera seem in some respects to correspond to the larvæ of other Insect orders. The Development of the Diptera is, however, as Weissman[‡] has shown, very abnormal in other respects.

Thus then we find in many of the principal groups of insects that, greatly as they differ from one another in their mature condition, when they leave the egg they consist of a head; a three-segmented thorax, with three pairs of legs; and a many-jointed abdomen, often with anal appendages. Now is there any mature animal which answers to this description? We need not have been surprised if this type, through which it would appear that insects must have passed so many ages since (for winged Neuroptera have been found in the carboniferous strata) had long ago become extinct. Yet it is not so. The interesting genus Campodea (Pl. 3, Fig. 5) still lives; it inhabits damp earth, and closely resembles the larva of Chloëon (Pl. 2, Fig. 1), constituting, indeed, a type which, as shown in Pl. 4, occurs in many orders of insects. It is true that the mouth parts of Campodea do not resemble either the strongly mandibulate form which prevails among the larvæ of Coleoptera, Orthoptera, Neuroptera, Hymenoptera, and Lepidoptera; or the suctorial type of the Homoptera and Heteroptera. It is, however, not the less interesting or significant on that account, since, as I have elsewhere endeavoured to point out, its mouth parts are intermediate[§] between the mandibulate and haustellate types; a fact which seems to me highly significant.

It seems to me, then, that there are good grounds for considering that the various types of insects are descended from ancestors more or less resembling the genus Campodea, with a body divided into head, thorax, and abdomen; the head provided with mouth-parts, eyes, and one pair of antennæ, the thorax with three pairs of legs, and the abdomen, in all probability, with caudal appendages.

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[*] Wien. Zool. Bot. Gesells. 1869

[†] Linnean Transactions, 1863.

[*] Linnean Transactions, 1866, vol. xxv.

[†] Linnean Transactions, vol. xxiv. p. 65.

[‡] Siebold and Kolliker's Zeits. f. Wiss. Zool., 1864.

[§] Linnean Journal, v. xi.

If these views are correct, the genus Campodea must be regarded as a form of remarkable interest, since it is the living representative of a primæval type from which not only the Collembola and Thysanura, but the other great orders of insects have derived their origin.

This ancient type may possibly have been derived from a less highly developed one, resembling the modern Tardigrades, a (Fig. 56) smaller and much less highly organised being than Campodea, which has been successively placed among the Acari and the Rotatora. It possesses two eyes, three anterior pairs of legs, and one at the posterior end of the body, giving it a curious resemblance to some Lepidopterous larvæ.

These legs, however, as it will be seen, are reduced to mere projections. But for them, the Tardigrada would closely resemble the vermiform larva so common among insects. Among the Coleoptera, for instance, the vermiform type occurs in the weevils; among Hymenoptera in the Bees and Ants; among Diptera it is general. Among Trichoptera the larva early acquires the three pairs of legs, but as Zaddach has shown,* there is a stage, though it is quickly passed through, in which the divisions of the body are indicated, but no trace of legs is yet present. Indeed, there appear to be reasons for considering that while among Crustacea the appendages appear before the segments, in Insects the segments precede the appendages, although this stage of development is very transitory, and apparently, in some cases, altogether suppressed. I say "apparently," because I am not yet satisfied that it will not eventually be found to occur in all cases. Zaddach, in his careful observations of the embryology of Phryganea, only once found a specimen in this stage, which also, according to the researches of Huxley,† seems to be little more than indicated in Aphis. It is therefore possible that in other cases, when no such stage has been observed, it is not really absent, but, from its transitoriness, has hitherto escaped attention.

Fritz Muller has expressed the opinion ‡ that this vermiform type is of comparatively recent origin; he says, "the ancient insects approached more nearly to the existing Orthoptera, and perhaps to the wingless Blattidæ, than to any other order, and the complete metamorphosis of the Beetles, Lepidotera, &c. is of later origin." "There were," he adds, "perfect insects before arvæ and pupæ." This opinion has been adopted by Mr. Packard § in his "Embryological Studies on Hexapodous Insects."

M. Brauer ‖ also considers that the vermiform larva is a more recent type than the Hexapod form, and is to be egarded not as a developmental form, but as an adaptational modification of the earlier active hexapod type. In proof of this he quotes the case of Sitaris.

Considering, however, the peculiar habits of this genus, to which I have already referred, and that the vermiform type is altogether lower in organisation and less differentiated than the Campodea form, I cannot but regard this case as exceptional; as one in which the development has been, so to ay, "falsified" by the struggle for existence, to use an xpression of Fritz Muller's, and which therefore does not truly indicate the successive stages of evolution. On the contrary, the facts seem to me to point to the conlusion that, though the grublike larvæ of Coleoptera, and some other insects, owe their present form mainly to the influence of external circumstances, and partially also to atavism, still the Campodea type is itself derived from earlier vermiform ancestors. Nicolas Wagner has shown in the case of a small gnat, allied to Cecidomyia, that even now, in some instances, the vermiform larvæ retain the power of reproduction. Such a larva (as, for instance, Fig. 57) very closely resembles some of the Rotatoria, such, for instance, as Albertia or Notommata; these differ generally in possessing vibratile cilia. There is, however, one genus—Lindia (Fig. 58)—in which these ciliæ are altogether absent, and which, though resembling Macrobiotus in many respects, differs from that genus in being entirely destitute of legs. I have never met with it myself, but it is described by Dujardin, who found it in a ditch near Paris, as oblong, vermiform, divided into rings, and terminating posteriorly in two short conical appendages. The jaws are not unlike those of the larvæ of Flies, and indeed many naturalists meeting with such a creature would, I am sure, regard it as a small Dipterous larva; yet Dujardin figures a specimen containing an egg, and seems to have no doubt that it is a mature form.*

JOHN LUBBOCK

(*To be continued.*)

AMERICAN SCIENTIFIC EXPEDITIONS†

THE present year will be pre-eminently characterised in the history of the United States by the number of scientific expeditions, thoroughly equipped in every respect, and fitted out for exploration in various regions of the great West; and although most of them have been already referred to in our columns, it may be well to recapitulate them in geographical order. The most northerly is the International Northern Boundary Commission, which is intended to survey the line of the forty-ninth parallel, from the Lake of the Woods to the crest of the Rocky Mountains. The survey of the eastern section of the northern boundary of the United States was completed many years ago by Colonel J. D. Graham and others, and that of the western section, from the Pacific coast to the Rocky Mountains, was brought to a close in 1860. The middle section, as was the western, is in charge of Archibald Campbell, Esq., of Washington, as commissioner, with Major Twining as chief engineer officer on the part of the United States. Dr. Elliott Coues, of the army, the well-known naturalist, accompanies the expedition in that capacity, and the work will be done in connection with a large party, equally well equipped, detailed by the British Government.

The labour of this Commission was begun in 1872, consisting in the examination of the line from the Lake of Woods to Pembina, this village being the starting-point for the present year.

The next expedition is that along the line of the Northern Pacific Railway, and will consist of a body of about 2,000 troops, under the immediate command of Colonel D. N. Stanley. This will concentrate at Fort Abraham Lincoln, on the Missouri, now representing the western terminus of the Northern Pacific Railway, and its route will be westward toward and across the Yellow Stone River. This large force is intended to keep the Indians in check, and prevent any interferences on their part with the location and construction parties of the railway. In view of the fact that this expedition passes through a rich but little-known country, abounding in objects of natural history and zoology, the president of the National Academy of Sciences memorialised the Secretary of War in reference to the appointment of a corps of scientific men to accompany it; and this communication being favourably received, a number of gentlemen were duly commissioned. Some of these, however, subsequently found themselves unable to carry out their intention; but finally an organisation was completed, with Mr. J. A. Allen, of Cambridge, as zoologist; Dr. Lionel R. Nettre, of New York, as mineralogist and geologist; Mr. William Pywell, of Washington, as photographer; Mr. Edward Konopicky, of Cambridge, as zoological and landscape artist; and Mr. C. W. Bennett as general assistant. These gentlemen have been commended especially to the kind attentions of General Sheridan and Colonel Stanley, and will receive every facility possible for carrying on their work.

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* Unters. ub. die Entwick. und der Bau der Gliederthiere, p. 73

† Linnean Transactions, v. xxii.

‡ Facts for Darwin, trans by Dallas, p 118.

§ Mem. Peabody Academy of Science, v 1. No 3.

‖ Wien. Zool. Bot. Gesells 1869, p 310.

* See also the descriptions given by Dujardin (Ann. des Sci Nat. 1851, v. xv) and Claparède (Anat. and Entwickl der Wirbellosen Thiere) of the interesting genus Echinoderes, which these two eminent naturalists unite in regarding as intermediate between the Annelides and the Crustacea

† Communicated by the Scientific Editor of *Harper's Weekly*.

The next expedition is that of Prof. F. V. Hayden, who continues the work upon which he has been engaged for so many years. His starting-point is Denver, and the region to be explored lies south of the fortieth parallel of latitude, and extending from Green River on the west to the eastern base of the Rocky Mountains. He expects to occupy several successive years in proceeding toward the Mexican boundary. The expedition has been divided into several parties, each with its commander. The general topographical and surveying work is under the direction of Mr. James T. Gardner, so well known in connection with Mr. Clarence King's explorations. Some of the specialists accompanying the expedition are Dr. F. M. Endlich and Mr. Marvin as geologists, and Mr. J. H. Batty as zoologist.

The next survey in the geographical order of arrangement is that of Lieutenant George M. Wheeler, in continuation of the labours of several preceding years. This expedition will be divided into four main field parties, one of which will be again subdivided, and includes four astronomical and triangulation parties. Party No. 1, under charge of Lieutenant Wheeler himself, will operate in portions of New Mexico and Arizona, and will be accompanied by Mr. G. K. Gilbert as chief geologist, and Dr. Oscar Loew as assistant geologist. Party No. 2, under Lieutenant Hoxie, will be accompanied by Mr. E. E. Howell as geologist, and Mr. H. W. Henshaw as naturalist. This party will move from Salt Lake to Camp Wingate, passing through portions of New Mexico and Arizona. The third party, under Lieutenant William L. Marshall, with Prof. J. J. Stevenson as geologist and mineralogist, and Dr. J. L. Rothrock as medical officer and naturalist, will move south-west from Denver through to Wingate, and explore also a portion of New Mexico and Arizona.

The fourth, or triangulation party, will start from Santa Fé, and carry a system of triangulation west to the meridian of Fort Wingate, and thence south to the Mexican border. The first astronomical party will be stationed at Salt Lake, with Mr. J. H. Clarke as observer; the second will be on the Denver and Santa Fé line, Dr. F. Kampf, observer; the third will be on the Union Pacific and the Central Pacific Railroad lines, with William W. Maryatt as observer; and the fourth party at Ogden, with Prof. H. B. Herr as observer. Here an observatory will be constructed for receiving signals from communicating stations, with a view of establishing differences of longitude.

The expedition of Major J. W. Powell on the Colorado River, in Utah, comes next in order, this gentleman being now occupied in finishing his work and preparing his report in compliance with the Act of Congress. Major Powell had been several years in this region, and has already constructed a map of wonderful interest and great accuracy. In connection with his work he has made a very large ethnological collection relating to the Piute Indians.

The explorations of Mr. Clarence King, who has been engaged for several years in the survey of the line of the fortieth parallel, will, it is understood, be completed during the present season by reviewing some portions of the route already traversed.

The engineer expedition under Captain Jones will also proceed from Cheyenne along the Wind River Mountains to some point on the Upper Missouri, and will be accompanied by Dr. Parry, the well-known botanist. It is also understood that a large Government party will start from Fort Ellis and proceed eastward, and form part of the Yellowstone expedition already referred to.

The exploration of Alaska will also be prosecuted in behalf of the Coast Survey by Mr. William H. Dall, who has already proceeded to the Aleutian Islands, with a view of preparing a proper chart of the same, and especially of selecting a suitable landing-place for the proposed Pacific Ocean cable. The labours of Mr. Henry W. Elliott and Captain Bryant in the islands of St. Paul and St. George, in Behring Sea, will, it is hoped, be as productive as in 1872.

Nearly all the parties referred to, while, of course, prepared for prosecuting the topographical, geographical, and astronomical service, are accompanied by competent geologists, botanists, and zoologists, and there is reason to believe that the amount of material which will be transmitted by them to the National Museum will exceed in magnitude and value that of any previous year since its establishment in 1857.

NOTES

AT a meeting of the Geographical Society on Monday evening, Sir Bartle Frere, who was in the chair, intimated that the Queen had been graciously pleased to grant a pension of 300*l.* a year to Dr. Livingstone. We are glad to see that the daily press is becoming alive to the scandal of putting off with such a paltry gift a man who has spent his life in the disinterested service of his country and of humanity: he has surely *earned* something more handsome. Sir Bartle Frere read a letter from Dr. Kirk, which stated that the East Coast Expedition was getting on well, and that its members were in good health. Dr. Dillon and Lieutenant Cameron had succeeded in traversing the wet country, and were now engaged in collecting porters on the inland side of the river. Lieutenant Murphy and Mr. Moffat were understood to be following. His arrival had done much for the assistance of the expedition. No further news had of late been received of the expedition, a circumstance regarded by Dr. Kirk in a favourable sense. A letter from Lieutenant Grandy, from the Western Expedition, was then read. In this communication the writer, in giving an account of the progress of the expedition, stated that the men were all well, and that the climate was deliciously cool.

THERE will be an Election to Five Scholarships at Jesus College, Oxford, on Tuesday, October 14. The annual value of the Scholarships is 80*l.*, and they are tenable to the close of the twentieth term from the Scholar's matriculation. Candidates must not on the day of election be full twenty-four years old. One of these Scholarships is an Open Scholarship. It will be given according to proficiency in Physical Science, combined with the Classical attainments required by the University. The Examination for this will commence on Tuesday, October 7, and it will be held at Magdalen College in company with that for a Magdalen Demyship and a Merton Post-Mastership. Papers will be set in Chemistry, Physics, and Biology; and an opportunity will be given of showing a knowledge of practical work in Chemistry and Biology. Candidates for this Scholarship, if not otherwise admitted to the Examination, are requested to call on the Principal of Jesus College, on Monday, Oct. 6; and if so admitted, to call upon him on any day in the same week, and to bring with them certificates of age and of past good conduct.

THERE will be an election to a Fellowship in Natural Science at Magdalen College, Oxford, in October next, the holder of which will not be required to take Holy Orders. The examination will be held in common with Merton College, preference being given to proficiency in Biology, the College reserving to themselves the power of taking candidates in any other branch of Natural Science if it shall seem expedient to do so. Candidates must have passed all the examinations required by the University of Oxford or University of Cambridge for the degree of Bachelor of Arts, and must not be in possession of any Ecclesiastical Benefice, or of any Property, Government Pension, or office tenable for life, or during good behaviour (not being an Academical office within the University of Oxford), the clear annual value of which shall exceed 230*l*. They must also produce testimonials of their fitness to become Fellows of the College as a place of religion, learning, and education, and these must be sent to the President on or before Monday, Sept. 29. Candidates are required to call on the President on Monday, Oct. 6, between the hours of 3 and 5, or 8 and 9 P.M. The examination will commence the following day.

DR. JAMES BOTTOMLEY, B.A., D.Sc., F.C.S., has been appointed to the Science Mastership of the Taunton College School. The liberality of two or three munificent friends has enabled the headmaster to place the science teaching on a new and enlarged footing. Science has been taught in the school since 1865 with imperfect instruments, accommodation, and teaching power, yet with sufficient thoroughness to pass many pupils in the London Matriculations and in the scientific portion of the Oxford Local Examinations. The apparatus will now be largely increased, a temporary but efficient laboratory is about to be erected, and a science master of the highest reputation has been secured.

THE fine specimen of the Octopus brought to the Brighton Aquarium from the French Coast in April last and suspected at the time by Mr. Saville Kent to be a female, has just verified this anticipation by depositing numerous eggs. The position selected by the creature for their lodgment is most opportune, the several clusters being attached to the rockwork, close to one another, within a few inches of the front glass of its tank; thus affording every facility for their observation to the general public, and enabling the officers on the Naturalist's Staff to watch their progress towards maturity from day to day. The eggs were deposited on Thursday last, the 19th inst., since which time the parent has vigilantly guarded them, usually encircling and partly concealing the whole within a coil of one or more of her snake-like arms, and vigorously repelling the near approach of any of her comrades in the same tank. Like those of the Argonaut or Paper Nautilus, the eggs of the Octopus are of small size compared with the ova of other Cephalopoda, the individuals being no more than one-eighth of an inch in length, of oval form, and are crowded round a central flexible stalk two or three inches long. A dozen or more of these compound clusters, each including over a hundred eggs, represent the number already deposited by the female Octopus in the Brighton tanks. The mate of the interesting parent is a fine fellow brought from the Cornish Coast last February. On the arrival of his fair companion he immediately vacated his oyster grotto in her favour and for many subsequent days lavished upon her the most assiduous attention.

MR. LIVINGSTONE STONE, the Assistant Commissioner on the part of the United States, has been engaged for some time past in collecting fresh-water fishes of various species to be transported to California, for the purpose of introducing them into the rivers and ponds of that State. For this purpose he had sent to him a car of the Central Pacific Railway, which he has had fitted up properly for this object. At one end of the car is a plank pond, lined with zinc and holding four tons of water, over which are berths for Mr. Stone and his assistants. The rest of the car is occupied with smaller tanks, and a reserve of sea and fresh water, household and commissary supplies, &c. Among the species that Mr. Stone carries with him, in the form of partly hatched eggs or young, are shad, cat-fish, yellow perch, wall-eyed or glass-eyed perch, eels, lobsters, and the like, and there is every reason to believe he will succeed in transferring his freight without material loss. If he accomplishes his object of placing these fish in the California waters, there is every reason to expect them to constitute before many years an important addition to the food resources of the State.

MR. BENTHAM'S Anniversary Address to the Linnean Society, just printed at the request of the Fellows, deals chiefly with the progress of physiological botany during the past year. He refers especially to Strasburger's investigations of the floral structure of Coniferæ and Gnetaceæ, and to the genealogical theory by which that botanist makes the Conifers the parent race from which the Gnetaceæ have directly descended, these again having engendered the higher Dicotyledons. This theory Mr. Bentham considers to rest on very slender grounds, preferring the hypothesis that the Gnetaceæ have remained the least modified from the common stock, the Coniferæ having undergone a greater progressive change in one direction, the total separation of the sexes, the Dicotyledons a greater advance in another direction, the increasing complexity of the floral development. Haeckel's conjectural pedigree of the Calcisponges is also criticised.

THE "session extraordinaire" of the Botanical Society of France will be held this year at Brussels under the auspices of the Royal Botanical Society of Belgium. The session will commence by a meeting at the Botanic Gardens, Brussels, on July 9, at 9 A.M. Excursions will be made to the botanical establishments at Brussels, Ghent, Liége, Antwerp, &c.; as well as to the grotto of Haux, the marshes of Hasselt, &c. English botanists are especially invited to take part in this meeting. The districts to be visited are stated to be of unusual interest from a botanical point of view.

THE subscriptions to the Sedgwick memorial give promise that a handsome museum will be erected to his memory. The amount already promised is very considerable. The Chancellor of the University, the Duke of Devonshire, heads the list with a donation of 1,000*l*. The High Steward, the Earl Powis, contributes 200*l*.; the Prince of Wales, 100 guineas; the Vice-Chancellor., Dr. Cookson, the two representatives in Parliament, the Right Hon. S. H. Walpole and Mr. Beresford Hope, as well as a large number of other gentlemen give 100*l*. each. The Earl of Derby has promised 200*l*., Prof. Selwyn, 500*l*.; the Master of Trinity College, 200*l*.; Prof. Lightfoot, 200*l*.

THE Royal Horticultural Society's Show at Bath was opened on Tuesday, and continues till Saturday.

THE official report of the Secretary of the U. S. Navy, respecting the Arctic exploring ship *Polaris*, dispels the suspicions respecting the manner of Captain Hall's death, and shows that the separation of the crew was accidental, but does not account for the failure of the *Polaris* to rescue the men on the ice. Important scientific results have been obtained. The supposed open Polar Sea proves to be a sound opening into Kennedy Channel, with an inlet on the east, probably marking the northern shore of Greenland. The *Tigress*, which has been purchased by the Navy department for the relief expedition, will start early in July.

THE Council appointed at the Conference of the Trades Guild of Learning, recently held at the Society of Arts, met on Saturday last. Amongst other business transacted it was resolved that in addition to various other eminent men, the following, as representatives of literature, science, and art, be invited to become vice-presidents of the guild:—Prof. Huxley, Sir Francis Grant, Mr. Alfred Tennyson, Dr. W. B. Carpenter, Prof. Tyndall, Sir Antonio Brady, Lord Lyttelton, Mr. Thomas Hughes, M.P., Mr. J. A. Froude, and Sir Sterndale Bennett. It was further resolved that the annual subscription for ordinary members be one shilling or upwards, and for associate members one guinea or upwards; that application should be made for donations to meet the preliminary expenses, and to furnish an income until the society is self-supporting; and that a prospectus of the objects and plans of the society should be issued as soon as possible.

COMMODORE SELFRIDGE has returned to the Navy Department at Washington, bringing with him the materials for presenting a detailed report of his exploration upon the Isthmus of Darien during the past winter in reference to the construction of an inter-oceanic ship-canal. The result of his inquiries has been much more favourable than was anticipated, and it is now estimated that only twenty-eight miles of canal need be constructed, the remainder of the distance consisting of the perfectly navigable waters of the Atrato, Doguado, and Napipi rivers. A tunnel will still be necessary, as estimated on a previous exploration, but this will only require to be three miles in length, instead of five, and it is estimated that the entire distance can be completed at a cost of less than 70,000,000 dols. Twenty-two miles of the canal are over an almost level plain, and only nine locks in all will be needed.

WE have just received the first number of the Bulletin, or Proceedings of the Society of Natural History of Buffalo, New York. Four similar numbers are to be issued each year, with a few plates. The number before us is solely occupied by the work of Mr. Aug. R. Grote, who contributes four papers describing new North American Moths, and giving catalogues of the Sphingidae and Zygaenidae of North America, followed by conclusions drawn from a study of the genera Hypena, and Herminia.

SINCE the diffraction spectrum differs from a prismatic spectrum of the same length in having the less refrangible rays more widely dispersed, it some time ago suggested itself to Prof. C. A. Young that a so-called *gitter-platte* or "grating" of fine lines might advantageously replace the prisms in spectroscopes designed for the observation of the solar prominences through the C line. Having recently obtained one of the beautiful gratings ruled upon speculum metal, having a ruled surface of something more than a square inch, the lines being spaced at intervals of $\frac{1}{6481}$ of an inch, he combined this with the collimator and telescope of a common chemical spectroscope, thus getting an instrument furnishing a spectrum of the first order, in which the D lines are about twice as widely separated as by the flint glass prism of 60° belonging with the original instrument. In the neighbourhood of C the dispersion is nearly the same as would be given by four prisms. The spectra of the higher orders are generally not so well seen on account of their overlapping each other, but fortunately with one particular adjustment of the angle between the collimator and telescope, the C line in the spectrum of the third order can be made to fall in the vacant space between the spectra of the second and fourth orders. On applying the new instrument to the equatorial, Prof. Young found that in the first order spectrum he could easily see the bright chromosphere lines C, D_3, and F; he could also, though with great difficulty, make out $H\gamma$, (2796K). On opening the slit the outline of the chromosphere and the forms of the prominences were well seen, both in the spectra of the first and third order. The grating is much lighter and easier to manage than a train of prisms, and if similar ruled plates can be furnished by the opticians at reasonable prices and of satisfactory quality, it would seem that for observations upon the chromosphere and prominences they might well to some extent supersede prisms.

THE Eleventh Annual Report of the Free Libraries Committee of Birmingham is very carefully drawn up. It contains some valuable analytical tables showing the average numbers of those who daily take advantage of the library, the ages of the readers, their occupations, along with the number of volumes issued to readers of each occupation, and tables showing the books most in demand. From the latter item we are glad to see that science in its various departments comes in for a very fair share of attention. In April 1872 the Reference Library and the Art Gallery were thrown open to the public on Sunday afternoons, and to judge from the statistics, the privilege has been taken considerable advantage of, especially by those who have least time during the week for mental improvement.

IN the last number of the *Journal of the Statistical Society* is an interesting paper by Mr. F. Galton, F.R.S., on the Relative Supplies from Town and Country Families to the population of future generations.* Mr. Galton took for the purpose of comparison, from the census returns, 1,000 families belonging to Coventry, in which there are various industries, and where the population is not increasing, and 1,000 families from small agricultural parishes in Warwickshire. After careful comparison and calculation, based on ascertained data, Mr. Galton concludes that the rate of supply in towns to the next adult generation is only 77 per cent., or, say, three-quarters of that in the country. In two generations the proportion falls to 59 per cent., that is, the adult grandchildren of artisan townsfolk are little more than half as numerous as those of labouring people who live in healthy country districts.

THE Reports and Proceedings for the year 1872–3 of the Miners' Association of Cornwall and Devon, contain some good papers, mostly of a practical nature, in connection with mining.

WE have received the Monthly Notices of the papers and proceedings of the Royal Society of Tasmania for 1870, 1871, and the half of 1872. A great part of them are occupied with valuable meteorological observations and statistics, and from the reports of the society's meetings and the numerous papers printed *in extenso* on subjects connected with all departments of science, we judge the society to be in a healthy condition. As might naturally be expected, many of the papers are devoted to the practical aspects of science, to pisciculture, arboriculture, agriculture, the rearing of sheep, &c.

WE would recommend to anyone visiting Derbyshire, especially the district around the Peak, Mr. Bates's little "Handbook to Castleton and its Neighbourhood," containing very full and well-compacted information on all the places of interest around. There is a useful section on the geology, mineralogy, and botany of the district, and we believe that Mr. John Tym, of Castleton, the publisher of the book, well known as a geologist, will willingly give anyone who calls at his shop, information on the natural history of the district.

WE would recommend to all Londoners who are at a loss how to spend an occasional holiday to procure the summer edition of Mr. Henry Walker's "Half-Holiday Guide," which is wonderfully cheap considering the quantity of matter it contains. It would take a few summers of half-holidays to exhaust all the charming resorts around London he describes. The book also contains much useful information for the botanist, geologist, ornithologist, entomologist, and microscopist, as well as with regard to various sports. Mr. Walker should, however, cease to quote so much irrelevant verse.

THE following additions have been made to the Brighton Aquarium during the past week:—Two Puffins (*Fratercula arctica*); small Crocodile (*Crocodilus sp.*) from Sumatra, presented by Captain Murray; Bass (*Labrax lupus*); Black Bream (*Cantharus lineatus*); Streaked Gurnards (*Trigla lineata*); Mackerel (*Scomber scomber*); Lumpfish (*Cyclopterus lumpus*); Grey Mullet (*Mugil capito*); Ballan Wrasse (*Labrus maculatus*); Flounders (*Pleuronectes flesus*), fresh-water variety, presented by F. J. Evans, Esq.; Herring (*Clupea harengus*); Conger Eels (*Conger vulgaris*); John Dorée (*Zeus faber*); Sea Horses (*Hippocampus ramulosus*) from the Mediterranean; Octopus (*Octopus vulgaris*); Oysters (*Ostrea edulis*); Zoophytes (*Actinoloba dianthus*), (*Sagartia nivea*), (*S. miniata*), (*Alcyonium digitatum*), (*Tubularia indivisa*).

THE additions to the Zoological Society's Gardens during the past week include a Dormouse Phalanger (*Dromicia nana*) from Tasmania, presented by Mast. W. F. Stratford; a Coati, brown variety (*Nasua nasica*) from S. America, presented by Mr. G. P. Crawford; a Lion (*Felis leo*) from Africa, presented by the Hon. M. E. G. Finch Hatton; a Rhesus Monkey (*Macacus erythreus*) from India, presented by Mr. J. C. Freeman; a Tasmanian Rat Kangaroo (*Hypsiprymnus cuniculus*), presented by Mr. J. Shelton; a Garnet's Galago (*Galago garnetti*) from E. Africa, presented by Mr. Bartle Frere; two horned Lizards (*Phrynosoma cornutum*) from Texas, presented by Mr. W. L. Booker; a Clifford's Snake (*Zamenis cliffordii*) from Cairo, presented by Mrs. E. Liveing; a black Stork (*Ciconia nigra*), two white Storks (*C. alba*), and a Spoon-bill (*Platalea leucorodia*), purchased; a red Kangaroo (*Macropus rufus*), and a Fallow Deer (*Dama vulgaris*), born in the Gardens.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 15.—"On the Heating of a Disc by Rapid Rotation *in vacuo*." By Prof. Balfour Stewart, M.A., F.R.S., and Prof. P. G. Tait, M.A.

In two previous communications to this Society, we gave an account of some experiments which we had made upon the heating of a disc through rotation *in vacuo*. In these experiments the increase of radiation of the heated disc was observed by means of a delicate thermopile and galvanometer. Three aluminium discs of various thicknesses and one ebonite disc were used, and the results derived from the experiments were as follows:—

(1) The heating effect observed appeared to be independent of the density, and of the chemical constitution of the residual air and vapour surrounding the discs.

(2) The quantity of heat developed under similar circumstances of rotation in three aluminium discs ·05, ·0375, ·025 of an inch in thickness respectively appeared to be the same, inasmuch as the relative thermometric effect for these discs varied inversely as their thickness.

(3) Besides the heating effect alluded to in (1) and (2), there was found to be, when the vacuum had been recently made, a strictly temporary effect, sometimes in the direction of heat, sometimes in that of cold, owing probably to the condensation or evaporation of small quantities of aqueous vapour; but this effect was only noticeable during rotation, disappearing the moment the motion was stopped.

In June 1871 the experiments were resumed. In the mean time the apparatus had been fitted with an arrangement working through a barometer-tube, by means of which, instead of trusting to radiation, the disc itself might, after rotation, be tapped by means of the pile, which could be brought up to it and then withdrawn. By this means a much larger effect might be obtained, and it became possible, by varying the adjustment, to find according to what law the heat-effect varies with the distance from the centre.

These experiments were conducted in the following manner: The disc was first of all tapped before rotation several times; at each tapping the momentary swing of the needle was recorded, and the mean of the readings was regarded as indicating the state of the disc with respect to heat.

The disc was next tapped after rotation, and the difference between the readings before and after was taken as indicating the change in the state of the disc produced by rotation.

The results derived by tapping an ebonite disc were found to be very different from the radiation-results, inasmuch as in the former the effect of the pressure and quality of the residual air is very apparent, while in the radiation-results it is hardly perceptible. A probable explanation of this will be given afterwards, but in the mean time, in view of these results, it has been thought expedient to discuss them quite independently and by themselves, with the view of ascertaining whether they can best be explained by a gas-effect alone, or whether they likewise indicate a residual effect independent of gas.

With this object calling A B the results at $\frac{1}{10}$ and $\frac{2}{10}$ let us take $\frac{(A)+(B)}{2}$ as representing the *whole effect* at a pressure of $\frac{3}{20}$ in. due to whatever cause or causes. We thus obtain

	Dry hydrogen	Dry air.	Dry carbonic acid
Whole effect at $\frac{3}{20}$ . .	9·5	25·0	24·0

Again, let us suppose that (A) – (B) denotes the *gas effect* for $\frac{1}{10}$ in., and we obtain

	Dry hydrogen	Dry air	Dry carbonic acid.
Gas-effect at $\frac{1}{10}$ . . .	4·0	20·0	18·0

Finally, let us regard as *unknown residual effect* the difference between the *whole effect* and the *gas-effect*, and we obtain

	Dry hydrogen.	Dry air.	Dry carbonic acid
Residual effect . . .	5·5	5·0	6·0

Similar experiments with the same galvanometer were made with a disc of cartridge-paper, of which the pores were filled with solid paraffin.

Treating these results in the same manner as those of the ebonite disc, we obtain:—

	Dry hydrogen.	Dry air.	Dry carbonic acid.
Whole effect ($\frac{3}{20}$) . .	25·0	45·0	43·5
Gas-effect ($\frac{1}{10}$) . . .	4·0	20·0	23·0
Residual effect . . .	21·0	25·0	20·5

Now, if we suppose that there is only one effect due to gas, it follows:—

(α) That the proportion between the effects due to the various gases experimented on (and all of the same pressure) is nevertheless different for the two discs.

(β) That the proportion (for the same disc) between the effects due to the various gases experimented on is different according to the pressure.

If, however, we suppose that there are two effects, one of which is independent of the residual gas, we find:—

(α) That, as regards the *gas-effect*, the proportion between that due to the various gases is nearly the same for both discs. Thus in the ebonite disc we have 4, 20, 18, while in the paper disc we have 4, 20, 23 as representing the gas-effect for the various gases.

(β) That the *residual effect* in either disc is nearly the same for the various gases. Thus in the ebonite disc we have 5·5, 5·0, 6·0, while in the paper disc we have 21·0, 25·0, 20·5 as representing the residual effect for the various gases.

The results are thus much more simple on the hypothesis of two effects, one of these being independent of the residual gas, than on the hypothesis of only one effect.

It was next endeavoured to ascertain whether these two effects were differently influenced by a blind, and it was found that the proportion between the two effects is greatly altered by the blind, so that while the hydrogen effect is not much stopped, the other is diminished very considerably; it was therefore concluded that the residual effect is not much altered by a chamois leather blind.

It was suggested to us by Prof. Helmholtz that it would be desirable to ascertain whether any difference was produced in the results by loading the disc on one side; for if these results be due to vibration, it might be supposed that they would be affected by this means.

It has been seen that the residual effect obtained from a disc covered with chamois leather is approximately the same as that from an uncovered disc; this would appear to us to be against the vibration hypothesis.

In an experiment made the disc was covered with a chamois leather blind with a segment cut out.

From a mean of two sets of experiments we may conclude that this arrangement does not much influence the results.

The disc was next treated in the following manner:—

It was covered with a chamois leather blind tied into holes drilled in the disc, and having two pieces of different shape cut out. The experiments gave (for an atmosphere of $\frac{1}{16}$ dry hydrogen) tapping in A 54, tapping in B 56, while for an uncovered disc they gave 53 as the heat-result. All these experiments apparently combine to prove that the result is not due to vibration.

Our next experiments were made with the view of testing whether or not the two effects, the residual and the gas-effect, were resident in the same particles of the disc; and for this purpose the experiments made immediately after rotation were compared with those made one minute afterwards.

The experiments available for this purpose are so numerous that they can bear splitting into two portions, in each of which the same result is seen.

Thus we have for an atmosphere of $\frac{1}{16}$ dry hydrogen, and as the mean of 30 individual comparisons.

Effect at first : Effect one minute after : : 1·30 : 1;

also, as the mean of 22 individual comparisons, we obtain the proportion of 1·19 : 1, while as the mean of the whole we obtain 1·25 : 1.

Treating in a similar manner the observations made with an atmosphere of $\frac{1}{16}$ hyd. + $\frac{1}{16}$ air, we obtain

As the mean of 25 comparisons .... 1·47 : 1.
As the mean of 21 comparisons .... 1·41 : 1,

while as the mean of the whole we obtain 1·44 : 1.

We therefore conclude that the residual effect is less diminished during the interval of one minute than the gas-effect.

We next made experiments with two aluminium discs ·05 and ·025 of an inch in thickness respectively. These discs were covered on both sides with a coating of lampblack applied by negative photographic varnish.

From these experiments it was concluded that there are two effects which are differently distributed over the particles of the disc.

It also appeared that the effect for $\frac{1}{16}$ hyd. which may be supposed to represent the residual effect, and that for $\frac{1}{16}$ hyd. + $\frac{1}{16}$ air, which may be supposed to represent the gas-effect, are both diminished in very nearly the same proportion, namely 100 : 77, by a transference of the pile to a position nearer the centre of the disc. And it was furthermore concluded from the experiments that in an aluminium disc covered with varnish, as well as in a disc of ebonite, we may imagine the residual effect to be more deeply seated than the gas-effect.

We venture on the following as what appears to us to be the most probable explanation of the whole body of experiments, including those with radiation.

(1) There is a temporary heat or cold effect which may be supposed to arise in particles very slightly attached to the disc; this is radiated off chiefly during rotation, and does not probably greatly affect the disc afterwards.

(2) There is a surface gas-effect, which in an aluminium and even in an ebonite disc is conducted into the interior as it arises, so that it does not greatly radiate during rotation of the disc. In a paper disc, however, which is formed of a badly conducting material loosely put together, part of the effect does escape as radiation during rotation.

(3) There is a residual effect, which is more deeply seated than the gas-effect. And inasmuch as radiation takes place from a perceptible depth, this effect is much more influential than the gas-effect in increasing radiation after rotation. In the case of a paper disc, this deeply seated effect will be less diminished by radiation during rotation than the gas-effect, and therefore after rotation in such a disc we might expect the gas-effect to be peculiarly small.

In the course of these experiments we have endeavoured to prove that this residual effect is not caused by vibration. The radiation-experiments with aluminium discs of three different thicknesses went, on the other hand, to show that it was of the nature of a surface-effect. This is confirmed by the results derived from tapping; for, in the first place, the experiments with aluminium discs show that the two effects (the residual and the gas-effect) are probably distributed in the same proportion, going from the centre to the circumference of the disc. Again, taking the two discs of thickness ·05 and ·025 of an inch, we obtain the following results:—

	Effect for $\frac{1}{16}$ hyd.	Effect for $\frac{1}{16}$ hyd. + $\frac{1}{16}$ air.
Thin disc ...	48 (22 observations).	228 (10 observations).
Thick disc...	29 (20 observations).	103 (10 observations).

Now, allowing for errors of experiment, we see that the residual, as well as the gas-effect, is reduced to about one-half for the thick disc.

Again, an experiment of a similar nature gave the effect for $\frac{1}{16}$ hyd. in an ebonite disc of $\frac{1}{16}$ in. in thickness = 33 against a result = 55 for the thin ebonite disc. Unfortunately it was omitted to make a comparison with these two discs for the gas-effect; nevertheless these results are all in favour of the residual effect being a surface-effect.

Our conclusion from the evidence before us is, that the residual effect is a surface-effect more deeply seated than the gas-effect, but distributed outwards from the centre to the circumference, very much in the same manner as the gas-effect. The residual effect likewise appears able to penetrate a chamois leather blind without any perceptible diminution. We regard these conclusions as preliminary, and shall endeavour in our future experiments to procure additional evidence of these properties of the residual effect, as well as to obtain new facts regarding it. In the meantime, as the subject is one of interest, and has been already too long delayed, we have not hesitated to bring these results before the notice of the Royal Society.

Geological Society, June 11.—Prof. Ramsay, F.R.S., vice-president, in the chair.—The following communications were read:—"On the nature and probable origin of the superficial deposits in the valleys and deserts of Central Persia," by W. T. Blanford. The general results may be summed up as follows:—Persia has undergone a gradual change from a moister to a drier climate simultaneously with the elevation of portions of its surface, resulting first in the conversion of old river-valleys into enclosed basins containing large lakes, probably brackish or salt. Then, as the rainfall diminished, the lakes gradually dried up, leaving desert plains. The amount of subaerial disintegration among the rocks of the high ground he considered to be in excess of the force available for its removal, the water which now falls only sufficing to wash the loosened materials from the steeper slopes into the valleys, and hence the valleys in the upper parts are gradually being filled up with coarse gravel-like detritus, just as their lower portions have been already hidden beneath lake-deposits.—"On *Caryophyllia Bredai* (Milne-Edwards and Haime) from the Red Crag of Woodbridge." by Prof. P. Martin Duncan, F.R.S. The author recorded the occurrence in the Red Crag of the Woodbridge district of a variety of *Caryophyllia Bredai* (Milne-Edwards and Haime).—"On the Cephalopoda-bed and the Oolite Sands of Dorset and part of Somerset," by James Buckman, F.L.S. From an investigation of the Cephalopoda-bed in quarries at Bradford Abbas in Dorsetshire, the author comes to the conclusion that it is quite distinct from the Cephalopoda-bed of Gloucestershire, and that it is the representative of the Rubbly Oolite at the top of Leckhampton Hill and Cold Comfort, and of the Gryphite and *Trigonia*-beds of the neighbourhood of Cheltenham. The Gloucestershire Cephalopoda-bed he regards as situated close to the bottom of the Inferior Oolite series; and this is also the position to which he refers the sandy beds above mentioned.—"*Catarthrosaurus Walkeri* (Seeley), an Ichthyosaurian from the Cambridge Upper Greensand," by H. G. Seeley, F.L.S. In this paper the author described a small Ichthyosaurian femur, discovered by Mr. J. F. Walker in the Upper Greensand of Cambridge. He noticed the general characteristics of the femur in Ichthyosaurians, and pointed out, as the chief peculiarities of the bone that he was describing, the subovate form of its head, and the presence of large flattened lateral trochanters, which, if of equal dimensions on both sides of the bone, would have made its greatest transverse measurement greater than its length. Upon this bone he proposed to found new a genus, *Catarthrosaurus*.

Royal Astronomical Society, June 13.—Prof. Cayley, F.R.S., president, in the chair.—S. J. Lambert, of Newton Observatory, Auckland, was elected a Fellow of the Society.—The Rev. J. Vale Mummery presented a large photographic portrait of Mrs. Somerville. He said that the Society had long been possessed of a portrait of Miss Caroline Herschel, and he was glad now to be the means of finding her so fitting a companion. Mrs. Somerville and Miss Herschel had been admitted as honorary members of the Society on the same evening in 1834. They had long been separated, first by distance and then by death, and it was only fitting that their portraits should now be hung together on the walls of the Society.—Paper "On a stereographic projection of the transit of Venus in 1882," by R. A. Proctor. The author said that his paper was intended to show the desirableness of limiting the preparations for Halley's method to the transit of 1874. In the transit of 1882 the lines bounding the region where the whole transit can be seen will lie much closer. This is the natural effect of the transit lasting only six hours. Again, the southern pole where difference of duration is greatest, instead of lying within the region where the whole transit can be seen as in 1874, lies outside that region It should be remembered also that in searching for suitable places of observation a fringe of 10° wide, measuring along the lines where the beginning and end of the transit are seen at sunrise or sunset, must be thrown out of account. Taking this into account the transit of 1882 is seen to be very little suited for Halley's method. Maps were shown to illustrate the paper.—"On occultations of stars by the moon and eclipses of Jupiter's satellites," by the Rev. R. Main. This paper contained a very extensive table of observations of eclipses of Jupiter's satellites. Several such sets of observations have recently been received by the Society, and it was remarked that a paper on the subject read by the Astronomer Royal last year was beginning to bear good fruit.—"Note on the discovery of a new minor planet, No. 131," by Dr. Peters. This is the nineteenth planet discovered by Dr. Peters. Dr. Luther has also discovered 19. Thanks to the American telegraphic system, it has already been observed in England as well as at Leipzig and Marseilles.—"Note on the Mass of Jupiter," by W. T. Lynn. In 1866, he had had the honour of laying before the Society an account of a determination of this element by Prof. Kruger, of Helsingfors. That determination having been recently improved by the aid of subsequent observations, the result was communicated by the author to the *Astronomische Nachrichten* (No. 1,941.) Mr. Syme has, in this "Note," placed it in juxtaposition with the determinations by Airy, Bessel, Jacobs, and Moller. The agreement thus shown is very satisfactory, especially as the methods employed are different—Airy, Bessel, and Jacobs deducing Jupiter's mass from the motions of his satellites, Moller from those of Faye's Comet, and Kruger from those of the planet Kemis. This important element in the solar system may be considered as well established.—"Note on Dr. Oudeman's Photographs of the Solar Eclipse of Dec. 11, 1871," by Col. Tennant. He had received two paper copies of the photographs taken in Java. He could recognise almost every depression of outline as in the Indian photographs, but there was much less detail. He thought we might learn something from them as to photography. It was evident that the light was more intense than in the Indian photographs, but the exposure for a short time had not had the effect of producing the halation which was there visible. He was convinced that in future eclipses it will be better to use a reflector. —Mr. Ranyard remarked that the paper copies of the Dutch photographs which he had seen had been printed from enlargements on glass in which the moon had been stopped out with black paper, or some other material. On measuring, he had found that the body of the moon, as given in the photographs, was by no means circular; and Mr. Davis had pointed out to him that the irradiation under the prominences was perfectly sharp at the edges as it would be when printed through a paper nitch. It was therefore unfair to institute any comparisons as to the amount of the irradiation in these and in the other photographs. —"Note on the sympathetic influence of clocks," by Mr. William Ellis. He had been testing a number of clocks placed upon a wooden frame at the Royal Observatory. At first he found a sympathetic influence, but when the frame was considerably strengthened, so as to prevent vibration, they ceased to influence one another. He concluded that the popular notion as to the vibrations in the air produced by the swing of one pendulum having any susceptible influence on another swinging near to it was erroneous.—"On a recording micrometer," by Mr. W. H Christie. This contained a description of two rather elaborate instruments for recording the transits of stars by pricks on a long strip of paper. It is intended to make experiments as to the possible use of the instruments at Greenwich.—Proposal to determine the solar parallax by observations of the opposition of the planet Flora. M. Galle invited the assistance of English and Australian astronomers. He had prepared and submitted to the Society a long list of suitable comparison stars.

Linnean Society, June 19.—Mr. Bentham, president, in the chair.—Prof. P. M. Duncan read a paper on the Development of the Gynæcium and method of Fertilisation of the Ovule in *Primula vulgaris*. Prof. Duncan had carefully followed the account given by Duchartre of the mode of development of the ovule in Primulaceæ, from which he differed in many important points, believing that the French observer had been led into error by dissecting only a cultivated and therefore to some extent abnormal variety. In tracing the development of the floral organs Duchartre states that he first of all detected the calyx, then the stamens, and finally the pistil, the placenta being formed in the centre of the cavity of the pistil, and never connected with the ovarian wall. With this statement Payen agrees. Dr. Duncan's observations agreed with these as far as the formation of the calyx and stamens was concerned; but within the latter he found simply a mamillary process. At the next stage there was a very short style, solid and not perforated, the ovarian wall including the placenta on which were the rudimentary ovules; the ovarian wall does not grow up over the placenta, but is produced from it by a kind of differentiation; subsequently the style lengthens and the small stigma is produced. The ovules appear in a spiral series, and are recognised by their power of reflecting light; the summit of the placenta has never any connection with the style. The ovule consists of nothing but a single integument and an embryo-sac: there is no inner integument and no nucleus. The lower portion of the tissue of the style is absolutely impervious to the pollen-tubes; and if these could enter the ovary in this way, the micropyles are in such intimate contact with the placenta, that they could never be reached by the tubes from the cavity of the ovary. Dr. Duncan has detected the passage of the pollen-tubes actually through the tissue of the placenta itself, from which they again emerge to reach the micropyle of the ovule. In the discussion which followed, this view of the course of the pollen-tubes was confirmed by Dr. T. S. Cobbold.—Dr. Hooker read a paper by the Rev. C. New, on the sub-alpine vegetation of Kilma-njaro. This is the only tropical African alpine flora with which we are acquainted; the mountain being situated in Eastern Africa, 3° S. lat., rising to a height of 20,000 ft., or nearly 5,000 ft. above the snow-level. The flora is essentially that of the Cameroons. The flora may be divided into seven regions of successive heights; the 1st is the inhabited district, with plantains, maize, &c.; the 2nd region is jungle; the 3rd is a forest of gigantic trees covered with moss, the herbaceous vegetation being essentially European, with the dock and stinging-nettle, frosts almost every night; the 4th consists of green hills covered with clover; the 5th is heath; the 6th bare hills; the 7th, everlasting snow. Of the fifty species contained in the collection, twenty were from the zone immediately beneath the perpetual snow; nearly all were of South African genera, very few European, and no new species not already known from the Cameroons. The flora is therefore essentially South African

Meteorological Society, June 18—Dr. J. W. Tripe, president, in the chair.—The following papers were read:—On some results of temperature observations at Durham, by John J Plummer.—On the Meteorology of New Zealand, 1872, by C R. Marten.—On the Climate of Vancouver Island, by Robert H. Scott, F.R.S.—Meteorological Observations at Zi-Ka-Wei, near Shanghai, by Rev. A. M. Colombel, with note by Rev. S. J. Perry, F.R.A.S.—Notes on the connection between Colliery Explosions and Weather, by R. H. Scott, F.R.S., and William Galloway.—Distribution of Rainfall Maxima in Great Britain and Ireland between the years 1848 and 1872 inclusive, by W. R. Birt, F.R.A.S., and note on the heavy Rainfall of March 4 at Natal, by R. J. Mann, M.D., F.R.A.S. The ordinary meeting was then adjourned and the Annual General Meeting was held, and the Report of the Council read. The Report stated that the Council had much pleasure in congratulating the Society, at the close of the twenty-third session, upon the termination of a year which will bear favourable comparison with any that precedes it, whether regard be had to the character of the papers read, to the attendance at the periodic meetings, to the number of new Fellows elected, or to the activity and interest evinced in the general proceedings. It was stated that it had been found necessary to hold an extra meeting in May to enable all the papers which had been received to be presented before the Society; and the Council had the gratification to announce that it is in contemplation to hold eight monthly meetings next session, instead of six as has been the practice hitherto. The number of new Fellows added to the Society during the year had amounted to 35, the accession thus indicated being considerably larger than upon any years since 1864 Reference was made to the library, the financial affairs, the proposed alterations of the bye-laws, and the recent meteorological conference at Leipzig; and the Council concluded by stating that they had had under consideration that evening a letter from the Board of Trade with reference to sending a representative to the Meteorological Congress to be held at Vienna in September next. The President then delivered an Address in which he chiefly referred to the progress of the Society during the two years that he had occupied the presidential chair. The following gentlemen were elected officers and council for the ensuing year:—President—Dr. Robert James Mann, F.[illegible].S. Vice-presidents—Arthur Brewin, F.R.A.S., George Dines, Henry Storks Eaton, Lieut.-Col. Alexander Strange, F.R.S. Treasurer—Henry Perigal, F.R.A.S. Trustees—Sir Antonio Brady, F.G.S., Stephen William Silver, F.R.G.S. Secretaries—George James Symons, John W. Tripe, M.D. Foreign Secretary—Robert H. Scott, F.R.S. Council—Charles Brooke, F.R.S., Charles O. F. Cator, Rogers Field, C.E., Frederic Gaster, James Glaisher, F.R.S., John Knox Laughton, F.R.A.S., William Carpenter Nash, Thomas Sopwith, F.R.S., Rev. Fenwick W. Stow, M.A., Capt. Henry Toynbee, F.R.A.S., Charles Vincent Walker, F.R.S., E. O. Wildman Whitehouse, C.E.

BERLIN

German Chemical Society, June 9.—A. W. Hofmann, president, in the chair. A. Behr and Van Dorp report oxide of lead heated in iron tubes to be a good oxidising agent for organic vapours: $C_6H_4(CH_3)_2$ yielding $C_6H_4(CH)_2$, &c.—E. Salkowsky has found that taurine escapes digestion in the human body to a large extent. A small quantity of the following compound, however, passes into the urine; a crystallised acid of the empirical formula, $C_3H_8N_2SO_4$, forming quadratic plates, which are easily soluble, and giving well-crystallised salts with Ba, Ag, &c. With baryta water it yields taurine, carbonic acid and ammonia. The acid appears to be a substitution product of our hydrogen in taurine through carbaminic acid. Dr. Salkowsky took ·5 grammes of taurine for twelve days following without suffering any great inconvenience to his health.—T. Thomsen sent in the results of very numerous experiments on the heat absorbed or developed by dissolving various salts in water. The same *savant* attacks the calorimetric method employed by Berthelot, and disputes his conclusions as to the existence of a hydrate $HCl + 8H_2O$.—K. Heumann has found that copper in contact with sulfide of ammonium becomes covered with crystals of subsulfide Cu_2S, according to the reaction $2CuO + 2(NH_4)_2S = Cu_2S + 4NH_3 + 2H_2O + S$.—H. v. Gegenfeld reports on the action of hypochlorous acid $HClO$ on allylic chloride. The dichlorhydrine thus formed he considers as isomeric with that prepared from glycerine, while L. Henry obtained a body through the same reaction, which he considers as identical with ordinary dichlorhydrine.—L. Bischopinck has studied the amides and the nitrites of the three chloracetic acids, particularly with regard to their physical properties. The most prominent result is the following irregularity in the boiling points of the nitriles, namely:—

$CH_3.CN$ boils at 81—82°
$CH_2Cl.CN$,, 123—124°
$CHCl_2.CN$,, 113—113°
$CCl_3.CN$,, 83—84°

The foregoing remarks were accompanied by a note of M. L. Henry on the boiling-points of the cyanides of negative radicals. He points out that if in HCN, H is replaced by a negative element or radical, the boiling point sinks; thus HCN 26°, Cl.CN 15°, CN.CN —21°, adding other examples, and the attempt of an explanation of this exceptional phenomenon. The same chemist has continued his researches on propargylic alcohol C_3H_3OH. He has found its boiling point equal to 114°, and he has prepared the bromide, the iodide, the sulfocyanide, and the acetate belonging to it. In treating brominated allylic alcohol $C_3H_4Br.OH$ with potash, he obtains besides propargylic alcohol an ether $(C_3H_4Br)_2O$, and perhaps also propargylic ether, which has not as yet been obtained in the pure state.

PARIS

Academy of Sciences, June 16.—M. de Quatrefages, president, in the chair. The following papers were read:—On the combustion heat of formic acid, by M. Berthelot.—On the alloys used for gold coinage, by M. Eug. Pélegot. The author advocated the addition of zinc to the alloy, and at the same time the reduction of the gold to a very great amount. He mentions with favour alloys containing from 48 to 66 mill. zinc, 354 to 372 copper, and 580 to 581 gold.—A report on the papers on *Phylloxera*, by MM. Duclaux, Max. Cornu, and L. Faucon was presented.—On the complete movements of a ship oscillating in calm water, by MM. O. Duhil, de Benazé and P. Risbec. The authors gave an account of their experiments on the *Elorn*, a vessel of 100 tons displacement.—Photo-chemical researches on the use of gases as developers, and on the influence of physical conditions as regards sensitisation, by M. Mergot, was a paper on some of the chemical phenomena of photography.—On a scientific balloon ascent on the 26th April, 1873, by MM. Crocé-Spinelli, Jobert, Pénaud, Petard, and Sivel.—Announcement of the discovery of Planet 132 at Washington on the 14th June, by Prof. Henry.—Researches on electricity produced by mechanical actions, &c., by M. L. Joulin.—Researches on essence of ylan-gilan (*Unona odoratissima*), by M. H. Gal. The author has discovered benzoic acid in this essence, and believes that this is the first instance of this body being found in an essence, it having hitherto been found only in the balsams.—Contributions to the history of the histologic constitution of Molitg's glarin, by M. A. Bechamp. This paper related to the gelatinous body found in the sulphurous springs of the Pyrenees. The author finds that microscopic examination shows it to be a mass of microzymes imprisoned in a hyalin matrix. He has tried various experiments on its action as a ferment.—On the estimation of the total nitrogen in manures, by M. H. Pellet.—On the estimation of phosphoric acid in natural phosphates, super-phosphates, and manures, by M. H. Joulie.—On a process for the estimation of hæmoglobin in blood, by M. Quinquaud.—On the determination of the mechanical equivalent of food, by M. A. Sanson. The author pointed out the immense value to all employers of animal motive power, such as military authorities, &c., of the value of a method for ascertaining the value in work of the forage they use for their horses. He estimated the value of 1 kilo. of protein in a good average ration, as, in round numbers, 1,600,000 metre-kilograms.—Experimental researches on the influence of barometric changes on the phenomena of life, 11th note, by M. P. Bert.

DIARY

THURSDAY, JUNE 26.

SOCIETY OF ANTIQUARIES, at 8.30.

FRIDAY, JUNE 27.

QUEKETT CLUB, at 8.

SATURDAY, JUNE 28.

GEOLOGISTS' ASSOCIATION.—Excursion to Hatfield.

TUESDAY, JULY 1

SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8.30.—The Fall of Nineveh and the First Year of Nebuchadnezzar, King of Babylon: J. W. Bosanquet.

WEDNESDAY, JULY 2.

HORTICULTURAL SOCIETY.—Rose Show.

BOOKS RECEIVED

ENGLISH.—Field Pocket Book for the Auxiliary Forces: Colonel Sir Garnett Wolseley (Macmillan and Co.).—Education of Man (Charles Griffin & Co.)—Light Science for Leisure Hours 2nd Series: R. A. Proctor (Longmans & Co.).—The Old Faith and the New: Dr. F. Strauss (Asher & Co.)—The Scholar's Arithmetic: Lewis Hensley (C. P. S. Macmillan & Co.).

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THURSDAY, JULY 3, 1873

AN ORDER OF INTELLECTUAL MERIT

THE many obvious objections that may be urged against the well-meant proposal which Earl Stanhope brought forward in the House of Lords the other evening, for the creation of an Order of Merit to confer upon men who have deserved well of their country in Literature, Science, and Art, have already been pretty fully discussed both in the Upper House itself, and by the daily press. Happily "It is not now as it hath been of yore;" the classes for whose behalf it is sought to create a special Order of Merit, are getting to be regarded as less and less a peculiar people, both by themselves and by the public generally. To many it appears that the creation of any such order would be going in the face of the progressive tendencies of the age, and, we are confident, would not be in accordance with the desires of many of the men whom Lord Stanhope is sincerely anxious to honour. It is well-known, that over and over again have both academical and imperial honours been refused by men whom all acknowledge to have produced works that must be placed in the highest rank of intellectual products, and they spurn patronage.

The matter is not, however, all one way. The medals conferred by the Royal Society are really the decorations of an Order of Merit, election to which, however, lies in the hands of competent men; and much of the objection to the creation of an Order of Merit, such as that proposed by Lord Stanhope, would be done away with if Government were composed of men as competent to select the candidates for such an honour, as are the Fellows of the Royal Society. No doubt as civilisation based on Science advances, a Government competent to elect to such an order, as well as of performing efficiently all the other functions of a model Government, will be found at the head of this great country.

Speaking specially for men of Science, for men who devote to the advancement of scientific knowledge what leisure they have to spare from the necessary work of bread-winning, we must at once point out a tremendous difference between them and those who are generally classed with them.

The work of the artist and the author is always a marketable commodity—sometimes a very marketable one—while the investigation of new scientific truth is absolutely unremunerative; all the same, we may safely say that they seek no such recognition from the State as is indicated in Lord Stanhope's proposal.

From the tenor of all the speeches in the Upper House on Friday night, even those adverse to the creation of a special Order of Merit, we judge that the Government, as well as the House of Lords, believes that men who attain eminence in Science are as deserving of recognition by the State as men who have distinguished themselves in the army or navy, in diplomacy or politics. If this is so, then we are sure we speak the wishes of the great majority of scientific men when we say that they are willing to dispense with all hope of ever obtaining any honour from the State, if Government would do what is without doubt its duty,—enable those who have shown themselves competent to pursue original scientific research, to devote all their time to this object without care as to the means of living.

Most of those who, not being rich men, have done most to advance scientific knowledge have done so in moments snatched from the duties imposed upon them by the necessity of procuring the wherewithal to support life. Many who do the most valuable work in Science, which is generally *not* the work that is most volubly brought before fashionable audiences, are compelled, for bare life, to adopt some profession, and almost the only profession open to men who have qualified themselves for thorough scientific research, is the profession of teaching. This profession, it is well known, is one demanding, for the thorough performance of its duties, a very large expenditure of the highest energy as well as of time, so that men of Science of the class we are speaking of, who are compelled to adopt it, have but a small amount of energy and little time left to devote to that pursuit on which their heart is set, for which their whole training has qualified them, and in which they have shown themselves competent to attain the highest results;—results of the greatest and most wide-spread value both to our own country and to humanity generally. Is it not shameful then, nay does it not argue the greatest blindness on the part of Government to the best interests of the country, that these men should be compelled to expend the very best of their valuable and well-skilled energies in the drudgery of a profession for which they may by no means be peculiarly fitted, merely to keep the life in their bodies, while but a very moderate expenditure on the part of the State, would enable them to devote, without dread of coming to want, the whole of their power to the pursuit of that research, from which the country already has reaped the highest benefit? No man whose opinion is of any value, not even any member of Her Majesty's Government, we believe, doubts the eminently practical utility of scientific research, and the dependence of our country for its foremost place among the nations of the world, that it should have at its disposal the highest and latest results of such research. Instead then of devising new and empty honours wherewith to reward men who, amid a life passed in the worry and struggle for existence, have been able to push forward scientific knowledge a short stage, would it not be honouring the pioneers of Science far more, and at the same time making an investment which ere long would be repaid a hundredfold, if Government would only bestow upon these men the means wherewith to do thoroughly, and with all their might, the unspeakably valuable work which at present they can only do by snatches, or be compelled to give up when probably it is about to bring forth noble results? If Lord Stanhope and those in both houses of Parliament who have the wisdom to see wherein the true glory and highest good of their country consist, would only set themselves earnestly to devise some plan whereby scientific research could be pursued under the most favourable circumstances, they would delight the hearts of scientific men infinitely more than if they heaped upon their heads all the honours of all the Courts of Europe.

COOKERY AT SOUTH KENSINGTON

THE most successful department of the International Exhibition this year is undoubtedly that connected with Cookery. Twice a day is a lecture delivered on some practical department of cooking, and at the same time a demonstration is given by a well-trained group of female cooks, in a conveniently fitted-up kitchen open to the audience. These lectures are the great attraction of the Exhibition, and many persons anxious to gain admission are turned away for want of space to accommodate them. This shows, at any rate, on the part of the public, an appreciation of the subject and a desire to be instructed as far as possible.

At the same time it is to be lamented that the class of persons who most need instruction in cooking do not attend. The charges of sixpence and a shilling for entrance to hear these lectures and see the cooking demonstrations must exclude the class of people for whom such instruction is most needed. Although there is a wide-spread notion that people in England do not know how to cook at all, yet we question very much if the civilised world produces better dinners than are to be found daily on the tables of the wealthy classes of England. They need not to consult economy either in the cost of materials of food or its preparation. For them lectures on cooking are not needed, and even their cooks, who get from fifty to a hundred pounds a year, could hardly be instructed by Mr. Buckmaster and his bevy of cleanly cooks. If anything is wanted by the wealthier classes, it is a more scientific knowledge of the nature of food and the processes by which it is prepared for digestion. This they will not get at South Kensington. Mr. Buckmaster's lectures are not intended as a scientific exposition of the chemical or physical properties of substances used as diet, or of the way in which they affect the palate or act on the body. They consist simply of directions how to prepare dishes, and the cooks in the kitchen follow his directions. There is no doubt that to thousands of people this is of great service. No housekeeper, however low in the scale of society, but must be benefited by seeing prepared poor man's soup, omelettes, macaroni, and Australian meat, in Mr. Buckmaster's kitchen. At the same time they will learn only how to imitate the methods of cooking they have seen: they will learn no principles. They will hear nothing about the nature of the materials they see cooked, unless it is that hot water and heat act upon them to produce the results they see. They will see eggs made into an omelette in a frying-pan, but hear nothing with regard to the nature of eggs, their value as an article of diet, and other means of cooking them besides frying.

Another defect we observed in these lectures was the truly British defect of ignoring weights and measures. Mr. Buckmaster's lecture sounded very like the magnification of a receipt out of an ordinary cookery book. Take a piece of this, a pinch of that, and a handful, a sprig, a few teaspoonfuls, and so on for every ingredient used. We know this is the rule of the kitchen, and any attempt to introduce scales and weights would be flouted with contempt. It is the same with temperature; water is called "cold," "warm," and "hot," without the slightest allusion to temperature. Surely in lectures like these accuracy ought to be studied; and when things can be measured and weighed, so good an opportunity of teaching the importance of this should not be lost. It is because of the neglect of these matters in the kitchens of our wealthier classes that they seldom have put on their tables dishes two days alike. Nay, we know more: we tasted some macaroni made by a cook who had been to Mr. Buckmaster's lecture, which was no more like the macaroni made in his kitchen than his was like plum pudding. This arose entirely from the cook not measuring rightly the time of cooking the macaroni and the quantity of the flavouring ingredients.

Now we do not say it is possible to teach all the science of cookery in one lecture, but we do say that it is possible to speak accurately about the *weights* of the materials used, the degrees of *heat* to be employed in cooking, and the *time* that things require to cook.

We throw out these suggestions in the hope of seeing them acted upon. There is no doubt that it would be attended with some difficulty. There is the Italian cook, Mr. Buckmaster's *chef*, and the four young female cooks, all not only to be educated, but to be got into the frame of mind to submit. We see also that there is a Cookery Committee, who would, we suppose, have to be consulted; but these gentlemen would, we are sure, assist in introducing so desirable a system of instruction. Mr. De Rivaz is on the Committee, and he is well known for his book on cookery called "Round the Table," as also for his receipts in the *Queen* newspaper.

Whether there is any intention on the part of this Committee to extend the lectures, and give a course on cookery comprising the teaching of the elements of the sciences involved in the facts acted upon in the kitchen, we do not know, but this would be a worthy object and probably would succeed, as the public is evidently disposed to listen to the subject. It must, however, be done at once, and done in the International Exhibition. It cannot be done at South Kensington; the experiment has been tried there and failed. The country gentlemen in the House of Commons do not see their way to voting public money for the instruction of people in London. Whether done in London or the country, such courses of instruction would be a capital way of getting a little scientific knowledge into the heads of people edgeways, as it were.

But now we come to the question of opening the present lectures to the poor. These lectures were intended for their instruction and got up in their interest, but they are conspicuous by their absence at these lectures. The whole Exhibition is open to them for a shilling, and when they have screwed this sum out of their hard-earned wages, and paid for a crust of bread and cheese and half-a-pint of beer, they have nothing to spare for learning cookery. Yet we are quite sure the money would be well spent. The persons in the community who suffer most for want of economy in cookery are the very poor. They buy their food in the most expensive way, by buying it in small quantities, and when they have got it they know less than any class how to cook. They know nothing of the way of making, or of the economy of using soup. They hardly know the difference between warm, hot, and boiling water in cooking food. The fact is, we believe, that half the food of this class is really lost for the want of a knowledge of the proper means of cooking it. To such people these lectures should be open at the cost of a penny or twopence each lecture; and that each person of this class who attends the Exhibition should have the benefit of the lectures and demonstrations, these should be more frequent, and the theatre larger.

Something may be done before the Exhibition closes; but the cookery question is a permanent one. Cannot something be done to establish a School of Cookery, in which teaching such as is now going on at the International show can be carried on continuously? We can conceive such an institution possible, and even self-supporting. The whole of the middle and upper classes are interested in getting good cooks, and the school boards should be urged to allow their elder female pupils to attend the instructions given in such an institution. This would be an immense economy to all, for it would save a large portion of that waste which now goes on in every household, in teaching girls to become the sort of cooks they are.

If girls and women could be sent to such a school with a previous elementary knowledge of chemistry, physiology, and natural philosophy, they would derive more advantage than they would otherwise get from the necessarily short courses in such a school. In short it comes to this, that nearly all the details of practical life are dependent on facts which are comprehended in the various branches of scientific knowledge; and it is only as men and women are taught the nature of these facts that society can progress and man attain the highest possibilities of civilisation.

E. LANKESTER

COX'S POPULAR PSYCHOLOGY

What am I? A Popular Introduction to Mental Philosophy and Psychology. Vol. I. The Mechanism of Man. By Edward William Cox, Serjeant-at-law. (Longmans and Co.)

NO doubt many of the Serjeant's friends will read his popular introduction to the study of psychology, and think it very profound, and many of them, especially his lady friends, charmed with the vague denunciation of "Scientists" and materialists, the religious element, the quackery of science, and the scraps of poetry, will be able to tell him in all sincerity that they think it "a very nice book." But from those whose opinion is worth the paper it is written on, Mr. Cox has nothing to hope. The first sentence of the preface declares that "The study of psychology has not kept pace with the progress of the physical sciences." The truth of this statement must be painfully brought home to every real student of psychology, by the fact that a man possessing the intelligence and general culture of Mr. Cox could write such a book, and that educated people will be found to read it. We can agree with the author that there is at the present time room for a work presenting the leading truths of mental science in, if possible, a popular shape. But surely one qualification of the writer who would make such a book for the benefit of the "many persons who desire to obtain some knowledge of psychology, but who are deterred from its study by the ponderous volumes of abstruse argument . . . intelligible] only to the far advanced philosopher," must be, that he is himself up with the best science of the day, that he has made himself acquainted with "the ponderous volumes of abstruse argument." Unfortunately Mr. Cox does not appear to have taken this view of the matter. In setting himself to produce an "outline of the science of psychology written in plain language," he has, in plain language, attempted work for which he is no more qualified than an ordinary farm labourer is qualified to translate Homer into the vernacular of his native village.

Like books of its class the volume before us is rich in curious absurdities of presumption. For instance, scientific men are very severely taken to task for their lamentable want of scientific method; and there is no end to the tirade against materialists, metaphysicians, and mental philosophers. Who these greatest of sinners are, we cannot tell; for Mr. Cox prudently refrains from mentioning names. Nor are we told very precisely what are the particularly damnable heresies with which they have poisoned the public mind; indeed, it would appear that mindful of the good old proverb that one cannot touch tar without being defiled, Mr. Cox has been careful to keep his own mind at an angry distance from all their evil thinking. It may however amuse some of our readers to know what, according to Mr. Cox, is not materialism, while it will enable all to estimate the claim of the writer to rank as a psychologist. This is spiritualism: "Rightly, then to conceive of spirit, the first step is clearly to comprehend that it is not, and cannot be, *immaterial* —but only that it is composed of very refined matter—so refined that it is imperceptible to our bodily senses, which are adapted only to perceive certain forms of matter that affect ourselves." "The soul, therefore, being composed of molecules infinitely finer than the molecules of the body—as fine possibly as those of the comet, could, with the utmost ease, permeate the body, infusing itself among all the atoms of which the body is built, and thus occupy the whole frame;" and as a consequence "the shape of the soul must be the shape of the body." The soul here spoken of is not "the mind" nor the "life," but the proprietor of the body, the mind, and the life. As Mr. Cox's "inquiry is designed to be purely *scientific*," and is "addressed mainly to those who reject the authority of the theologian," we must give one specimen of the scientific arguments, in support of the existence of this entity, which scientists in their stupidity have hitherto failed to appreciate. Here is the best one:—"Does any sane man ever talk or write of his mind or his life as 'Me?' Does he not always say '*my* mind,' '*your* mind, '*my* life,' '*your* life,'—that is to say 'the mind, the life, —that belongs to *me*,' 'the life—the mind—that belongs to *you*.'" We hope the learned serjeant does better than this when he has a concrete mortal for a client. Without going farther a-field for an answer it must be sufficient to remind him that we not only say "*my* mind," and "*your* mind," but also "*my* soul," and "*your* soul," "*my*self," and "*your*self." Who, or what is the "*Me*," which according to the profound word-argument must exist as the proprietor of the *soul* and the *self*? This very refined existence has not yet got a name; but perhaps Mr. Cox, now his attention has been called to it, will be able to tell us in his second volume (which already promises to be much more interesting than the one before us) what sort of matter it is made of, its shape, and its dwelling place.

One word more; if men will write nonsense, they might at least endeavour to write original nonsense. It is sad to think that even young ladies should have to admire the old empty sentences in every new book. S.

OUR BOOK SHELF

The Darwinian Theory and the Law of the Migration of Organisms. By Moritz Wagner; translated by J. L. Laird. (Sandford.)

After the perusal of the preface to this pamphlet, the reader will expect to find that a serious objection to the Darwinian hypothesis has been detected, and that what is to follow will, by the introduction of a new law, clear up the assumed difficulty, and immortalise its discoverer. "The Law of the Migration of Organisms" of Prof. Wagner is that it is only by the isolated migration of single individuals from the station of their species, that natural selection could and can be effected, and that only by this means new varieties of plants and animals could arise in the past as well as in the present. This law is based on the considerations that the greater the change to which individuals are subjected on migration from their homes to some fresh locality, the greater will be their tendency to vary, and the less they have the opportunity of crossing with the parent stock, the more permanent will variations become. Most of the observations on which these arguments are founded have been arrived at from the author's researches on the distribution of insects and plants; and he has been led to propose it, because, as he says, "Darwin's work neither satisfactorily explains the external cause which gives the first impulse to increased individual variability, and consequently to natural selection; nor that condition which, in connection with a certain advantage in the struggle for life, renders the new characteristic indispensable."

To us it is not easy to see what direct bearing this law has on the theory of natural selection, for it seems to be nothing but one of the many deductions of Lamarck's theory of the origin of species. It is evident that on that very ingenious but equally inefficient hypothesis, the removal of individuals from their homes to some other locality in which the temperature and food are different, would cause them to vary; and that if the so modified forms are allowed again to mix with those which have not altered their position, the induced peculiarities will disappear. But, though by artificial selection an apparently similar result may be attained, yet in a wild state this is hardly the sequence of events which the evolution hypothesis supposes. According to it, the forces which come into play affect large numbers, and being generally comparable in degree and gradual in their action, those individuals which escape change in one direction are almost certain to undergo some equally considerable modification in another; consequently there will at no time be left any of the original unmodified stock for the varieties to intermix with, as required in the theory under consideration, at the same time that the effect of simple change of locality in producing new and well-marked varieties has not been conclusively proved.

From the study of the breeds of horses and cattle, Prof. M. Wagner is convinced that the invariable result of intercrossing is uniformity, and that only in connection with isolation is natural selection able to come into play. This, as do many other remarks throughout this pamphlet, shows clearly that its author does not really recognise the point of Mr. Darwin's great theory, and that whilst under the idea that he is attempting to modify it, he is really discussing another, but distantly related, and much less important problem. Such being the case, it is not surprising that the author of the theory of Natural Selection should differ from the German professor; with whom we also cannot agree in thinking that "perhaps that generous British naturalist, who is always open to conviction, after calmly weighing his reasons and data, may yet be induced to modify his opinions."

A Practical Manual of Chemical Analysis and Assaying, as applied to the Manufacture of Iron from its Ores, and to Cast Iron, Wrought Iron, and Steel as found in Commerce. By L. L. de Koninck, Dr. Sc., and E. Dietz. Edited, with notes, by Robert Mallet, F.R.S., F.G.S., M.I.C.E., &c. (London: Chapman and Hall, 1872.)

The above little work appeared at Liège in 1871, and as it was well arranged, succinct, and clear in its descriptions, Mr. Mallet considered it worthy of translation. The plan is similar to that of Fresenius's well-known quantitative analysis, the reagents being described first, then the apparatus and operations, and then the practical application to the special class of work to which the book is devoted. On the whole we cannot help thinking that too much space is given to matter with which every person ought to be thoroughly familiar before he attempts to make a practical application of his chemical knowledge. The supercession of the skilled chemist by the "tolerably intelligent man" mentioned by the editor in his preface is not, we think, a desirable reform. The editor's notes consist of some four and twenty pages of small print at the end of the book, and they are full of valuable suggestions. His remarks on the construction and arrangement of the laboratory of an ironwork are particularly worthy of attention. The book concludes with a table of atomic weights, one for the conversion of English weights and measures, with their metrical equivalents, and one of constants for calculating percentages of substances found. The book will no doubt prove very useful in its special field.

Verhandlungen der k.-k. Zoologisch-botanischen Gesellschaft in Wien. Jahrgang, 1872, 22^er^ Band. (Leipzig: Brockhaus.)

The annual volume of "Transactions of the Zoological and Botanical Society of Vienna" contains, as usual, a number of interesting and valuable articles. The papers are almost entirely systematic and descriptive:—On the flora of Poland (the longest paper in the volume); on birds from the shores of China and Japan; on the lichens of the Tyrol; on a collection of birds from Australia; on the bees of Germany; on North American Micro-Lepidoptera; on the oak-galls of Central Europe: and others of a similar character. Physiological or anatomical contributions occupy but a small portion of the volume, which is illustrated by seven plates.

The Art of Grafting and Budding. By Charles Baltet. (London: W. Robinson, 1873.)

The various modes of the reproduction of plants comprised under the designations grafting, budding, layering, &c., have been more scientifically studied and carried to greater perfection by gardeners in France than in England. Baltet's "L'Art de Greffer" is the text-book on this branch of horticulture, and of this little volume we have here a translation, although the omission to note this fact on the title-page might give unwary purchasers who have not dipped into the preface the impression that it is an original work. M. Baltet is so successful a fruit-grower, and his manual is so well and favourably known, that no apology was necessary in furnishing the English reader with a translation of it, which will be an indispensable companion to all engaged in horticulture. At the end of the volume is a useful list of the more commonly grown trees and shrubs, with instructions as to the best kind of stock on which to graft them, and the method to be pursued; though it is a pity that the translator did not take the trouble to re-arrange them in some order more intelligible to the English reader than that of the alphabetical sequence of the common French names.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Dr. Bastian's Turnip-Cheese Experiments

FROM Dr. Bastian's letter in last week's NATURE I learn that my last communication has afforded him satisfaction. The gratification which I feel at this expression of his approval is mixed with some surprise; for however confirmatory my experiments may be of his, so far as relates to the bare fact that boiling is insufficient to destroy the germinating power of the turnip-cheese liquid, they certainly do not tell in favour of the inference which he is understood to draw from that fact.

The experiments which Dr. Bastian was kind enough to show me last December were regarded by him as unequivocal instances of spontaneous generation. He will remember that at that time I stated to him, both orally and in writing, that the significance of the results in their relation to the doctrine of heterogenesis, appeared to me to be doubtful, and that I thought it probable that they would be interpreted by different persons in opposite senses, according to their preconceived opinions. I expressed myself in a similar manner at a discussion which took place on the subject last winter at the Royal Society. It was for the purpose of clearing up this doubt that I made the experiments recorded in my last communication. I did not expect to prove that the production of Bacteria in Dr. Bastian's experiments was *not* spontaneous, but merely to determine whether the fact afforded any support to the opposite conclusion.

Having first shown that living organisms increase and multiply in the liquid in question, when boiled at the ordinary temperature, under circumstances which absolutely preclude the introduction of living matter from without, I prove that under otherwise similar conditions this result is not obtained when the liquid is subjected to ebullition at a slightly higher temperature. I show further that the liquid even when heated to 102°·5 C. suffers no impairment of its power of supporting the life of Bacteria, for by inoculating it with a drop of ordinary distilled water it at once becomes pregnant. Hence I conclude, not that spontaneous generation is impossible, but that the particular experiment in question is not an instance of it, and that no argument founded on it in favour of the doctrine is of the slightest value.

It is unnecessary for me to occupy your space by at any length adverting to the side questions raised by Dr. Bastian in the other paragraphs of his letter.

In examining the liquids within a few days after heating rather than later, I followed his own method.

I made no attempt to determine the temperature of ebullition in flasks with capillary orifices, because I know of no method by which it could be done accurately. Besides, it was not required for my purpose.

I employed the word "chance" in its ordinary sense. In the sentence to which Dr. Bastian refers I explained that, although there may be a limit of temperature at which a liquid, before possessing the power of breeding Bacteria, is deprived of that power, experiments such as mine are insufficient to define that limit. As regards the turnip-cheese liquid it has been shown that between the temperatures of 100° and 102° C., the probability of pregnancy diminishes rapidly as the temperature increases. It is not as yet possible to say at what point the probability vanishes.

University College, June 30 J. BURDON SANDERSON

The Zodiacal Light

CONTRARY to Mr. Hall's experience of astronomical books (see NATURE, vol. viii p. 7), in neither Herschel's "Outlines of Astronomy," Humboldt's "Cosmos," nor Guillemin's "Heavens," can I find any hint of a permanent difference between the brightness of the zodiacal light east of the sun and west of it, though Arago's "Popular Astronomy" says that according to Cassini, "it is generally less lively and less extended in the morning than in the evening." But even if Cassini was correct, this is no positive proof of any difference between the two "branches" of the zodiacal light at the same time, seeing that he lived in the temperate zone, and probably did not observe it in both morning and evening at the same time of year. Mr. Hall's situation in Jamaica is favourable for investigating this point, and I should not wonder if he finds the fact different from what he supposes. But even the books that consider the zodiacal light to surround the sun in the shape of a lens, acknowledge that it may extend further one way than another, and further at one time than another. T. W. BACKHOUSE

Sunderland, June 7

AT about half-past one in the morning of June 5, the sky was clear, but the stars were not very brilliant, on account of the diffused light, and consequently the Eastern branch of the Zodiacal Light was very faint; as I was endeavouring to trace its course, a strong beam of light appeared so suddenly as to have quite a startling effect; it was not shot out like the rays of the Aurora Borealis, but gathered strength throughout its whole course, which lay through Aquarius, over the stars α and β Capricorni, through Sagittarius, across the Milky Way, and through Scorpio, passing to the N. of Antares; its visible length was therefore upwards of 100°, and as I was about to make accurate observations, it suddenly disappeared, having lasted somewhat less than one minute.

Its course was therefore nearly parallel to the Ecliptic, and about 6° to the N. of it; its breadth was from 3° to 4°; its brilliancy was equal to that of the brightest part of the Milky Way, through which it passed, and therefore allowed me to judge very accurately; and it had no colour.

Now Humboldt says in his "Cosmos,"* "I have occasionally been astonished, in the tropical climates of South America, to observe the variable intensity of the Zodiacal Light," and he considered the variation to be due to atmospheric changes, as I myself have hitherto done; but in the case above no ordinary atmospheric changes could have produced the effect observed.

It occurred June 4d. 18h. 40m. Greenwich mean time, and it would be very interesting to know whether the magnetic instruments were affected at any part of the earth.

Jamaica, June 1873 MAXWELL HALL

Meteorological Influence of Trap Rocks

THE thermometer in a mine, or coal-pit, rises, according to Herschel, 1° for every 90 feet of descent, or 58° per mile; and, according to Clerk Maxwell, the rate of increase in this country is 1° for every 50 feet of descent. These results are obtained in passing through a very small portion of the superficial crust of the earth; such, for example, as a part of the coal formation, which possesses a very low degree of conductivity. We can hardly, indeed, conceive a worse conductor than a crust consisting of alternating strata of freestone, shale, till, coal, limestone, &c. But these strata are very frequently perforated by comparatively homogeneous intrusions in the form of trap dykes, which not only possess greater conductivity, but which, from the analogy presented by volcanoes, very probably extend down to the molten matter subjacent to the external crust of the earth. Such trap dykes may be compared to an iron poker thrust through the superficial strata having its lower end in a state of fusion, and its upper end kept cool by radiation into the atmosphere. Through any continuous dyke, if this view be correct, there will therefore be a more rapid escape of heat; and when such igneous rocks occupy spaces of many square miles of the earth's surface, one would, at first sight, expect them to play a very important part in affecting the meteorological conditions of the district in which they are found. They might be expected, by the large amount of heat which they conducted freely to the earth's surface, to stimulate the growth of plants; and by the radiation of the liberated heat into the atmosphere, they ought to become—especially during night—the generators of storms, by causing a constant ascent of rarefied air. It is quite true, however, that the meteorological effects of such an agent must, as in the case of volcanoes, be observed by the far grander cycle of disturbances initiated by the solar heat; and that its agricultural efficiency may be, to a large extent, negatived by differences of chemical constitution, acidity, and exposure. Still, however, the influence is there, and ought, in one way or other, to make itself sensible.

Do any of your readers possess information bearing upon this question? Such, for example, as experiments on the conductivity of the different kinds of trap as compared with the stratified rocks, or observations of the temperature of the air, especially during night, above trap-rocks as compared with that of the air above surrounding districts of the coal measures, or statistics of the fertility and periods of fructification of crops under similar differences of conditions. Of course the great difficulty affecting the last point is the difference in the chemical constitution of the soils produced by the decomposition of trap and stratified rocks. THOMAS STEVENSON

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* Otté's transl., vol. i. p. 131.

Edinburgh, June 21

Winters and Summers

A FRIEND writes to me:—"From my observations of climate here (Belfast) I should say that I never saw a severe winter followed by a really fine summer. The severest winters I remember were those of 1854-5, and 1859-60. The summer of 1855 was very wet, and that of 1860 deplorable. The finest summers I remember were those of 1842, 1857, and 1868; in every case the preceding winter was very mild."

I would add to this, that the severe winters of 1865 and 1870 were not followed by remarkably fine summers. The harvest weather of 1866 was unusually bad.

Can any of your readers throw light on this subject from carefully kept registers? JOSEPH JOHN MURPHY

Old Forge, Dunmurry, June 6

Cyclones

MR. MAURY'S theory of Cyclones, as stated in NATURE of the 19th, is, in my opinion, true and valuable. I hope you will permit me to call the attention of your readers to my letter in NATURE, Vol. iv. p. 305, where it will appear that I had independently arrived at the conclusion stated by him, "that the origin of cyclones is found in the tendency of the south-east trade-winds to invade the north-east trades by sweeping over the equator into our hemispheres." Only the words "south-east" and "north-east" must exchange places, and "the opposite hemisphere," must be read, instead of "our hemisphere," if we are to apply the theory to the cyclones of the Southern Indian Ocean and of the Southern Pacific. On this latter subject, see Mr. Whitmee's letter in NATURE, vol vi. p. 121.

I wish, however, to call your attention to what I think an error in the diagram of the winds, which Mr. Maury reprints from Prof. Ferrel. It represents the winds at the surface of the earth in the Polar regions as blowing in nearly the same direction as the trade winds. This appears mechanically impossible, and I cannot think that Prof. Coffin's data are extensive enough as regards the Polar regions. As the late Capt. Maury remarks, the west winds of the higher and middle latitudes constitute "an everlasting cyclone on a great scale;" that is to say, a vast vortex whereof the pole is the centre. But it appears impossible that the direction of the motion of a vortex should be reversed at its centre. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, June 24

A Mirage in the Fens

As the phenomenon called Mirage is not very common in this country, though more frequent in the Fens, perhaps, than elsewhere, I presume that a description of one which was seen on Thursday, May 29, last, will be interesting to the readers of NATURE.

Driving from Wisbech towards Thorney on the morning named, I stopped at Guyhirne, and my friend, Mr. S. B. J. Skertchley, of H. M. Geological Survey, who accompanied me, mounted the parapet of the bridge of the March and Spalding Railway, to view the Fens from that elevation, and then called my attention to what appeared a beautiful lake spread out a few miles distant. The illusory waters were of a bluish grey colour, and being apparently raised from the level, presented the perspective of a Mere of considerable breadth. But this was not a dull expanse; there were variously formed indentations—islands dotted here and there, pollard willows inverted, and the reflection of tall poplars and elms on the glassy surface. The use of my field-glass only brought these features more distinctly to the eye. As we stood on the bridge, we were looking from W. by S. to W. Whittlesea Church was eight miles distant, and Thorney Abbey seven miles The mirage was stretched out from Eastern Fen over Prior's Fen to the west of Thorney, *i.e.* three or four miles. It was 11 o'clock. There was a fresh breeze from N.E.; the sky was not half obscured by cloud; the barometer stood high, being four degrees difference between the dry and wet bulb thermometers at 9 A.M. All these conditions were favourable to evaporation; there had been more than half an inch of rain the Monday previous. Mr. S. had witnessed a similar phenomenon from another point of view (see NATURE, vol. ii. p. 337) in 1870, when he saw it both E. and W. of his position, but on Thursday last there was not even a mist in any other part of the horizon. On both occasions the wind was N.E. It may be interesting to know whether these phenomena appear with a mild and moist S.W. or W. breeze.

Wisbech, June 5 SAML. H. MILLER

The Westerly Progress of Cities

REFERRING to Mr. W. F. Barrett's letter I would remark that there is a similar phrase, viz the westerly or north-westerly progress of nations, which is intimately connected with "the westerly progress of cities," and the former helps to explain the latter. As a rule the more westerly of two peoples inhabiting a country is there by compulsion, having been driven thither by the invader who, as a rule, makes the attack from the east. The remnants of the ancient Celtic race, inhabiting portions of the western shores and highlands of Spain, France, and the British Isles, are an evidence of this. We see the same process going on now in America: the aborigines being driven before the invader, to the west. There are insignificant exceptions, both in ancient and modern times, but they only prove the rule.

So much then for the westerly among the peoples of a land: they are in the west by violent compulsion. Among the inhabitants of a city the westerly are there also by compulsion—not a compulsion by violence, but by uncomfortable pressure; in which case it is the powerful or wealthy who retire before the weaker or poorer?

The very fact of the westerly progress of nations establishes the further fact that what becomes afterwards more or less the eastern part of the city is the older and that where the first habitations were erected. An exception would be such a case as a city built on a western coast without any adjacent country to the west. Here the wealthy in retiring before their less fortunate fellow-citizens must necessarily go more or less to the east. B. G. JENKINS

London, June 9

To the instances of "westing" adduced by Mr. W. F. Barrett as occurring in the large towns of the Old World it is desirable to add that a similar tendency prevails in the large towns of the New, excepting, of course, the cases in which physical barriers impede or prevent it.

It should be observed, also, that this westward current of progress in cities appears to be but the special manifestation of a principle much more general—the direction of great emigrations and of the advance of civilisation, apparently in pre-historic and certainly throughout historical times, having been uniformly towards the west. G. J. R.

How does the Cuckoo deposit her Eggs?

A FEW days ago while examining a reed bed in the fens of Lincolnshire, near Wainfleet, I found a Reed Warbler's nest, in which was deposited a Cuckoo's egg. From the *shape* of the nest, which was very narrow and deep, and from the *position* of the nest, which was built on slender reeds, on the outer edge of the bed, it was utterly impossible that the egg could have been laid, as in the first place, the nest was far too small for so large a bird as the cuckoo to sit in; and in the second, the weight of the bird would have inevitably swamped the nest. Does not this fact go far, at any rate, to confirm the theory held by many ornithologists to be the correct one, that the female cuckoo drops her eggs into nests by means of her bill, as it is well known she is provided by Nature with an enlargement in the throat, in which the egg could be carried in safety during her flight in search of a suitable place in which to deposit it. I give here a quotation from Bewick on the subject:—

"Naturalists are not agreed as to whether the female cuckoo lays her egg at once in the nest of another bird, or whether she lays it first on the ground, and then, seizing it with her bill, conveys it in her throat (supposed to be enlarged for this purpose) to the nest which is to be its depository."

I should be glad if any of your correspondents will inform me if the male bird has a like enlargement in the throat, or is it only to be found in the hen? T. AUDAS.

Regent's Terrace, Hull

THE LATE MR. ARCHIBALD SMITH

MR. ARCHIBALD SMITH was born at Glasgow in 1813; his father, Mr. James Smith, of Jordanhill, Lanarkshire, was well known as a geologist, and as the author of a learned and critical work on the Voyage and Shipwreck of St. Paul.

At the University of Glasgow Mr. Smith was a contemporary of the late Norman McLeod and of the present Archbishop of Canterbury, with both of whom he retained a friendship through life.

From Glasgow he went to Trinity College, Cambridge, where, while still an undergraduate, he commenced to contribute papers to the Mathematical journals; his first, a most important paper "On the Equation to Fresnel's Wave Surface," is an excellent example of the extreme neatness and elegance of his style; it was published under the signature A. S. in the Cambridge Phil. Trans. and in the Phil. Magazine.

He, however, as the result well showed, did not allow his amateur mathematics to interfere with the regular course of Tripos reading, and he also found time for a good share of athletic exercise. He pulled in the Trinity boat of which the late Lord Justice Selwyn was stroke; all the oars in that boat were reading men, and were familiarly known as "Peacock's examples" (Peacock being a well-known tutor of the day). It was no doubt owing to Mr. Smith's strong physical constitution which was thus well trained in early life, that he was able so long to sustain the great strain of mental effort and the want of rest to which he never scrupled to subject himself in after years when occasion required.

In 1836 he finished his undergraduate's career by taking the first place in the mathematical tripos as well as the first Smith's prize, and he was soon after elected a Fellow of his College. The second wrangler of his year was Bishop Colenso.

Having chosen the profession of the Chancery Bar, Mr. Smith became a pupil and a friend of Mr. James Parker, afterwards Vice-Chancellor, and is said to have acquired the sound legal learning and careful method which distinguished that judge. It was during the intervals of his laborious Chancery practice that he found time for the long series of magnetic investigations which has made him famous throughout Europe.

His connection with Magnetic Science arose from intimacy with Sir Edward Sabine, the late distinguished president of the Royal Society, and who was interested in the question of the Deviation of the Compass, first as member of a committee appointed by the Admiralty to consider the question, and afterwards as having undertaken the reduction and publication of the magnetic observations made by Sir James Ross in his Antarctic voyage.

In the years 1842 to 1847 Mr. Smith, at General (then Colonel) Sabine's request, deduced from Poisson's general equations, formulæ for the correction of the observations made on board ship. These were published in successive numbers of Sabine's "Contributions to Terrestrial Magnetism," in the Transactions of the Royal Society.

In 1851, at the request of Captain Johnson, the superintendent of the Compass Department of the Royal Navy, he deduced from the formulæ the convenient tabular forms, and computed the auxiliary tables for determining the co-efficients A, B, C, D, E, which have ever since been in use. These were published by the Admiralty in successive editions, but without the demonstrations or formulæ.

In 1859 Mr. Archibald Smith edited and published the voyage of Scoresby to Australia, which was undertaken chiefly for magnetic research; and in his introduction gave, for the first time, the *exact* formulæ for the effect of the iron of a ship on the compass, the former approximate formulæ being found insufficient.

In 1862 he, conjointly with Captain Evans, the present chief of the Compass department, prepared the Admiralty Compass Manual, a book which has since been translated into French, German, Russian, and Portuguese, and gone through three editions. The work is divided into four parts, the first of which contains practical rules to enable a seaman by the process of swinging his ship to obtain a table of the deviations of the compass on each point, and then to apply the tabular corrections to the courses steered. The second part is a description of "Napier's graphic method," the practical advantages of which are that it enables the navigator from observations of deviations made on any number of courses, whether equi-distant or not, to construct a curve in which the errors of observation are as far as possible mutually compensated, and which gives him the deviation as well on the compass courses as on the correct magnetic courses. Part III. contains the practical application to this subject of mathematical formulæ derived from the fundamental equations deduced by Poisson from Coulomb's theory of magnetism. Prior to this time it was considered sufficient to use approximate formulæ, going as far only as terms involving the first powers of the co-efficients of deviation; but the very large deviations found in iron-plated ships of war rendered it desirable to use in certain cases the exact instead of the approximate formulæ, and this part was therefore re-written. The fourth part of the "Manual" contains charts of the lines of equal variation, equal dip, and equal horizontal force over the globe; the first for the purpose of enabling the navigator at sea to determine the deviation by astronomical observations, the two latter to throw light on the changes which the deviations undergo in a lengthened voyage, and to enable the navigator to anticipate the changes which will take place on a change of geographical position.

All Mr. Smith's investigations were undertaken as labours of love; but we must not leave unnoticed some of the recognitions which he received.

In the year 1865 one of the Royal medals of the Royal Society was awarded to him, and he was elected a corresponding member of the Naval Scientific Committee of Russia; in the following year the Emperor of Russia, with a most complimentary letter, presented him with a gold compass emblazoned with the Imperial arms, and set with brilliants.

Recently, too, our own Government offered him a present of 2,000*l*., and intimated the fact to him in a handsome letter from the First Lord of the Admiralty, begging his acceptance, not by way of recompense, but as a mark of the high appreciation which the Government had for the services he had rendered.

The history of Mr. Archibald Smith's legal life is soon told. He attained the reputation of being an eminently concise and perspicuous draughtsman, and made a practice at the bar which was above the average both in extent and importance.

When Sir James Parker was made Vice-Chancellor he appointed Mr. Smith his Secretary; but the early death of Sir James brought these duties to a close. Later, a Judgeship in Queensland was offered to him, which he declined. It is said that the important change which has substituted figures for words as to dates and sums occurring in bills in Chancery was made at the suggestion of Mr. Archibald Smith.

In 1868, when the Universities of Glasgow and Aberdeen were formed into a parliamentary constituency the liberal electors chose Mr. Smith as their candidate, and they did their best, though without avail, to bring him in for the new seat.

About two years ago he was compelled by ill-health to give up work; but he had greatly rallied; and the attack which ended fatally was totally unexpected, and of but a few hours' duration. In private life those who knew Mr. Smith best admired him most; he leaves unnumbered friends to testify to the noble simplicity of his disposition, and to the true warmth of his heart, which was always open amongst his multifarious and engrossing work.

NEW EXPERIMENTS FOR THE DETERMINATION OF THE VELOCITY OF LIGHT BY M. ALFRED CORNU

AN exact value of the velocity of light is equally interesting to astronomers and physicists. It is interesting to astronomers, for it enables us to calculate an important and not exactly known number, namely, the distance from the sun to the earth, for which cause the learned world is looking forward with so much impatience to the passage of Venus on the disc of the sun, as the observation of this phenomenon, it is hoped, will fill up this chasm. It is interesting to physicists likewise, it is evident, but especially since the remarkable researches* of Prof. Clerk-Maxwell, who has found an unexpected relation between the theories of light and electricity.

M. Alfred Cornu's experiments, to which we now call attention, have for these reasons a great interest.

The first who busied himself with this difficult question was Rœmer, a Dane, at the Observatory of Paris, where Picart had called him; but the observation of the eclipses of Jupiter's satellites, although giving a pretty good value of the velocity of light, offers, notwithstanding, some causes of error, especially the difference of brightness of Jupiter's satellites at their maximum or minimum distance from the earth; and it requires moreover an exact value of the diameter of the terrestrial orbit.

M. Fizeau (1849) showed that it was not necessary to employ astronomical phenomena, and that it was possible on the surface of the earth to make use of relatively short distances, such as four or five English miles. This rather bold experiment was much spoken of. He operated between Montmartre and Suresnes, near Paris, at a distance of about five English miles and a half.

Léon Foucault, some time after, putting into execution a project of Arago, proposed another method founded on the revolving mirror of Sir Ch. Wheatstone. The value obtained by him, 189,000 miles (298,000 kilometres) was made use of by astronomers, who deduced for the parallax of the sun a number (8″. 86), that is in concordance with the best observations of the transit of Venus.

The number obtained at first by M. Fizeau was higher, but it was given by him, who dwelt upon all the difficulties of such a measurement, with hesitation.

M. Alfred Cornu left aside Foucault's method (viz., that of the revolving mirror) which is liable to serious objections, and employed that of M. Fizeau, although he had tried the two methods of experiment at the Polytechnic School, where many physicists were able to see them.

M. Fizeau's method is free from all objection. A ray of light is sent between the teeth of a cog-wheel, and it is reflected at a great distance, so as to bring it back to the point of departure. If the revolving motion given to the wheel is sufficiently rapid, the ray on its way back meets a tooth, instead of a free passage, and does not pass through; when the speed is double, the ray meets the following interval, and passes through again, and so forth alternately for increasing rates of revolution.

Thus the returning ray alternately presents a minimum (or an extinction) and a maximum; but the speed of rotation (in order to be measured) must be kept constant during several seconds in those moments; it is one of the greatest difficulties of the experiment, for that speed is enormous. Let us add the want of precision in the evolution of a maximum or a minimum.

M. Alfred Cornu has obviated all those difficulties:—

1. By giving a speed of rotation not constant but increasing or decreasing according to a regular law, which he registers by means of electricity; so that he easily knows the speed at every moment.

2. By registering in the same manner the exact time in which the ray of light disappears and appears again: and thus he does not observe the instant of maximum or minimum, but two instants which are equally distant from the moment that is to be determined.

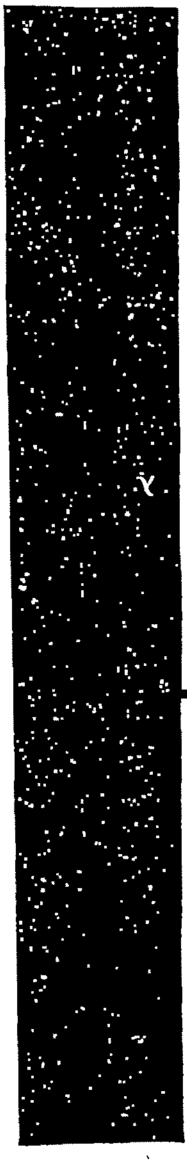
The various results are traced by fine needles that run on a sheet of paper covered with lamp-black, and rolled round a revolving cylinder. If the needles remain motionless, they describe a helix on the black paper, which becomes a straight line when the cylinder is unrolled. But these points are extremities of armatures of electro-magnets, and are moved when the electricity passes through; and during all the time the current passes, the traced line is above the level of the normal line.

The annexed sketch shows a part of an experiment made in the month of July 1872.

The line *a* on the right hand side represents the increasing speed of the wheel; each time a cog of the apparatus, in its movement of rotation, touched a certain wire, the electric current had passed through, and deviated the needle for the time the cog was passing (from A to B, from C to D). During the time, from the beginning of one deviation to the other (from A to C, from C to E, from E to G), 50,000 teeth had passed. We clearly see that these intervals are decreasing, because the speed increases.

The median line indicates seconds which are sent by an electric clock.

The third line has been obtained by the observer himself by means of a Morse-key; he made the electric current pass during the time the light was invisible; P Q and R S. The sketch thus shows two extinctions and two reappearances of light. It is the beginning of the experiment.



Copy of the Automatic Registrations.

This method, moreover, obviates one of the greatest difficulties in physical experiments, namely the noting down of various numbers, that diverts the observer and complicates operations. Furthermore, there remains not only the remembrance of the experiment made, but an exact, real, and living drawing.

M. A. Cornu has, moreover, changed the rather large and expensive apparatus of M. Froment for another, strong and small, for it is not bigger than the fists. He uses the works of a common clock, which do not cost more than a sovereign. He has only replaced the largest wheel of the scapement by another one, lighter and more finely toothed. Special experiments, not mentioned in his present memoir enabled him to choose the most proper diameter for that cog wheel. A strong spring drives the wheel 700 or 800 revolutions in a second.

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* Everyone knows that in one of the last meetings of the British Association Sir William Thomson has estimated them at their real value.

A drag has been added, in order to check the speed. By a special arrangement, the rotation of the wheel can be reversed, in order to eliminate certain errors that might result from the apparatus itself.

In order to try the improvements of the apparatus, a first series of experiments was made between the Polytechnic School and a tower of the telegraph office, at a distance of about one mile and a half (2 kilometres and a half). The observer could perceive a window of this tower amid a forest of chimneys. The distance was too short: he prudently did not publish the result.

A second series was attempted by him between the Polytechnic School and the Valérien Hill, at a distance of about six miles and a half (10 kilometres 310 metres).

But a transparent atmosphere is seldom now to be obtained in misty Paris If we go up to the garret where the observer stands, we perceive a sea of roofs below; on the right Montmartre Hill, on the left the heights of Meudon, and in the front the Valérien fortress; in one of the rooms in the barracks the mirror and the collimator were established.

The apparatus that sends forth the ray of light (an instrument with a large aperture) was laid on a solid timberwork; in front of the eyepiece is the little machine; on the left side the source of light is established, a ray of which, reflected by a glass, is sent between two teeth of the wheel.

But the Mont Valérien is concealed by mist; the window of the barrack is hardly distinguishable, although the sky is cloudless. Paris is covered with a damp and dusty veil. The sun sets behind the fortress, and suddenly the mist disappears and the air becomes transparent. The ray of light between the teeth of the wheel is to be seen in the telescope as a faint star in the midst of the inverted image of the window; it is a star of the sixth magnitude, the intensity of which increases and becomes of the first magnitude with the transparency of the air. But it is necessary to make the experiments hastily, for that transparency will not last more than one hour.[*]

An obstacle nearly checked the observer; the image often scintillated, and was agitated in such a manner that it was impossible to pursue the experiment. It was the warm air of a chimney unluckily standing in the way of the ray of light, the kitchen chimney of the Lycée Louis le Grand. M. Cornu waited for the holidays, and the operations were at last worked out.

He thus made more than a thousand experiments, and calculated 690 of them.

In order to determine the distance between the two stations, he compared the measures previously determined, and made himself a triangulation; the average of those numbers gave him the number above cited, about six miles and a half (10 kilometres, 310 metres).

He did not at once take the average of the numbers of his experiments, but he gave a greater value to the numbers obtained under the best circumstances. It appears evident that the results deduced from the fifth disappearance of the light are superior to those deduced from the first one, because of the more exact value of the velocity of the wheel, and that the favourable atmospheric condition rendered the disappearance and reappearances of light more plain.

The average thus obtained gives for the velocity of light 189,300 miles in a second; by dividing the number by the refractive indices of the air (1·0003) we obtain the number 189,200 miles in a second in a vacuum; the possible error in this value is about $\frac{1}{300}$.

M. Fizeau had found about 194,000 miles (312,000 kil.); Foucault 189,000 miles (298,000 kil.). The physicists will wonder at the concordance between M. Cornu's number and that of Foucault, obtained by an entirely different method; and so will the astronomers; for this number of 189,000 miles gives by calculating the value of the parallax of the sun the number 8″.86; and it is exactly the one recently obtained by M. Leverrier as a consequence of three series of observations made on the movement of planets, particularly of Mars and Venus.

If experiments on the velocity of light were made again under good topographic and atmospheric conditions, and between two stations, the distance of which would be known by a geodetic calculation, a value of this velocity would be obtained with an error less than $\frac{1}{1000}$. Astronomical methods do not easily perhaps give such an approach.

The author concludes his paper by saying: "It is to be desired for the honour of French science, that those great works relative to the velocity of light, begun by Rœmer at the observatory of Paris, pursued and simplified by some learned Frenchmen, should be finished in France with a precision worthy of their astronomical and physical importance."

M. C.

Explanation of the Diagram (*see next page*)

A, Source of light; a petroleum lamp. B, a combination of lenses to direct and concentrate the light. C, D, E, F, are shown from above in order to show the direction of the ray of light:—C, glass plate, on the surface of which the light is reflected and sent into the telescope according to the direction of the arrows; *a* is a little handle which permits of small motions being given to the little plate in order to arrange it properly. D, works of a common clock drawn to the $\frac{1}{4}$ of its linear dimensions.—It is used to put in motion the cog wheel Y, between the teeth of which the ray of light is sent forth. *W*, wire touched by a cog *d* of the axis of the third wheel, at each revolution; it is united to the electro-magnet *n* (of the plate G), and thus the number of revolutions during a second is registered by *r*. b_1, b_2, two barrels that give revolving motions in a contrary direction, in order to eliminate certain errors that might result from the apparatus itself. *k*, a wheel on the side of which a drag H bears (H has been drawn apart for greater clearness); *l*, horizontal axis of rotation; V, screw; whenever by means of V the rod *p* is brought to *q*, in the same manner *t* is brought to *u*, and the extremity *s* does not rub on the side of the wheel. (Note.—A decreasing pressure is thus used, an increasing one is rendered impossible so as to prevent the delicate works from being broken.) D′, front view of the same work; the same things designed by the same letters primed. D″ shows the respective situation of the two barrels b_1 and b_2. E, telescope; the light is transmitted to a distance of six miles and a half, and comes back on the same path: the apparatus that reflects it back is a telescope like E, and performing the office of a collimator the eye-piece of which is replaced by a little mirror properly disposed. F, eye-piece of E, with which the ray of light is observed at its return; it is observed through the glass-plate C on which it has been reflected. G, apparatus by which the various data of the experiments are registered. X, lamp-blacked cylinder. Y, moveable system bearing the electro-magnets *l*, *m*, *n*. The cylinder revolves without changing its place with an uniform rotatory motion given by a special apparatus. The movable system slides by a uniform motion communicated by means of a stretching weight. The manner of giving this motion has not been represented; the relative motion is the same as if the system were immoveable, and the cylinder going forwards and revolving in the same time. *l*, *m*, *n*, electro-magnets; *p*, *q*, *r*, armatures; they terminate in needles and describe on the lamp-blacked paper the three lines drawn on the sketch. One extremity of the wire of the electro-magnets communicates with the earth, the other with a pole of a special pile; the other pole of the pile communicates also with the earth. On the way of the current that passes through from each particular pile to the three electro-magnets *l m n*, is placed an interruptor different in each case. It registers: for *n* the law of rotation of the wheel *k* (it gives the speed of the wheel Y for each moment); for *m*, the seconds of time; they are sent by an electrical clock; for *l*, it registers the time of appearances and disappearances of the light, by means of a Morse-key, on which the observer keeps his hand during the experiment. Each experiment with six, and even seven disappearances, lasts about two minutes.

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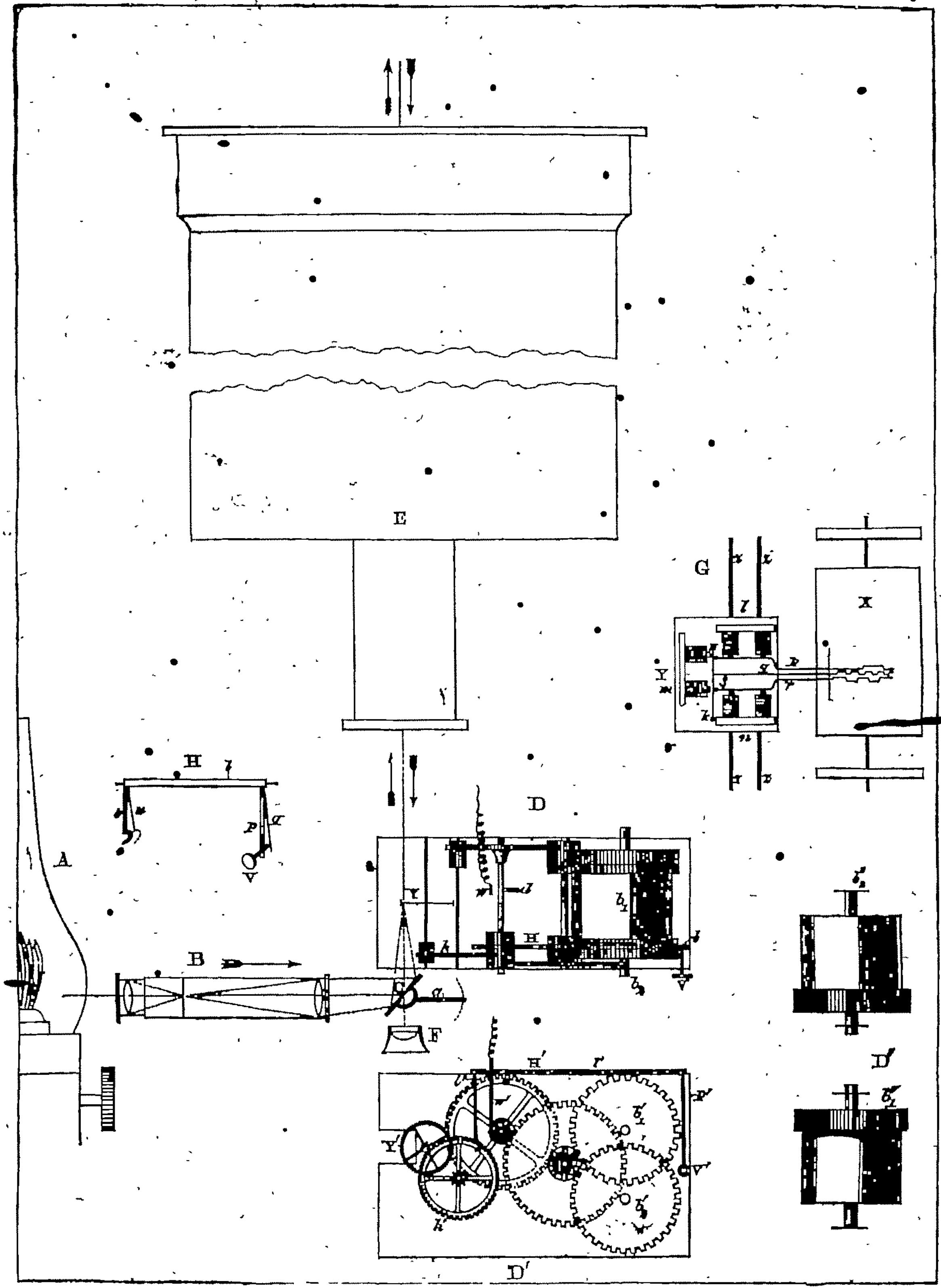
[*] The source of light was Drummond's lime-light, or only a petroleum lamp. It was necessary sometimes, in the finest weathers, to moderate it, in order to have a disappearance of light more favourable to observations than a minimum of intensity.



Cornu's Apparatus for ascertaining the Velocity of Light.

ON THE FERTILISATION OF FLOWERS BY INSECTS AND ON THE RECIPROCAL ADAPTATIONS OF BOTH

DURING the last ten years, since, by his wonderful work on Orchids,* Darwin anew turned the attention of naturalists to the remarkable connection between the structure of flowers and the insects visiting and fertilising them, many essays on the contrivances of flowers as apparently affording facilities for intercrossing distinct individuals have been published; but there is no doubt that by far the greatest part of the work on this subject is still to be done. The most conspicuous flowers attracted, of course, in the first place, the attention of inquirers, and much greater pains was taken to show the possibility of their cross-fertilisation by insects than to observe whether self-fertilisation may possibly take place if not visited by insects. Another very obvious deficiency of observations indispensable to be made on the subject in question resulted,—the fertilisation of flowers by insects being studied by botanists but little acquainted with insects. From this cause, for the most part, when flowers were examined as to their intercrossing by insects, no complete observations were made as to the insects themselves which were supposed to visit and fertilise the flowers, and in many cases the agency of insects was over-estimated in consequence of not observing them directly.

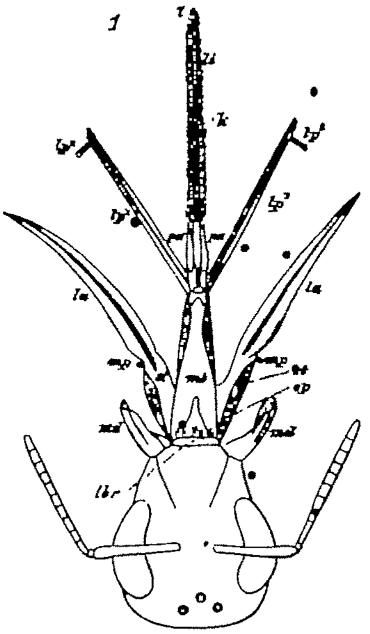


FIG 1.—Head of a humble-bee (*Bombus muscorum* L. ♀) seen from above, with the oral apparatus stretched out to its fullest extent (5 : 1)

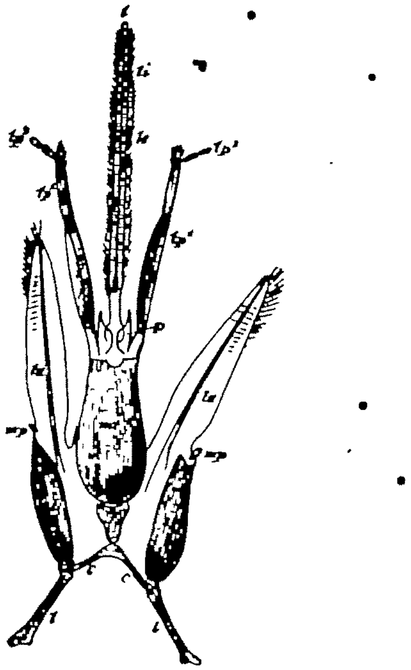


FIG 2.—Sucking apparatus of a honey-bee seen from beneath (12 : 1)

Therefore, being myself acquainted with our flowers as well as with a great number of our insects, I thought it would be as agreeable as useful if I observed, as far as it was possible for me, the insects which really visit and fertilise our flowers, their adaptations to gain the honey and the pollen, and on the other hand, the adaptations of our flowers to the insects that visit them; and having during a series of years bestowed all my leisure upon observations of this kind, I put them together in a work which was published some months ago ("Die Befruchtung der Blumen durch Insecten und die gegenseitigen Anpassungen beider." Leipzig, 1873.) Supposing that this book is in the hands of only very few Englishmen, I think it may be of some interest for the readers of NATURE if I make them acquainted with the principal new facts contained in my work, adding some observations made since its publication.

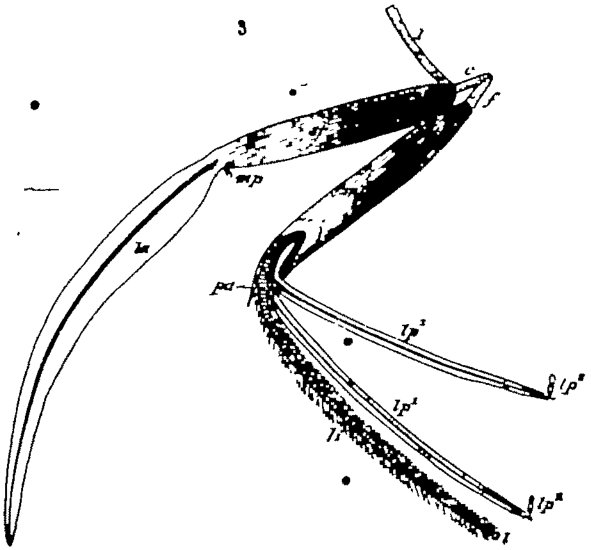


FIG 3.—Lateral view of the sucking apparatus of a humble-bee (*Bombus silvarum* L.), representing all the four foldings partly commenced, partly imperfectly executed. A piece of the tubular mentum is broken away to show the folding of the base of the tongue (7 : 1).

I.—*In what manner the hive- and humble-bees obtain the honey of the flowers*

The first accurate description and drawing of the parts of the mouth of the hive-bee were given by Swammerdamm about two centuries ago, but he did not succeed in finding out the true function of the tongue; he described and drew it as perforated at the end,* and believed that it was a simple sucking pipe. His successors saw that the tongue of the bee is by no means perforated at the end, and that fluids, for that reason, cannot enter through its interior, but must be transported to the opening of the œsophagus by the outside of the tongue. Thus with Swammerdamm's error, that the tongue was perforated at the end, the view that it was a sucking organ was also rejected, and since then, even down to our own day, zoologists seem almost unanimously to have denied in general the sucking power of bees. Milne-Edwards calls the Hymenoptera licking insects ("Insectes lécheurs"), and says that the honey-bees nourish themselves not by sucking, but, as it were, by lapping, nearly in the same manner as a cat does ("Ainsi il n'est pas en pompant que l'Abeille se nourrit, mais pour ainsi dire en lapant à peu près comme le fait un chat"). In like manner Carl Vogt expresses his opinion on the same subject, with only the difference that he chooses for the comparison the dog instead of the cat. The bees make use of their tongue to lap, says Carl Vogt, in a somewhat similar manner as dogs apply their tongue to drink ("Sie gebrauchen ihre Zunge etwa in ähnlicher Weise zum Schlappen, wie die Hunde sich der ihrigen zum Saufen bedienen.") * Also Claus † calls the parts of the mouth of the Hymenoptera biting and licking ("beissend und leckend"), and Gerstaecker blames, in his annual report on the Progress of Entomology, Schenck for describing the tongue of the bees as serving to suck honey, whereas, according to Gerstaecker's opinion, it is only able to lick it. Hence, a good number of our best zoologists absolutely denying the sucking of bees, and our entomological works affording, indeed, very detailed descriptions of the single parts of the mouth of the bees, but not sufficiently accurate ones of the use of them, it may not be fruitless if I explain here, in some detail, the function of the oral apparatus of the bee.

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* "On the various Contrivances by which British and Foreign Orchids are fertilised by Insects, and on the good Effects of Intercrossing." London, 1862.

* "Joh. Swammerdamm, Bibel der Natur. Aus dem Hollandischen übersetzt." Leipzig, 1752. Taf. xvii.

If we stretch out to its fullest extent, as shown in Figs. 1 and 2, the complex machinery of the oral apparatus of a hive- or humble-bee, which, when at rest, is placed by different foldings in an excavation in the under-side of the head, so as to permit but little of it to be seen, the most prominent part we observe is the long vermicular annulated tongue (*ligula*, *li*), at the end of which a little membranous lobe is seen (*l*), the same which was erroneously thought by Swammerdamm to be perforated. The ligula is composed of a great number of rings, each of which is provided with a whorl of hairs; each whorl of hairs can be erected at will by the bee and pressed close to the ligula. The base of the ligula, which bears two appendages, the *paraglossæ* (*pa*), is inserted, together with them, in the tubular *mentum* (*mt*), and can be drawn back, as Fig. 3 shows, into the extremity of the tubular mentum, so that only the tips of the paraglossæ are visible. On both sides of the ligula we observe, also inserted in the mentum, the two four-jointed *labial palpi* (*lp*), the two first joints of which (lp^1), being flattened and very slender, with a central rib, form a sheath to the tongue, enclosing it from beneath, whilst the two minute joints at the tip of the labial palpi (lp^2) serve as feelers.

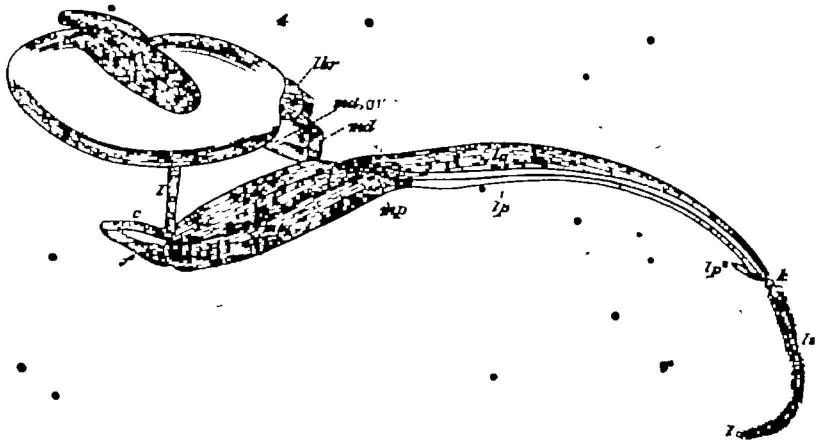

FIG. 4.—Lateral view of the sucking apparatus of a humble-bee (*Bombus hortorum* L. ♀) in a middle sucking position (7 : 1).

When drawn back into the extremity of the tubular mentum, as is shown in Fig. 3, the tongue by no means overtops the labial palpi, but is wholly enclosed by them from beneath, whilst when pulled out as far as possible (as shown in Figs. 1, 2, and 4) it considerably overtops the labial palpi. The base of the mentum is inserted in a horny ridge, called by Kirby (in his "Monographia Apum Angliæ") the *fulcrum* (*f*). The fulcrum is placed at the conjunction of two diverging horny ridges, called by Kirby *cardines* (*c*), which connect the base of the fulcrum with the basal portion or *stipes* (*st*) of the maxillæ. The cardines can be turned round their food-points; when turned forwards, they also push forwards the fulcrum and the mentum, so as to overtop considerably the basal portion of the maxillæ (as shown Figs. 1, 2); when turned backwards, they also draw backwards the parts inserted in them, and the mentum is now enclosed by the basal portion of the maxillæ (as shown in Fig. 4). In this position the terminal portions of the two maxillæ, the *laminæ* (*la*) appearing as two flattened, lanceolate, horny pieces with a central rib, form a sheath to the tongue enclosing it from above, whilst at the same time the two first joints of the labial palpi enclose it from beneath. The *maxillary palpi* (*mp*) exist in the mouth of typical bees only as atrophied useless organs.

Besides the two foldings hitherto explained, two other foldings are to be mentioned. First, the whole apparatus hitherto described is inserted in the terminal points of two long, horny ridges enclosed in the excavation of the head and moveable round their food-points (*l*, *lora*, of Kirby). When turned forwards, the lora push forward to twice their own length the maxillæ and the mentum, with all their appendages; when turned backwards, they draw them backwards the same distance. Secondly, when all three withdrawals hitherto mentioned—(1) of the base of the tongue, (2) of the cardines, (3) of the lora—are effected, the mentum lies, defended on each side by the basal portion of the maxillæ (*st*), enclosed in the excavation of the under-side of the head; only the tongue sheathed by the laminæ and the labial palpi overtop the head, and prevent the jaws from being used; but all these overtopping parts are bent downwards and backward very easily; and now the jaws or *mandibulæ* (*md*) are not prevented from being employed.

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* C. Vogt, Zool Briefe i p 678.

† Grundzüge der Zoologie, 1866, p. 523.

The separate parts of the mouth of the bee and their power of moving having been considered, it remains to examine what use the bee makes of them in its different actions.

1. In order to empty the deepest honey tubes accessible to it, the bee stretches out all the moveable parts of its sucking apparatus (lora, cardines, laminæ, maxillar palpi, and tongue) in the same manner as is shown in Figs. 1 and 2, with the only difference that the two first joints of the labial palpi sheathe the tongue from beneath, and that the laminæ closely embrace the mentum and the basal part of the tongue from above. Then the terminal hairy whorls of the tongue, protruded as far as possible and advanced to the bottom of the honey-tube, being wetted with honey, the bee, turning backwards the cardines (*c*), withdraws the mentum, together with the tongue and the labial palpi, so far that the laminæ are no longer overtopped by the labial palpi, and that the laminæ and the labial palpi together, closely embracing the tongue, form a sucking-pipe, of which only the part *k-l* (Fig. 4) of the tongue is prominent. But almost at the same time the bee, folding the base of the tongue into the tubular extremity of the mentum, withdraws the terminal hairy whorls wetted with honey into the sucking-pipe, in which the honey is forthwith driven downwards to the oral opening by the erection of the whorls of hairs progressing quickly from the tip of the tongue towards its base, and simultaneously by the enlargement of the interior abdominal hollows connected with the œsophagus, which are visible from the outside by the swelling of the abdomen, and which must suck the honey towards the œsophagus.

Fig. 4 shows the head of a humble-bee in a medium sucking position. When from this position the base of the tongue is folded into the hollow extremity of the mentum (as illustrated by Fig. 3), the part *k-l* of the tongue wetted with honey is withdrawn into the sucking-pipe. Now when the lora (*l* in Fig. 4, directed downwards) are turned backwards round their food-points, the base of the sucking pipe (near *mp* Fig. 4) is withdrawn to the opening of the mouth (between the base of the two mandibulæ, *md* and the labrum, *lbr*, Fig. 4, below the epipharynx *ep*, Fig. 1), and the honey is, by pressing and sucking, driven to the œsophagus. When the lora (*l*) are again turned forwards, the whole sucking apparatus is pushed forward double the length of the lora; and now the cardines turning forward, the mentum with its appendages again advances double the length of the cardines, while the maxillæ remain at the same place, and the laminæ from this cause embrace only the mentum and the basal portion of the tongue; when at last the base of the tongue infolded in the tubular mentum is stretched out, the tongue is again protruded to its fullest extent, and the terminal whorls of hairs are again wetted with honey at the bottom of the honey-tube of the flower.

In a flower rich in honey, a humble bee may be observed executing four, five, and sometimes more, even eight or ten separate acts of suction, probably accompanied by as many protrusions of the tip of the tongue into the honey, and withdrawals of it and of the whole sucking-pipe.

I am fully convinced that the movements of the oral apparatus of the bees are as described; for by intoxicating honey- and humble-bees by chloroform, and immersing the tip of their tongue into a solution of sugar, I sometimes succeeded in seeing the movements described performed sufficiently slowly to discern each separate act very well. What occurred within the sheath of the tongue formed by the laminæ and the maxillary palpi, was of course not visible, but bending them aside after wetting the tip of the tongue with the solution of sugar, I sometimes saw the erection of the whorls of hairs progressing from the tip towards the base of the tongue.

Hence undoubtedly the statement of zoologists, who, absolutely denying the sucking power of bees, assert that they lick or lap the honey in a manner similar to a dog or a cat when drinking, must be essentially modified. The terminal whorls of hairs are filled with honey by adhesion; this honey withdrawn into the sheath of the tongue is driven towards the œsophagus by a double cause, first by the pressure of the erect whorls of hairs, and secondly by suction.

HERMANN MULLER

(*To be continued*)

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*ON SOME REMARKABLE FORMS OF ANIMAL LIFE FROM GREAT DEEPS OFF THE NORWEGIAN COAST**

THE name of George Ossian Sars is honourably connected with a very interesting chapter in the history of deep-sea research. As early as 1850, his illustrious father, Dr. Michael Sars, had challenged Edward Forbes's conclusions respecting the bathymetrical terminus of animal life. He remarked,† that at least in the Norwegian Seas, it appeared to extend much beyond the limit which the English naturalist had fixed for it. Forbes had not dredged below 230 fathoms, and at this depth he had only obtained two living Mollusca and a couple of Serpulæ; hence he was led to place the zero of animal life at 300 fathoms. Sars, on the contrary, even at the early period just mentioned, had obtained from a depth of 300 fathoms a number of animals, including a species of Coral, Molluscs, Polyzoa, &c.; and he sagaciously remarked that there was evidence of the existence of a *vigorous* animal life at this great depth, inasmuch as some of the species (*e.g. Terabratula septigera* and *Lima excavata*) were the largest known representatives of their respective genera. In confirmation of his opinion, he was able to offer, in 1864, a Catalogue of 92 animals, which had been obtained in depths varying from 200 to 300 fathoms. More recently his son has devoted himself with much energy and success to deep-sea investigation, and in 1868 had extended his dredgings to 450 fathoms, and added no less than 335 species to those already published. He says:—"I found to my great surprise at this enormous depth, not . . . a poor and oppressed Fauna, but on the contrary a richly developed and varied animal life. . . . And so far was I from observing any sign of diminished intensity in this animal life at increased depths that it seemed, on the contrary, as if there was just beginning to appear a rich and in many respects peculiar deep-sea fauna, of which only a very incomplete notion had previously existed." Amongst the new forms thus obtained was the famous *Rhizocrinus Lofotensis*, descended from Oolitic ancestry, which furnished, according to Dr. Carpenter, "a principal 'motive'" of the *Lightning* expedition. It is interesting to learn that these productive dredgings at the great depth of 200–450 fathoms were accomplished in an ordinary fishing-boat with a crew of three men.

In the important paper which forms the subject of the present notice, Mr. G. O. Sars has given us an account of some of the results of his dredgings in the "great deeps" off the Coast of Norway, founded partly on the posthumous manuscripts of the late Prof. Sars, and partly on his own investigations. Various new species of Mollusca, Annelids, Corals, and Sponges, all of them dwellers in depths varying from 100 to about 500 fathoms, are described, and illustrated by excellent figures. But that which gives a peculiar and distinctive interest to the work is the elaborate memoir on a remarkable Polyzoon, taken in the year 1866, from a depth of 120 fathoms, at Skraaven, in Lofoten. This unique animal is not only generically distinct from all the forms that had been recognised at the time of its discovery, but must be referred to a new Order or Sub-class: it is chiefly interesting, however, to the biologist from the light which it throws on the history and affinities of the tribe to which it belongs. Its occurrence was first recorded in 1868 by the elder Sars, who gave it the name of *Halilophus mirabilis*, but did not at that time enter upon the details of its structure. In 1869 Allman described a new Polyzoon, under the name of *Rhabdopleura Normanni*, which had been dredged up from deep water in Shetland, and which presented some remarkable peculiarities. Its polypides (according to Allman) were of the Hippocrepian type, having the tentacles disposed in the form of a horse-shoe, instead of circularly, an arrangement which had only been noticed so far amongst the fresh-water division of the Polyzoa. Another anomalous character was the presence of a rigid, chitinous rod, extending throughout the creeping portions of the polyzoarium, to which the polypides were attached at intervals by means of a long flexible cord. It now appears that the Shetland Polyzoon belongs to the same genus as the Lofoten form just mentioned. Allman, however, having only access to specimens preserved in spirit, was unable to master all the details of the structure or to apprehend fully the significance of the organism as a whole. For a complete knowledge of *Rhabdopleura* we are indebted to the careful observations of the younger Sars, who studied the living animal; while to his father we owe a most interesting interpretation of the facts which the son had established.

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* Partly from posthumous manuscripts of the late Prof. Dr. Michael Sars. By George Ossian Sars.

† "Beretning om en i Sommeren, 1849, foretagen Zoologisk Reise i Lofoten og Finmarken," p. 13.

Without entering into minutiæ, I shall endeavour to describe briefly the characteristics which mark out the *Rhabdopleura* as unique, and invest it with so high an interest, not only for the student of the Polyzoa, but also for the philosophical biologist. In the first place, it may be stated broadly that we find in this form the Polyzoan type in a rudimentary and half-developed condition. It clearly represents a very early stage in its evolution, if evolution be the method of Nature. The points which separate it most strikingly from its congeners are not the equivalent of the ordinary differences that occur amongst the members of the same class; they might rather be regarded as surviving features of another and very different type, from which it has diverged, and are strictly transitional in character. *Rhabdopleura* is a Polyzoon, and yet not *all* Polyzoon. A large portion of its structure, while clearly taking the Polyzoan direction, differs widely from that of all known Polyzoa. Some of the features which we should regard as most characteristic of this class are altogether wanting. And organs in which the Polyzoan type is most distinctly traceable, appear in a simpler and more rudimentary condition than in any other known form. In a word, two types of structure seem to blend in this remarkable animal, one, as it were, fading away, and the other dawning.

The polyzoarium in *Rhabdopleura* bears a striking remblance to that of a Hydroid, and might belong to a *Coryne* or *Eudendrium*. It consists of a number of erect, chitinous tubes, distinctly annulated, which are united by a creeping, tubular stem. Each of the erect tubes (zoœcia) contains a polypide, and every polypide is attached by a contractile cord to a dark-coloured, cylindrical rod, which pervades the creeping portion of the polyzoary. The polypide differs from those of the normal Polyzoa in the following important particulars:—

1. It is without any sort of attachment to its cell, in which it lies quite free. In all other known Polyzoa a membrane (the endocyst) lines the cavity of the cell, and envelopes the polypide, to which it is attached above, at the base of the tentacular crown. When the animal retreats into its cell, it draws in with it the anterior portion of this membrane, which securely closes the aperture. Between the endocyst and the body of the polypide is a space (the perigastric cavity), in which the nutritive fluid is confined. But in *Rhabdopleura* the endocyst is altogether absent, or appears in a perfectly elementary condition, as a "thin, glassy skin," immediately surrounding the digestive apparatus. There is nothing to close the orifice of the cell, and the surrounding water passes freely into its interior. There is no perigastric cavity or fluid. The polypide is as free and unattached as a Hydroid in its calycle; and its only connection with the colony is through the contractile cord already referred to.

2. The digestive system is of the Polyzoan type, but of much lower grade than is found elsewhere. There is little specialisation of parts; the stomach and intestine consist of a simple tube, wider towards its upper extremity and narrowing off rapidly towards the posterior end, which is bent abruptly upon itself. The intestine is not separated from the true stomach by any valve, but is an immediate continuation of it, and passes off from its lower extremity in a straight line to the anal orifice.

In the normal Polyzoa, on the contrary, the stomach is divided into two well-defined regions; and the intestine, which is marked off by a distinct valve, takes the origin between the upper portion and the large, sub-globular sac, in which it terminates below. We have in *Rhabdopleura* the bent tube and the two orifices (oral and anal), but beyond this, perfect simplicity of structure.

3. The tentacular apparatus exhibits some remarkable features. It differs essentially from that of the marine, and also from that of the fresh-water Polyzoa, though it most nearly approaches the latter. It consists of two symmetrical lobes or arms, which extend out dorsally from the anterior part of the body, diverging to each side; and each of which bears a double row of ciliated tentacles. These lobes are very flexible, and exhibit great mobility, bending slowly in various directions; and in this respect they contrast strikingly with the unchanging lophophore of the fresh-water Polyzoa. The single tentacular crown, which belongs to all the other known members of the class has here disappeared; and instead of the circular verticil of the marine, and the crescentic but continuous series of the fresh-water species, we have here two series, borne on distinct flexible and movable appendages.

4. In *Rhabdopleura*, the complicated muscular system concerned in the protrusion and retraction of the polypide, which is so characteristic of the Polyzoa, and on which their lively and rapid movements depend, is suppressed along with the endocyst. *Retraction* is effected solely by means of the cord that passes from the body to the rod pervading the creeping stem. It is a very slow and sluggish process, the polypide exhibiting none of the sensitiveness and vivacity of its kindred. Under extreme provocation it retires very deliberately; an ordinary Polyzoon disappears with the speed of light, on the slightest alarm. This sluggishness, as our author remarks, is accounted for "by the want of special retractor-muscles, and by the slightly developed contractile elements, not distinguishable as evident muscular fibres, in the contractile cord."

Still more remarkable is the mode in which the protrusion of the polypide is effected. In the absence of the usual muscular appliances, it is difficult, at first sight, to imagine how the creature can raise itself from the lower extremity to the aperture of its tubular dwelling. It appears, however, that a special and most singular organ exists for the purpose, and that here also the *Rhabdopleura* departs altogether from the customs of its race. This organ consists of a large and prominent shield or disc, which projects from the anterior end of the body between the oral and anal orifices, and is thickly covered with cilia. It evidently corresponds with an anomalous structure (known as the *epistome*), which occurs only amongst the freshwater Polyzoa, and the function of which has not hitherto been determined. Sars has observed that this ciliated disc is closely appressed to the wall of the cell, during the process of protrusion, and is in fact a kind of foot or creeping-organ, by means of which the polypide laboriously draws itself up towards the aperture of its tube. The Polyzoon, which, in its normal condition, is equipped with a powerful muscular apparatus, and remarkable for its vivacious habits, here literally crawls out of its cell.

5. It only remains to notice the dark-coloured cord, which runs throughout the creeping stem, and is a very marked feature of this curious form. It is described as a cylindrical tube, with firm, horny walls, inclosing a soft, transparent, cellular substance, from which branches are given off at intervals, and enter into the contractile cord of each polypide. This "axial cord" may no doubt be compared with the so-called nerve-trunk pervading the stem of other marine Polyzoa—the principal element of the supposed colonial nervous-system. Our author rightly regards the soft substance extending through the cord, as a sort of incompletely defined nervous trunk connecting all the individuals of the colony.

Of the development of *Rhabdopleura* little can be said at present. Both Sars and Allman, indeed, have recorded observations made on the formation of buds; but they disagree in their interpretation of several important points; and we must wait for further information before we can master this portion of the history.

From the foregoing account it is evident, as stated at first, that in *Rhabdopleura* we have the polyzoan structure in a very rudimentary condition, and half disguised by features that are alien to it as it now exists; some of its principal elements are fully established, though in a simpler form than we find them elsewhere; some are altogether wanting; while one important class of functions (the various movements of the polypide) is provided for by means which have no parallel whatever amongst other members of the tribe, and in part by an organ, which survives, reduced in size and with a different office, in one section only, as the so-called *epistome* of the fresh-water species.

Allman's examination of the Shetland *Rhabdopleura*, as preserved in spirits, led him to regard the Polyzoa as connected with the Mollusca, through the Lamellibranchiata, rather than the Brachiopods. Prof. Sars, relying on his son's investigations, takes a very different view of their affinities. He regards the *Rhabdopleura* as an organism "which stands as it were in the middle between the Hydrozoa and the Polyzoa," and forms a transition from one to the other. It is undoubtedly, he says, "like many other animals which at present inhabit the greater depths of the sea, . . . *a very old form*, which in its organisation has still retained several features from the time when the animal type that we call Polyzoa first developed itself from a lower type." He considers it to prove that the Polyzoa "are most closely related to the type of the *Cœlenterates*, and especially to the class *Hydrozoa*," from which they are probably derived.

It is my present object merely to report results, and not to offer any criticism upon them; but it may safely be said that the paper, a portion of which I have summarised, is one of the most interesting and important contributions to biological literature, that have lately appeared.

It is right to add that the author, considering "one of the great universal languages" preferable to his mother-tongue, as the vehicle of scientific research; and as a graceful acknowledgment of the services rendered by our countrymen in recent times to zoological science, has courageously, and to the relief of many of his readers, written his memoir in English.

THOMAS HINCKS

NOTES

AT the Midsummer Commencements, held last week in Trinity College, Dublin, the honorary degree of LL.D. was conferred by the University of Dublin on Dr. Andrews, of Belfast, and Professor Wright, of Cambridge.

DR. JAMES MURIE, Professor of Anatomy in the Edinburgh Veterinary College, has been elected to the newly-founded lectureship of Animal Physiology in the Edinburgh School of Arts.

ARCHÆOLOGISTS will be interested, and no doubt pleased, to hear, that Sir John Lubbock has just bought Silbury Hill, the grandest tumulus in Great Britain, if not in Europe.

WE have a number of earthquakes to chronicle this week; that in India, it will be noticed, preceded only by a day those of Italy. The earthquakes in Chili, on the 15th May, were of a very serious character. They affected Valparaiso, Santiago, Quillota, La Ligua, Canquenes, and Salvados. At Chillan, Concepcion, and Talchuano, in the south, so far as we can understand, it was slight. At Valparaiso, it commenced at 12.32 P.M., and lasted forty-two seconds, with a vertical motion, so that the ground danced under foot. Two churches and many buildings were damaged. Gas branches were wrenched from the ceilings, and books thrown from the shelves. In Salvados, in Central America, the earthquakes had ceased in May. At 2 P.M. on the 28th June, Assecrghur Fort was visited by an earthquake which lasted for about three or four seconds, direction from north-west to south-east. On the morning of June 29, about five o'clock, an earthquake visited several parts of Italy. At Verona, Treviso, and Venice, though the shocks were severe, little damage was done; but at Feletto, north of Piane, and near Conegliano, the church fell in and thirty-eight people are reported to have been killed. At Belluno four persons were killed and several wounded. At Pieve del Alpago several persons were injured. Two persons were killed at Torres, four at Curago, eleven at Puos, two at Visione, and one at Cavessago.

WE regret to hear that difficulties have arisen in the management of the Brighton Aquarium, which are likely to lead to the resignation of Mr. Saville Kent, who lately vacated a post in the British Museum for that of Curator and Resident Naturalist to the Aquarium. Of the nature of the dispute we are not informed, but it seems unfortunate if some means may not still be found by which an amicable arrangement may be arrived at between Mr. Kent and his colleagues by which his services may be retained to the institution.

THE female Octopus at the Brighton Aquarium still continues to guard her clusters of ova with the greatest vigilance, refreshing them at short intervals by turning upon them a powerful stream by means of her tubular funnel; no increase to the number deposited having taken place since last week, the usual complement produced may be presumed to have been excluded. The truncate "*Hectocotylus*" arm of the male, in this instance the third on the left side, is fast recovering its normal condition, a new slender filamentous process has sprung from the ruptured extremity, resembling, in detail, the reproduced arm of an *Ophiocoma* or Brittle Starfish Mr. Saville Kent is of the opinion that the *Octopus tuberculatus* of D'Orbigny will prove on closer investigation to be the mate of *O. vulgaris*; the difference in appearance between individuals of the same species but the opposite sex being most marked when once recognised; the general surface of the integument in the female is comparatively smooth, while numerous rugosities and elevated papillæ adorn that of the male, more particularly in the neighbourhood of the head.

IT has been announced by cable from America that a new planet (No. 132) was discovered by Prof. Henry on June 13.

THE just published lecture, delivered in April last by Prof. Flower at the Royal Institution, on "Palæontological Evidence of Gradual Modification of Animal Forms," is accompanied by an excellent and very ingeniously-constructed diagram of the affinities of the different members of the class Ungulata, including all the fossil as well as the recent forms. Each genus is represented by a circle, the comparative size of which indicates the number of species included in it. The existing genera are left white, and those which have fossil representatives are surrounded by rings, which are so shaded as to make it easy by referring to an accompanying table, to find in which stratum the form first appears; the extinct genera appear as shaded circles. Consequently the Peccary and Babirussa are represented by unsurrounded white circles, while *Coryphodon* and *Lophiodon* are all shaded; *Antilope* is a large white circle surrounded by a late Miocene ring; *Aceratherium* has a central late Miocene circle and an early Miocene ring, indicating its range in time. Such a method applied to all the classes of animals, if equally thorough and accurate, would be an invaluable acquisition to Zoological Science.

THE following telegram dated Alexandria, June 30, 1873, 1 P.M., has been received at the Foreign Office, from the Hon. H. C. Vivian, Her Majesty's Acting Agent and Consul-General in Egypt:—"Telegram just received from Sir Samuel Baker, dated Khartoum, yesterday, reports his safe arrival there in good health, with all the other Europeans. The country as far as Equator annexed to Egyptian dominion. All rebellions, intrigues, and slave trade completely put down. Country orderly. Government perfectly organised, and road open as far as Zanzibar. El Zaraf navigable. Victory on June 8 with only 105 men, over army of Onioso. This mission completely successful."

M. DE LESSEPS is a candidate for the place in the French Academy vacant by the death of the late M. de Verneuil.

THE name *Drepanephorus* having been recently used by Sir Philip Egerton for a species of fossil fishes, Mr. Sclater proposes to change the generic name which he gave to the Paradise Bird discovered by the Italian naturalist D'Albertis, to *Drepanornis*. We shall shortly have the opportunity of offering to our readers a description of this bird from the hand of Mr. Sclater, together with a drawing illustrating its peculiarities.

SOME years ago, in connection with the Berlin Geographical Society, an Association, joined in by all the chief European powers except France and England, was formed for the purpose of determining a standard European metre, to be based on the exact determination of the meridian between Christiania and Palermo. The work has developed itself into the ascertainment of the dimensions of the globe, and the Association has been now joined by France, England thus being the only power which holds itself aloof from taking part in the highly valuable work. The result will be the union of the triangulation of the whole of Europe.

AT the recent D.Sc. examination of the University of London Mr. Richard Wormell, M.A., passed in Electricity, and Mr. Augustus C. Maybury in Geology.

ATTENTION has been lately given by the American Ethnologists to the fossil skeleton of Guadeloupe, and they support the suggestion that it belongs to the Carib race. This admission still allows of considerable antiquity.

DOCTOR Don Ricardo de la Parra, died at Envijado, in Antiouquia, U.S. of Colombia, on May 9. He was about to publish a work on Elephantiasis, which had been a special study.

THE volcano of Puraca, in the western state of Cauca, in the U.S. of Colombia, has been in convulsion for three years, and is now causing great alarm. It gives rise to frequent storms.

THE forthcoming number of Petermann's *Mittheilungen* will contain a very interesting article by Carl Dambeck on the Geographical Distribution of Sea-fish, in which the author divides the ocean into eleven regions, and gives lists of the principal fishes to be found in each region.

MR. LAMONT's fine yacht *Diana*, which was chartered by Mr. Leigh Smith, and which recently left Dundee on a Polar Expedition, is reported by the whaler *Eclipse*, which arrived at Peterhead on Sunday. The letters which have been received announce that the party were on June 1 last in latitude 77·40, being among the floating ice, which reached northward to Spitzbergen. At that time all connected with the expedition were well, and notwithstanding that very severe weather had prevailed since leaving Scotland, no accident had happened. The arrangements had been slightly interfered with in consequence of the tempestuous weather, and the island of Jan Mayen had not been reached. The *Diana* was to proceed along the outside of the ice towards the north-west corner of Spitzbergen, where she will meet a storeship which preceded her.

MUCH gratification is felt in Peru at the discovery of a new coal deposit near Pisco, which is said to be one of the best and richest on the Pacific coast, and the locomotives on the Ica and and Pisco Railway are using it with great success. The mine is situated close to the sea, and near a perfectly safe harbour, and the coal is said to be finer in quality than any in Chili, and of great extent, and, if so, must prove to be of very great economical value.

A GENERAL meeting of the members of the Aeronautical Society of Great Britain was held on Monday evening in the theatre of the Society of Arts, under the presidency of Mr. Glaisher. A number of models prepared for the occasion were exhibited by persons actively interested in the advancement of the great scheme of aërial navigation. The chairman, in his opening remarks, expressed his satisfaction at having to record several marks of progress made during the past year in the science in which they were all so interested. These marks were certainly slight, but they were nevertheless decided steps in the right direction. Very many experiments of the highest importance to the furtherance of aërial navigation had been carried out in many cases with what might be considered tolerably satisfactory results. The Society had, he added, expended a sum of 1,200*l*. in the construction of a balloon the motive power of which was to be brought about by a small steam engine, now in preparation, of a merely nominal weight, and giving, for its size, an exceedingly high pressure of steam. A model of this was exhibited in operation by Messrs. Thomas Moy and R. E. Shill. Papers were read during the evening by several gentlemen, including Mr. Bennett and Mr. D. S. Brown.

THE French "Society of the Friends of Science," an association for succouring the widows and orphans of men of science, has distributed during the last three years, in spite of the misfortunes of the country, 88,439 fr.

THE scarcity of rags has, it is well known, recently induced paper manufacturers to look out for new textures as substitutes for those formerly used. In France hop-stalks have been successfully utilised for this purpose, and in this country an attempt has been made to utilise jute for newspapers. A copy of the *Warrington Guardian*, printed on jute paper, has been sent us, and it appears to us quite satisfactory.

A SOCIETY for the Promotion of Scientific Industry has recently been established in Manchester. Its object is the increase of the technical knowledge and skill of those engaged in the various industries, the improvement and advancement of manufactures and the industrial arts and sciences, and the general progress, extension, and well-being of industry and trade. The society is sending out artisans to Vienna to profit by the Exhibition now being held there, as was done by the Society of Arts on the occasion of the Paris Exhibition, and it proposes to hold in the autumn an exhibition of designs in textile fabrics and of fuel economisers.

A PAPER entitled "Contributions to a Knowledge of North American Moths," by Aug. R. Grote, was read on June 6 before the Buffalo (U.S.) Society of Natural Sciences, in which it was stated that three new genera (Litognatha, Meghypena, Phæcasiophora), and nineteen hitherto undescribed species (Acronycta, 4; Agrotis, 1; Cloantha, 2; Litognatha, 2; Meghypena, 2; Botis, 1; Phæcasiophora, 1; Eurycreon, 1; Penthina, 3; Grapholitha, 1; Oeta, 1) occur in the North American insect fauna. At the same time a paper entitled "Descriptions of New Species of Fungi," by Chas. H. Peck, was read, in which it was stated that 142 hitherto undescribed species of fungi (Hymenomycetes, 96; Gasteromycetes, 11; Coniomycetes, 18; Hyphomycetes, 6; Ascomycetes, 11) occur in the flora of the United States.

IN connection with the Social Science Congress, to be held at Norwich, from the 1st to the 8th of October next, there will be an Exhibition of Educational, Sanitary, and Domestic Appliances, based on the experiment which proved so successful at Leeds in 1871. The object of the exhibition is to bring under the notice of the public generally, and particularly those who are interested in social, sanitary, and educational questions, the latest scientific appliances for improving the public health and promoting education. The exhibition will be open to exhibitors from all parts, and the management will be under the superintendence of a committee.

A VALUABLE paper in the May number of the *Canadian Journal* is a contribution to a Fauna Canadensis, by Prof. H. Alleyne Nicholson, being an account of the animals dredged in Lake Ontario in 1872. The dredgings were all carried on within a radius of ten miles from Toronto, and Prof. Nicholson describes the nature of the bottom, and forty-three species of animals taken up in the dredge, belonging to Annelida, Crustacea, Arachnida, Insecta, Mollusca, and Vertebrata. The paper possesses several points of interest.

WE have received Nos. 3 and 4 of the *School Laboratory of Physical Science*, a small quarterly journal edited by Prof. Hinrichs, Director of the Laboratory of the Iowa State University. The longest paper is entitled "Science in Schools," and gives a comparative view of the place occupied by Physical Science in the Classical Courses of the American Colleges, the palm in this respect being given to Harvard. Prof. Hinrichs thinks, notwithstanding the comparatively great importance attached to physical science in America, the place allotted to it in her universities is still far from satisfactory. Under the head of "Laboratory Notes," Prof. Hinrichs gives a method of determining the Velocity of Sound in the Atmosphere.

MR. T. LOGIN, C.E., Superintending Engineer, 2nd Circle, Punjab, has sent us a small pamphlet, entitled "Practical Notes on the Egyptian Mode of Cotton Cultivation," containing a series of well-arranged directions on this subject, founded on Mr. Login's own experiments, which appear to have been unusually successful.

WE have received from Messrs. Asher and Co., Nos. 378, 379, 380, of Kirchoff and Wigand's (of Leipzig) "Antiquarisches Bucherlager," containing long lists of very valuable works in Mathematical, Physical, and Mechanical Sciences.

ACCORDING to the *American Artisan*, the new educational system in Japan embraces the organisation of 8 colleges, 256 high schools, and over 50,000 public schools, at which the attendance is to be compulsory for all children above six years of age.

A SUPPLEMENT to the Fifth Annual Report of the United States Geological Survey of 1871, contains an enumeration with descriptions by Mr. Leo Lesquereux, of some tertiary fossil plants, from specimens procured in the explorations of Dr. F. V. Hayden. in 1870. Another small pamphlet connected with the same survey contains carefully compiled and very valuable lists of elevations and distances in that portion of the United States west of the Mississippi, collated and arranged by Prof. C. Thomas.

THE "Report of the Entomological Society of Ontario," for 1872, contains papers on Insects injurious to the Grape, the Strawberry, the Hop, the Maple, the Peach, the Potato, on some innoxious insects, and on beneficial insects.

WE have received the "Report of Progress" of the Geological Survey of Canada for 1871-72, containing detailed and well-compiled accounts from the various parties who are carrying on the work.

WE learn that there has been erected a small observatory on the Columbia (U.S.) College campus for educational and, we hope, also for scientific purposes. The observatory is furnished with an equatorial, accompanied by a seven-prism spectroscope, by Clark, and a position micrometer, besides an altazimuth and a zenith telescope.

WE take the following from a paragraph entitled "Prof. Agassiz on Natural History in Schools," in the *University Monthly* (New York):—"I am satisfied that there are branches of knowledge which are better taught without books than with them; and there are some cases so obvious, that I wonder why it is that teachers always resort to books when they would teach some new branch in their schools. When we would study natural history, instead of books let us take specimens—stones, minerals, crystals. When we would study plants, let us go to the plants themselves, and not to books describing them. When we would study animals, let us observe animals."

ADDITIONS to the Brighton Aquarium during the past week; 2 Bass (*Labrax lupus*); 14 Black Bream (*Cantharus lineatus*); 1 Ballan Wrasse (*Labrus maculatus*); 1 three-bearded Rockling (*Motella vulgaris*); 6 Sea Crayfish (*Palinurus vulgaris*); 1 Toad Crab (*Dromia vulgaris*), 1 Octopus (*Octopus vulgaris*), presented by Mr. C. J. Small, of Hastings; 1 Sea-hare (*Aplysia punctata*); Oysters (*Ostrea edulis*); Mussels (*Mytilus edulis*); Zoophytes (*Tealia crassicornis, Alcyonium digitatum*).

THE additions to the Zoological Society's Gardens, during the last week, include an Erxleben's Monkey (*Cercopithecus erxlebeni*); a Moustache Monkey (*C. cephus*); a banded Ichneumon (*Herpestes fasciatus*) and two bronze Spotted Doves (*Chalcopelia chalcospilos*), from West Africa, presented by Mr. J. J. Monteiro; a greater Sulphur Crested Cockatoo (*Cacatua galerita*), from Australia, presented by Mrs. Thomas; a Hyacinth Porphyrio (*Porphyrio hyacinthinus*), from West Africa, presented by Lady Cust; a grey Ichneumon (*Herpestes griseus*), from India, presented by Mr. W. Walker; an Argus Pheasant (*Argus giganteus*), from Malacca; two Rufous-tailed Pheasants (*Ceriornis erythrophthalmus*), from India; a white-handed Gibbon (*Hylobates lar*), from the Malay Peninsula; a Puma (*Felis concolor*), from Bogota; two Lanner Falcons (*Falco lanarius*), from E. Europe, deposited.

SCIENTIFIC SERIALS

Der Naturforscher, May.—This serial, containing little that is original, furnishes a weekly supply of well-selected and adapted matter from various sources. In the present number attention may be called to an academical address delivered by Herr Streng at Giessen, on the "circle-course" of substances in nature, treating chiefly of geological phenomena; to an account of Herr Janettaz's recent careful researches on the conduction of heat in crystals (some 44 mineral species having been examined); to a theoretical investigation by Herr Handl (Vienna Academy) of the conditions of saturated and supersaturated solutions, and to several papers of meteorological experiment: on moisture in forests and in the open, on the temperature of rain, and on the velocity of winds as measured on various heights on Antwerp Cathedral.—Some observations of M. Du Breuil on the partial decortication of horse-chestnuts, are worthy of notice. He found about twenty of these trees in the park at Compiègne, the bark of which had been eaten off twenty-four years previously, by rabbits, to a height of 30 or 40 centimetres. From several experiments he concluded that the chestnuts could live thus long without communication with the soil, and that the elements necessary to their growth were obtained partly from the atmosphere, partly through endosmose from the woody tissue formed before decortication.—Among several French Academy papers are those by M. Jamin on the laws of the normal magnet, and M. Faye on circulation of hydrogen in the sun.—English and American science is also represented.—A curious fact is stated in the "Kleinere Mittheilungen": Herr Eimer has recently found, on a precipitous rock near the island of Capri, a new species of lizard It is blue all over, with dark spots on the back; while the lizards in Capri are of a bright green, with only a little blue at the extremities. Now the rock (which is frequented by birds of prey) has little or no vegetation, and its natural colour is a bluish grey, or dark blue in the shaded parts. The lizard, when at rest, can hardly be detected by sight, its colour is so like that of the rock. Herr Eimer finds indications that the rock was once connected with the land, and supposes green lizards to have gone over and been gradually transformed to blue, through natural selection.

THE *American Journal of Science and Arts* for June commences with a biographical notice of Dr. John Torrey, the botanist, who died in March last, in the 77th year of his age.—Mr. G. J. Brush contributes a paper on the analysis of an Anglesite from Arizona, worked out in the Sheffield Laboratory of Yale College.—Prof. Dana discusses some results of the earth's contraction from cooling, including the origin of mountains and the nature of the earth's interior.—Prof. J. H. Eaton has a paper on the relations of the sandstone, conglomerates, and limestone of Sauk County, Wisconsin, to each other and to the Azoic.—Prof. Le Conte replies to Mr. T. S. Hunt's criticisms on his paper on the formation of the great features of the earth's surface.—Mr. Verrill remarks on Mr. Jeffrey's article on "The Mollusca of Europe compared with those of Eastern North America," in which, while differing from that author, who thinks that most of the New World forms are derived from the old, he considers the reverse is the case—Prof. Young proposes the use of diffraction "gratings" as a substitute for the trains of prisms in a solar spectroscope; and he considers that they might well supersede prisms on account of their lightness and ease in management. Prof. Marsh gives further notices of Tertiary mammals, describing two new genera, *Tillotherium* and *Brontotherium*, allied respectively to *Anchippodus* and *Titanotherium*.

Bulletin Mensuel de la Société d'Acclimatation de Paris.—The April number of this serial has only just come to hand. It gives details of all the prizes in the gift of the Society for papers or works on matters in which it is specially interested, or for success in carrying out its objects in the acclimatisation or improvement of various animals or plants. No less than 88 prizes, of the money value of more than 75,000 fr. (3,000*l.*), remain to be competed for, besides 31 medals. By this means the Society does much to popularise the work it has in hand, and to make known the experience gained by those who have interested themselves in it. The system of lending specimens, on condition of receiving, for further distribution, a certain part of the produce, is explained in a paper by M. Passy, the vice-president. It appears that Algeria and Madeira, Guadeloupe and Martinique, besides Switzerland, Russia, Italy, Austria, and some other European countries, are brought within the field of the Society by means of branches, or affiliated societies of a similar nature.—A paper entitled "Le Jardin de mon Grandpère," by Edmond About, the George Augustus Sala of French literature, gives some idea of the benefits conferred by careful cultivation "To increase the resources given by Nature to man is a task at once too noble and too useful not to induce the sympathy and earnest assistance of people in all parts of the world." Such is the aim of the Society. The last year has had good results. Foreign countries have all been made to give their quota towards increasing the material wealth of France and the knowledge of those interested in the Society. "China, hitherto so unknown, will soon have no secrets from us. A work on the ichthyology of the Celestial Land has given details as to the modes of pisciculture in that country." The financial position of the Society is satisfactory, the balance-sheet for 1872 showing receipts 54,944 fr (2,200*l.*), and expenditure 45,704 fr (1,828*l*).

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 15.—On a Periodicity of Rainfall in connection with the Sun-spot Periodicity, by C. Meldrum, Director of the Meteorological Observatory, Mauritius. Communicated by Sir Edward Sabine.

Assuming that there is a sun-spot periodicity, in the course of which the sun undergoes a variation with respect to heat, or some other form of energy, we should expect to find a corresponding variation in the state of our atmosphere.

With this idea, it was some time ago determined to discuss the cyclones that had occurred during the last twenty-five years in the Southern Indian Ocean, and it was found, what had been often surmised, that they were more frequent and more violent in the maxima than in the minima sun-spot years

It is well known that the cyclones of the Indian Ocean are attended with much rain, which is not confined to the body of the storm, but extends over wide areas. Years remarkable for cyclones therefore, should be also years remarkable for rain; but to test this inference, with regard to the Indian Ocean, we had no rainfall statistics, except eighteen years' observations at Mauritius; and these were in every respect favourable, the rainiest years having been those in which cyclones were most abundant. In the absence of other data, the Brisbane and Adelaide rainfalls were consulted, and it was found that, like Mauritius rainfall, they indicated a periodicity. It was then surmised that there might be a rainfall periodicity generally; and that, if such was the case, both it and the cyclone-periodicity were concomitant effects of one and the same cause. This supposition having been strengthened by the results of an examination of the rainfall of England, it was resolved to examine all the rainfall tables (containing one or more sun-spot periods) that could be obtained By comparison of an extensive series of weather statistics kept at a large number of places all over the world, the decided conclusion is that, with scarcely an exception, all the years of maxima and minima rainfall are within a fraction of the corresponding maximum and minimum sun-spot year.

Chemical Society, June 19.—Dr. Odling, F.R.S., president, in the chair.—Nine communications were read, of which the following are the titles.—1. "Researches on the Action of the Copper Zinc Couple on Organic Bodies III. on Normal and iso-propyl iodide," by J. H. Gladstone, F.R.S., and A. Tribe, being a continuation, in the propyl series, of the author's previous researches. 2. "On the Influence of Pressure on Fermentation, Part 4 The influence of reduced atmospheric pressure on the alcoholic fermentation," by Horace T. Brown, in which he finds that, under diminished pressure, the progress of the alcoholic fermentation is retarded in a remarkable way. 3. "On Cymene from different Sources, optically considered," by J. H. Gladstone, F.R.S. 4. "Note on the Action of Bromine on Alizarine," by W. H. Perkin, F.R.S. This reaction gives rise to *Bromalizarine*, an orange-coloured crystalline substance, possessing feebler dyeing properties than pure alizarine, the colouring principle of madder. 5. "On some Oxidation and Decomposition Products of Morphine Derivatives," by G. L. Mayer and C. R. A. Wright, D Sc. 6 "On the Decomposition of Tricalcic Phosphate by Water," by R. Warington. 7. "Communications from the Laboratory of the London Institution, No. XII.": "On the Nature and on some Derivatives of Coal-tar Cresol," by Dr. H. E. Armstrong and C. L. Field.

8 "On a new Tellurium Mineral, with Notes on a Systematic Mineralogical Nomenclature," by J. B. Hannay. 9 "Note on the Relation among the atomic Weights," by J. A. R. Newlands. The president, in adjourning the meeting until after the recess, congratulated the members on the number and importance of the papers that had been read during the session.

Zoological Society, June 17.—The Viscount Walden, F.R.S., president, in the chair.—Mr. Sclater laid before the meeting the first sheets of a catalogue of the birds of the Neotropical Region, prepared by himself and Mr. Osbert Salvin, and shortly to be published under the title "Nomenclator Avium Neotropicalium." The number of species included in it, as known to the authors, was 3,565.—Mr. Sclater exhibited and made remarks on a collection of birds recently made in New Guinea by Signor D'Albertis. The most remarkable of them was a new Paradise bird belonging to the Epimachine Section, but peculiar for its long incurved bill, which was proposed to be called *Drepanephorus albertisi*, after its discoverer.—Mr. J. W. Clark exhibited the skull of a Seal from the Northern Pacific, which appeared to be *Halicyon richardsi*, of Gray, and explained his reasons for regarding it as indistinguishable from *Phoca vitulina* of the North Atlantic.—A communication was read from Lord Walsingham, giving particulars as to the distribution of the different species of Deer and other Ruminants of Oregon and Northern California.—Dr A. Leith Adams read a memoir on the osteology of the Maltese Fossil Elephants, in which was given the description of a large collection of remains discovered by him in Malta in the years 1860–1866. Dr. Adams referred these remains to two distinct species—a larger *Elephas mnaidriensis*, and a smaller—the *E. melitensis* of Falconer, and assigned *E. falconeri* of Busk to a smaller form of the latter species.—Mr. H. J. Elwes read a paper on the geographical distribution of Asiatic birds, in which he entered into the question of the best subdivision of the Indo-Malayan Region.—A communication was read from Mr. W. S. Atkinson, of Darjeeling, containing the description of a new genus and species of *Papilionidæ* from the South Eastern Himalayas, proposed to be called *Bhutanitis lidderdalii*.—Mr. R. B. Sharpe contributed the fourth of a series of papers on African birds. The present memoir dealt with the African Cuckoos, which were fully described and their geographical distribution pointed out.—Mr. R. B. Sharpe read a second communication, describing three new species of birds, proposed to be called *Macrodipteryx sperlingi* from the Bay of Malimba, West Africa, *Chamætylas princei* from the Gold Coast, and *Basa erythrothorax* from Celebes.—Mr. Sclater read a paper on the Curassows, based mainly upon specimens now or lately living in the Society's Gardens, and gave details on their geographical distribution and on the variations of sex of the known species.—A communication was read from Mr. R. Swinhoe on Chinese Deer, with notices of two new species proposed to be called *Cervus kopschi* and *C. euopis*.—Mr. Sclater read a note on the genus *Ornithion* of Hartlaub, and the synonymy of the four known species.—Mr. A. H. Garrod read a memoir on certain muscles of the thigh of birds and their value in classification, founded principally upon the examination of a large number of specimens that had lived in the Society's collection. This meeting closes the Scientific Session 1872–73.

Anthropological Institute, June 17.—Prof. Busk, F.R.S., president, in the chair.—Mr. J. G. Waller exhibited a series of bronze implements discovered on the site of an ancient camp near Hythe, Kent; and Mr. J. E. Price exhibited pottery and bones of *Bos* found at New Southgate.—Lieut. C. S. Holland read a paper on "The Ainos." The following papers were also read:—Account of an interview with a tribe of Bushmans in South Africa, by G. W. Stow, F.G.S.—Specimens of native Australian languages, by A. Mackenzie.—A brief account of three microcephales, by Dr. John Shortt.—On a patoo-patoo from New Zealand, by Sir Duncan Gibb, Bart.—The healing art in the North of Scotland in the olden time, by Rev. Walter Gregor, M.A.—On a hypogeum at Valaquil, Isle of Uist, by A. Carmichael.—Heathen ceremonies still practised in Livonia, by the Baron de Bogouschefsky.—The westerly drifting of nomads from the 15th to the 19th century, Part XI.—The Bulgarians, by H. H. Howorth.

Entomological Society, June 2.—Sir Sidney S. Saunders, V.P., in the chair.—Mr. Muller exhibited a remarkable *Psyche* case, sent by Mr. Rothney from Calcutta. It was composed of thorns, all of equal length (about 1¼ inches), arranged with the points all in one direction, so as effectually to guard the entrance against an enemy.—Sir Sidney Saunders exhibited a series of living Hymenopterous larvæ and pupæ in briar stems lately received from Albania. These stems having been split, showed the occupants in their natural cells. Specimens of the perfect insects reared from the larvæ were also exhibited.—Mr Muller communicated some notes on the discovery by Dr. Joly, of Toulouse, of a nymph belonging to the genus *Oligoneuria*, the immature state of which had been hitherto quite unknown. Drawings of the upper and under sides of the nymph accompanied the notes.—Mr. Wollaston communicated a valuable paper "On the genera of the Cossonidæ." It comprised (1) a catalogue of the several groups, arranged systematically and tabulated; (2) full generic diagnoses, taken seriatim; (3) Observations (diagnostic and geographical) on each separate genus; (4) brief characters of 139 species not hitherto recorded; (5) a complete list of the particular members of the family (amounting in all to 253).—The Secretary read a letter he had received from Mr. Roland Trimen, of Cape Town, containing some remarks on the Rev. R. P. Murray's "Notes on Variations of Neuration observed in certain Papilionidæ," published in the Proceedings of the Society in November last, and referring certain cases of variation to reversion to ancestral characters, pointing to a remote community of origin between the Papilionidæ and the higher Heterocera.

Berlin

Geographical Society, June 7.—Baron Richthofen, president, in the chair.—Dr. Neumayer spoke on methods of measuring the temperature of the water of the sea at great depth, and a new instrument for that purpose, invented by himself. The discovery of the fact that the bulb of an ordinary mercurial thermometer does not indicate correctly the temperature when subjected to the pressure of many atmospheres such as prevails at great depth, and that the errors of any single reading may react as much as 12 degrees of Fahrenheit, first led to the improved method of surrounding the bulb with a larger one filled with alcohol. The thermometrical errors, so far as they relate to the working of the instrument itself, are thereby nearly abolished. The difficulty, however, remains of ascertaining the point in the scale which the column of mercury reaches at any required depth of water. The various methods devised for overcoming it are chiefly directed towards the introduction of means for indicating the maximum and minimum points. No one of them fully answers this purpose. Any further improvement must therefore have for its object the reading of the thermometer while under water. A step in this direction was made by Mr. N. Siemens, but it was argued that the results arrived at by this method are not satisfactory, although it may eventually be improved. Dr. Neumayer's new principle is based upon the plan of devising a self-registering thermometer which may be lowered into the sea, and his first object was to find out a kind of light which should be able to do photographic work and yet not create errors by producing heat. The Geissler tubes answer these conditions, chiefly those filled with nitrogen, which emit a bright light and do not affect the temperature in any measurable degree. The new apparatus, which was exhibited and experimented with, consists of a large vessel of brass containing (1) two vertical thermometers, which perforate the bottom and protrude into an open compartment underneath, free to the access of water; (2) a galvanic battery, with two Geissler tubes inserted, running in front of, and close to, the thermometers; (3) two rolls of Talbot paper standing upright and immediately back of the thermometers, and revolving by means of a clockwork. As soon as the batteries are closed and the clockwork wound up, the luminous columns of the nitrogen cause the picture of the column of mercury to be reproduced on the photographic paper behind, together with all the lines marking the partition of the scale. The vessel is shut hermetically and lowered into the sea to any required depth. When raised again, the record of the temperature which the surrounding water had at any minute, and therefore at the particular depth to which the apparatus was then lowered, is read distinctly on the paper. An additional improvement was made by attaching on the top of the instrument a compass-card turning freely around its axis, and on the outside of the vessel a sort of wing, which will be directed by the current when the ship is in a slight motion. By an ingenious contrivance the deviation of the direction of the wing from the north and south line of the card is indicated by the same photographic means. It is believed that the direction of the current at various depths will thus be determined.—Mr. Siemens proposed to use chased copper in the place of brass in constructing the vessel, on account of its offering greater resistance to pressure, and believed to have already found satisfactory means for improving the instrument invented by himself and his brother.—Dr. Mauthe gave an account of Khiwa based on the study of Russian literature on the subject, winding up with the suggestion, that the withdrawing of a large body of the water from the Amu for the irrigation of the oasis, deprived the lake Aral of so large a supply, that to this circumstance might be due the diminution its surface has suffered, and the fact of its present isolation. The water which before took its way through lake Aral to the Caspian, now evaporates from the rice-fields of Khiwa.

Geological Society, June 4.—Dr. J. Ewald in the chair.—Baron Richthofen drew attention to the activity recently displayed, according to newspaper reports, by several volcanoes of Japan, some of which have not been active for a long time, and gave an account of the distribution of volcanoes in Japan. The west and east portion of the aggregate body of the Japanese islands (leaving out of consideration the small inland passages), is in every way the direct continuation of the mountain system which occupies the south-eastern portion of China, the axial chain of which extends from the frontier of Annam to the island of Chusan, in the direction of W. 30° S.; E. 30° N. It is accompanied on either side by a number of parallel chains. The prolongation of the main portion of this group of linear chains passes through the island of Kiushiu to the great bend of Japan; and in that entire region of country, the structure of the hills, the rocks of which they are made up (chiefly Silurian and Devonian strata accompanied by granite), and the lines of strike are the same which were observed in south-eastern China. This first system is intersected, at either end, by another which runs SS.W., NN.E. On the west, it commences in Kiushiu, and extends southward in the direction of the Liu-Kiu islands, while on the east it constitutes the northern branch of the main island, and, with a slight deviation in its course, continues through the islands of Yesso and Saghalin. A third system, which does not properly belong to Japan, is indicated by the S.W. and N.E. line of the Kuril islands. The first system, where it occupies the breadth of the country for itself alone, is as free from volcanoes or any accumulation of volcanic rocks as it is in south-eastern China. The second is accompanied by volcanoes. But the greatest accumulation of volcanic rocks, as well as of extinct volcanoes, is found in the places of interference, or those regions where the lines of the two systems cross each other; and besides, in that region where the third system branches off from the second. To the same three regions of interference those volcanoes are confined which have been active in historical times. Some details were then given regarding the structure of Kiushiu. This island, although having its longer axis directed from north to south, is intersected, as it were, by several solid bars made up of very ancient rocks, and following the strike of W. 30° S., E. 30° N. They form high mountain barriers, the most central of which (south of the provinces of Higo and Bungo) rises to over 7,000 feet, and is extremely wild and rugged. Among the details regarding the volcanoes of Satsuma, particular attention was drawn to the fact that the various families of volcanic rocks have arrived there at the surface in exactly the same order of succession as is the case in Hungary, Mexico, the Great Basin, and many other volcanic regions, namely, 1st, Propylite, or trachytic greenstone; 2nd, Andesite; 3rd, Trachyte and Rhyolite; and 4th, the basaltic rocks. There is the greatest accumulation of mountain masses in Japan, one of the several chains rising to upwards of 11,000 feet in its summits. Among them are situated several gigantic volcanoes, such as Fusi-yama, the highest of all, Yatsunga-Jake, a series of elevated cones with extinct craters, and several others partly active and partly extinct. Those of the third group were not visited by Richthofen.—Prof. E. Weiss exhibited some curious octahedral crystals of Hausmannite, remarkable on account of certain re-entering angles and the striated aspect of the faces, and proved that the lines which caused this appearance were due to a kind of twin formation not hitherto observed.

Paris

Academy of Sciences, June 23.—M. de Quatrefages, president, in the chair.—The following papers were read:—Second note on guano, by M. Chevreul.—New researches on the silent electric discharge, by MM. P. and A. Thenard.—Researches on chlorine and its compounds, by M. Berthelot. The author dealt with the compounds of chlorine with water and the protosalts.—A new series of observations on the solar protuberances; new remarks on the relations between protuberances and spots, by Father Secchi. The Rev. Father presented his observations for the last quarter, and then, in his letter, criticised Respighi's late remarks on the absence of the chromosphere over spots, which he maintains is not the case. He then gave an account of some experiments on sodium vapour, which, however, contained nothing new, and then proceeded to state that the line D_3 appears to him to coincide with one of the components of the D group which appears when the sun is near the horizon. He has also found a bright iron line between b_2 and b_3, and having examined the spectrum of iron with a battery of 50 cells, has seen 480 lines, but could not find 1474 Kirchhoff; he hopes to repeat this experiment, and if the results are same, he considers that the absence of Fe from the corona will be proved. With magnesium in the lamp, he finds the same nebulosity as is exhibited by the sodium lines, but it is accompanied by a banded spectrum of MgO; he thinks that if the nebulosity is also due to the oxide, that the occurrence of oxidation in the sun will be proved.—On the influence of atmospheric refraction as it affects the time of contact in a transit of Venus, by M. E. Dubois.—On the coloration and greening of *Neottia Nidus-avis*, by M. E. Prillieux.—On semi-diurnal barometric variations, by M. Broun.—On hot-air warming apparatus, by M. Ducrot.—A letter was received from M. de Lesseps praying the Academy to include his name among those of the candidates for the vacant seat of Académicien libre, vacant by M. de Verneuil's death.—On the constitution of the sun and the theory of the spots, by M. E. Vicaire.—On the production of methylic alcohol by the distillation of calcic formate, by MM. C. Friedel and R. D. Silva. The authors believe that formic aldehyde is first formed by the reaction $(CHO_2)_2Ca = CO_3Ca ? + H_2O + CH_2O$, and that the aldehyde is converted into alcohol by the action of nascent hydrogen.—On terebene, by M. J. Riban.—On the production of the rotatory power in the neutral derivatives of mannite, by M. G. Bouchardat.—An answer to a late note, by M. du Moncel, on the resistance maxima of induction coils, by M. Raynaud.

DIARY

FRIDAY, July 4.

Geologists' Association, at 8.
Archæological Institute, at 4.
Horticultural Society, at 3.—Lecture.

SATURDAY, July 5.

Geologists' Association.—Excursion to Plumstead and Crossness.

MONDAY, July 7.

Geographical Society, at 8.30.—Boat Journey up the River Wami: C. C. Hill.—Remarks on Zanzibar and the East Coast of Africa: Sir Bartle Frere, K.C.B., president.
Entomological Society, at 7.

BOOKS RECEIVED

American.—Families of Fishes: Theo. Gill (Smithsonian Institution).—Memoir of Sir Benjamin Thompson, Count Rumford, 2 vols: George Ellis (Claxton & Co., U.S.A.).—U.S. Sanitary Commission in Valley of Mississippi, 1861–6: Dr. Newberry (Cleveland, U.S.A.).—Geological Survey of Indiana: E. T. Cox (Indianapolis, U.S.A.).

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THURSDAY, JULY 10, 1873

THE ENDOWMENT OF RESEARCH

II.

IN a recent number attention was drawn to the public importance of original research in the Sciences, and it was insisted that certain funds which lie ready to the hand should be devoted towards the maintenance of those who undertake the national duty of extending the bounds of scientific knowledge.

In this article it is proposed to strengthen those positions by a reference to the already published evidence of the Royal Commission at present inquiring into "Scientific Instruction and the Advancement of Science." The object of the labours of the Commission is twofold, but concerning the former part nothing need now be said, except that regulated activity in independent investigation is the main condition upon which depends successful teaching alike in the individual professor, and the scientific schools of the nation.

The Commission was especially directed to ascertain how far the endowments of the Universities and Colleges might be directed to aid the needs of Science. On this point much valuable evidence was given by several distinguished members of our two wealthy Universities, and there was a general agreement of opinion that so far as Instruction and Examination are concerned, the Universities are showing a praiseworthy disposition to encourage their scientific students. On the other hand, it was universally admitted that the Oxford Science-school, despite the excellent teaching of its professors, is not progressing so well as might be expected, and that the University is lamentably deficient in that part of its functions which is concerned with the promotion of knowledge for its own sake.

Among the Oxford witnesses Sir B. Brodie, who was at the time that he gave his evidence Waynflete Professor of Chemistry, is conspicuous as well for the precision with which he pointed out the causes of the present defects, as for the definiteness of the scheme by which he proposed to remedy them. According to him, "Universities are Institutions of which the object is, in the first place, to promote scientific education and to diffuse scientific knowledge, and in the second place to preserve and to extend scientific knowledge." He was of opinion that "the latter of these duties is at present not sufficiently kept in view, whereas in old days the case had been different." His suggestions were that "the University should establish, on a larger scale than now, museums and scientific collections, for the present ones are organised too much with a purely educational object; and secondly (a point to which he attached by far the most importance), that the means of existence and of scientific study should be provided for certain professors or individuals, by whatever name they may be called, whose chief function should be scientific investigation and the representation and advancement of their various special Sciences."

He further went on to suggest that "these professors should be, to a great extent, separate from the ordinary teaching staff of the University, professors of the Science itself, rather than professors of the teaching of the Science:" that "in their lectures they should give to the public what they have attained for themselves, and have under them a limited number of pupils as assistants in their own original researches." The case of Liebig at Giessen will naturally suggest itself to our readers as an apt illustration of the particular mode of advancing Science here advocated; and from the evidence of Sir W. Thompson before the Commission it may be learnt that both at Glasgow and at Owens College a somewhat similar plan is being energetically carried out.

Sir B. Brodie, however, would appear with characteristic zeal to go even one step beyond this, for he instances as "a capital example of such a foundation as he would desire the Radcliffe Observatory at Oxford, where the observer gives no lectures at all, is not even attached to the University, but solely put there to do astronomical work. The Board of Curators, themselves not necessarily members of the University, having large funds at their disposal, give to the observer whatever he wants, whilst he on his part, as the sole evidence of his industry, makes an annual report on the condition of the observatory and the work done, and publishes certain tables." Here we also think that we have found, so far as the theory of the institution goes, an admirable model of the manner in which the cultivation of Science for its own sake may be endowed with great advantage to the country and without any manifest risk of sinecurism. In the language of the Dean of Christ Church, "we should very much like to see eminent men residing at Oxford only partially employed in teaching, but employing a great part of their time in scientific research."

With reference to the endowment of research here advocated it is necessary that a warning should be explicitly given against dangers which threaten from two different sides. On the one hand it is most important, in England more than in other countries, that the simple pursuit of Science as knowledge should not be confounded with the practical application of scientific truth to the numberless arts of modern civilisation. Applied Science is a profession which promises to become of a highly remunerative character. The analyst, the engineer, and the electrician may require pecuniary help and regulation from the Central Government for their technical schools, but they emphatically do not require to be themselves supported by national endowments. On the other hand, the ordinary scientific teacher at the universities, where not the poor but the rich as a rule are taught, should not in our opinion be regarded *quâ* teacher as the proper recipient of the funds of an endowment. It may very well be that while education in Science is struggling towards recognition, the teachers may claim some sort of aid to put them on a level with those branches of instruction which have the advantage of ancient prestige; it may also be thought advantageous that certain teachers should receive endowments, not for the tuition they give, but for the investigations they are carrying on independently of their other work; yet it must be granted that either of these cases is of an exceptional character.

On all hands are to be seen the disastrous consequences of endowing teaching proper, and of compelling original research to take its chance at the hands of the amateur. It must happen that the professor (so called) will be constrained to give up the whole of his time to the duty which is most expected of him, and that original research will suffer both in quantity and in quality. The most general principles of political economy are sufficient to show that in a wealthy and moderately enlightened country the remuneration of teaching had better be regulated by the equitable standard which impartial competition will not fail to establish. It is for those subjects which, though of essential importance to the welfare of the country, are in themselves naturally unremunerative, that the old endowments for the promotion of education and knowledge, whatever may have been the particular means by which these ends were originally to be attained, are now required. Among these subjects disinterested application to pure Science is manifestly the chief.

In a subsequent article we propose to show that the funds of the Colleges cannot be more consistently applied than to this purpose, and that the progressive well-being of the Universities mainly depends upon the degree to which they are concerned in the advancement of knowledge. C.

THOME'S LEHRBUCH DER ZOOLOGIE

Lehrbuch der Zoologie. Von Dr. Otto Wilhelm Thomé; Pp. 416. (Brunswick: 1872.)

IF Germans wonder, not without reason, who buy our manuals of microscopic mounting, Englishmen may equally wonder for whom such books as Dr. Thomé's are written. We have technical treatises on special branches of zoology, and we have popular natural history books, but a manual like this would find a poor sale in England. It is a school manual, and its existence is explained by the introduction of zoology to some extent into the curriculum of the German gymnasia and much more into that of the Realschule, which more or less correspond to the "modern side" of our public schools, or may be described as answering in intention, though of course immeasurably superior in performance, to English "commercial schools." Whether zoology ought to form a regular part of school work, even where room is made by giving up Greek altogether and Latin more or less, is an important question. As a part of *education* in the proper sense of the word, it is so inferior in exactness, in conciseness, in facility of demonstration, and convenience for observation and experiment to such rivals as botany, physics, and even chemistry, that its claims may practically be ignored. Moreover, looking at school work from another point of view, it is obvious that any scheme of utilitarian *instruction* which is good for much must include ignorance of the greater part of human knowledge, in order to provide for acquaintance with the rest; and the first addition to the indispensable elements of reading, writing, and arithmetic would probably be claimed for geography, political economy, or the rudiments of hygiene, as more useful branches of knowledge than zoology. A boy with a bent for natural history would gain far more good from reading the bits of zoology in such books as the "Voyage of the *Beagle*," the "Malay Archipelago," or "Kosmos," and by collecting bird's eggs or butterflies, than he would by painfully wading through the details of Dr. Thomé's closely printed pages. And when zoology is taken up as a serious study by older students, most teachers will agree that the best plan is for them to begin by a careful study of a particular branch of the subject, with the help of such a handbook as Flowers' "Osteology of the Mammalia."

Looking to the object of the book, the reader will find Dr. Thomé's work fairly done. The first hundred pages are devoted to a popular sketch of human anatomy and physiology, from which all notice of generation and development is excluded. Otherwise it is as complete as the space will allow. The remainder of the book describes the several classes of animals, beginning with Mammalia and following the arrangement into seven types—Vertebrata, Mollusca, Arthropoda, Vermes, Echinodermata, Cœlenterata and Protozoa—which is now generally accepted among German naturalists. A diagram of these types is given, which might serve for a genealogical tree; but no hint of this intention is given. The sub-division into classes and orders is not particularly good. Thus among Mammalia the Sirenia are confounded with the Cetacea, Ray's obsolete distribution into Ungulata and Unguiculata is preserved, and the orders Ruminantia and Pachydermata appear, as if nothing had been done to clear up the real affinities of these groups since Cuvier published the "Regne Animal." The classification of birds is not more unsatisfactory than that of other writers; and in the class of fishes Müller's orders are commendably followed. Tunicata and Bryozoa are of course excluded from Mollusca, and help to fill the lumber-room of Vermes. A very large share is, as usual, given to the account of insects, while marine zoology and the Protozoa receive comparatively little attention.

Three hundred and fifty-eight woodcuts make an important feature of the work. Most of these are good in themselves and well printed. Those illustrating human anatomy and histology are the best, and almost all borrowed from Henle. No indication of this or any other source is given, but it is easy to recognise that some of the figures have been taken from the admirable cuts in Bell's "British Reptiles," others from Forbes, Milne-Edwards, and other well-known works; while some of the Mammalia appear to have been drawn from children's toys. Fig. 350, of a sponge, is a curiously modified reproduction of the original drawing in Grant's "Outlines of Comparative Anatomy" (p. 312). Of the thirty-one figures of birds, twenty-seven represent European species, and of these all but four are copied from Yarrell's British Birds. One excellent addition to each figure is a note of the relation it bears to the actual size of the animal represented, or of the average length of the latter. There are not many figures of anatomical details, but almost all are good, some being taken from Gegenbaur's "Vergleichende Anatomie."

To compare Dr. Thomé's book as a whole with serious scientific treatises even of the second class, like that of Claus, would be unfair: but even as a "cram-book" it is inferior to Nicholson's Zoology: and it gives far too little space to descriptions of the habits and character of well-known groups like mammals, birds, and insects, to be really popular. Such books as Knight's "Museum of Animated Nature" are much more interesting and quite as scientific. P. S.

VALENTIN'S QUALITATIVE ANALYSIS

A Course of Qualitative Chemical Analysis. By William George Valentin, F.C.S., Principal Demonstrator of Practical Chemistry in the Royal School of Mines and Science Training Schools, South Kensington. (London: J. and A. Churchill. 1873.)

IT is a good sign of the present activity of scientific study in this country that there should have already been a call for a second edition of a work which only appeared two years back, in the early part of 1871.

The author has, in the second edition, separated the second part of his orignal work, and this, treating entirely of qualitative analysis, forms the volume now before us. The elements which occur in the main as bases are divided into five groups, and the first portion of the book is devoted to a careful study of each element of each group beginning with group V., a method the advantages of which will be seen by a very short study. The first 103 pages are devoted to this matter, and the attention of the student is then devoted to the study of the reactions of the acids. No particular grouping is here attempted, the acids being simply taken under the head of the principal element of each, *e.g.* sulphuric acid is followed by sulphurous acid, and that by hyposulphurous and hydrosulphuric acids. We remark here, by the way, that the polythionic acids are dismissed with the notice that they must be reserved for a more extensive course of study. A few of the more common organic acids are then referred to, and the whole matter treated of is shown in the condensed form as tables. In these we notice no important alterations from those of the edition of 1871, and of them we can, after considerable experience, speak in the highest terms, students soon learning to use them with great accuracy and despatch.

Mr. Valentin has stated in his preface that he purposely omits considering the rarer elements in his tables. In this we cordially agree with him as regards the tables intended for students, but we cannot help wishing that Mr. Valentin had put in the appendix some analytical information with regard to these bodies in a tabular form; as we feel sure that his great experience in the analysis of every possible kind of body would have enabled him to give valuable information to many who are compelled occasionally to make diligent search for elements which are not always met with. Many old students of the College of Chemistry will recognise an old friend on pp. 50 and 51 in the alternative table for group IIIA., it being no other than the old table used there up to the time of the introduction of the newer methods given at the end of the book.

We notice with pleasure that the analytical tables are published in a separate form, printed on De La Rue's parchment paper; this is certainly very good news for chemical students who have to use them. Who does not know the gradual process of obliteration and destruction by acids and alkalies which gradually, but surely, rendered his most carefully prepared and written analytical tables useless. It would be a great boon to all compelled to use books in the laboratory, if some modification of this material could be used for binding them. In conclusion we can strongly recommend the book to anyone desiring either to get or to give a thorough grounding in analytical chemistry; and the only fault we can find with it is that rather too profuse use is made of symbolical formulæ, for they are scarcely required in a book on analytical subjects only, and the first volume gives quite a sufficient amount of information on their use and nature. We hope that Mr. Valentin will some day give us a quantitative analysis.

R. J. F.

OUR BOOK SHELF

Celestial Objects for Common Telescopes. By the Rev. T. W. Webb, M.A., F.R.A.S. Third edition, revised and enlarged. (London: Longmans, Green, and Co, 1873.)

POSSESSORS of what Mr. Webb calls "common telescopes," will be pleased to have another edition of this most useful adjunct to their instruments, with corrections and additions up to the present time. Now that silvered glass reflectors are so cheap, and apertures little below six inches not uncommon in the hands of amateur astronomers, the author's definition of a common telescope is probably too limited, but these limits are extended as we proceed with the book and find mention of objects barely visible with nine inches. The advice on the use of telescopes, and the mode of observation is sound and good, and too much stress cannot be laid on the necessity of a good solid stand; a good telescope will be absolutely useless with an unsteady mounting. The description of the various phenomena to be viewed in the members of the Solar system may lead possessors of small telescopes to expect too much, the separation of Saturn's rings, the markings on Jupiter's satellites, to wit, although mention is made of the apertures required to view the features mentioned; but this may also make the book useful for work with larger instruments. We must take objection to the great contrast of light and shade, as is often the case in other works, in the cuts of Venus and Jupiter's moons, the dark markings on Venus being infinitely too black, they in reality being only just visible, with first-rate instruments, to a practised astronomer. Drawings of this kind only represent position and shape, but it must be remembered that an amateur expects to see through the telescope exactly what he sees in a drawing. One-third of the book is taken up with a selection of double stars and nebula, as in the former editions, with measures of position and distance up to later dates. Altogether the book will be found most useful to every incipient astronomer, but perhaps there may be too strong a tendency to star-gazing induced by it, and we should have been more gratified to have seen directions to readers having telescopes of certain sizes how to make their observations of real use and not a mere pastime. For instance, double image micrometers can be used on less apertures than 6-in. without clockwork; and some instructions in the use of them, and in reducing their observations so as to show the motions of binaries, would be of great service in teaching amateurs to do useful work; a hint, also, on drawing the ever-changing belts of Jupiter, any extraordinary spots on the sun, the larger nebulæ, and last, not least, the star clusters. As soon as amateurs have seen the planets and a few double stars, they should begin to make themselves useful, otherwise they soon get tired of the mere star-gazing and the telescope becomes to them a thing of the past.

G. M. S.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Dr. Sanderson's Experiments and Archebiosis

IN last week's NATURE Dr. Sanderson expresses some surprise that I was gratified by the facts recorded in his previous letter. My reasons were these. Dr. Sanderson's experiments in the eight successive cases in which he employed the temperature of 100° C. for twenty minutes were entirely confirmatory of my own, and were, moreover, so conducted as to refute the objections which have been urged by Dr. Wm. Roberts and others.

As to the bearing of Dr. Sanderson's experiments with higher temperatures and more prolonged periods of exposure to heat upon the general question of the independent origin of living matter, I wholly dissent from his now expressed conclusions, for the following reasons:—

In the first place his fluids were not kept sufficiently long before they were submitted to microscopical examination. Dr. Sanderson is quite mistaken in supposing that in examining his liquids within 3–6 days after their preparation he was following my method—more especially in cases such as these where the fluids have been exposed to temperatures higher than usual, or to 100° C. for upwards of twenty minutes. Three to six weeks have often elapsed before I thought it judicious to open my flasks (*See* "Beginnings of Life," vol. i. p. 355, p. 441, and Append. C.). In opening all his flasks at the end of 3–6 days, Dr. Sanderson lost the opportunity of watching the changes which might have ensued later in many of his experimental fluids—and hence lost his right to draw any conclusions from these abortive trials.

Secondly these experiments are open to another objection. Dr. Sanderson concludes from them that exposure to a temperature of 101° C. almost always arrests the tendency to fermentation in his experimental fluids. This conclusion I believe to be erroneous, because in the former series of experiments which I performed in his presence, and of which he recorded the results in your pages (NATURE, vol. vii. p. 180), fermentation occurred in the majority of cases in fluids which I have very good reasons for believing to have been raised to a temperature of 103·33° C.* The method recently employed by Dr. Sanderson for superheating his flasks was needlessly complicated, and the exact temperature to which they had been exposed was known only by inference—never by direct thermometric observation.

Leaving now the discussion of the experimental facts I come to the examination of Dr. Sanderson's inferences, which seem still more open to objection.

Dr. Sanderson, in common with most others, had up to the date of his witnessing my experiments, admitted that Bacteria and their germs were killed in all fluids with which he had experimented at the temperature of 100° C. (see "Thirteenth Report of Medical Officer of Privy Council, 1871.") It was, indeed, this conviction which inspired himself, and many others, with a strong disbelief in the results which I obtained with previously boiled infusions.

What remains, then, for Dr. Sanderson to do, prior to drawing inferences such as he now expresses, is to ascertain, by direct examination, whether the temperature of 100° C. is or is not fatal to the life of Bacteria. It is upon this that the interpretation of my results can alone depend. I have already contributed my share to the inquiry by several long series of experiments, each of which has led me to the same conclusion, viz., that Bacteria and their germs, when in the moist state, are killed at a temperature of 60° C. (See "Beginnings of Life," vol. i. p. 325-333; "Proceedings of Royal Society," No. 143, 1873; and another paper about to appear in the next number of the "Proceedings.") It is for Dr. Sanderson, or any competent observers who are sufficiently interested, to examine my experiments and results on this part of the subject, or else to devise others for themselves having a similar bearing.

If I am right in believing that 60° C. is the thermal death-point of Bacteria in the moist state, the conclusion which must be drawn from the now admitted results occurring in fluids which have been heated to 100° C. suffice for my argument as to the reality of Archebiosis. The further investigation of the results of raising fluids to higher temperatures for protracted periods is of great interest, but does not at all affect the question of the reality of Archebiosis; and Dr. Sanderson's present experiments have, therefore, none of the significance in the argument which he strangely enough appears to claim for them.

Briefly, having admitted that Bacteria arise in fluids which have been submitted to a temperature of 100° C., it is for Dr. Sanderson to show that they are not killed in fluids at 60° C., as I maintain that they are, before he can attempt with any effect to draw inferences of his own, or to criticise those which I have drawn on the subject of the independent origin of living matter.

H. CHARLTON BASTIAN

University College, July 7

Dr. Bastian's Experiments

REGARDING Dr. Bastian's letter in NATURE of June 26, I am happy to be able to make a note of an experiment which is of interest and importance. I sealed a tube on to a flask of about 100 cc. capacity at right angles to the neck, and drew out the end so as to form a capillary orifice. About 30 cc. of water were put into the flask, and a thermometer in an india-rubber cork was wired into the neck. On boiling the water the steam had not issued during more than half-a-minute, before the temperature was 102° C., and in less than ten minutes it had reached 118° C.; fearing the safety of the apparatus, I did not proceed further, nor indeed did I wish to do more. The joint experiments of Drs. Sanderson and Bastian, then newly published in your paper, led me to this. My view being that Pasteur's experiments on milk, mixed with carbonate of lime, and the liquid known as "Pasteur's solution" mixed with carbonate of lime, conclusively show that liquids which ordinarily develop *Bacteria*, will, if they remain neutral after boiling at 100° C. also develop these organisms: raise the temperature to 110° C. and the *Bacteria* no longer show themselves.

Thus believing, I concluded that the absence of *Bacteria* in some of Drs. Sanderson and Bastian's flasks in which were placed neutral or only slightly alkaline infusions, was probably due to the liquids being heated above 100° C., by boiling in vessels with capillary orifices. That my supposition was correct is more than likely; in fact experiments with infusions confirmed it. That an aqueous solution may so easily be raised to 118° C. is a point in chemical manipulation which will be turned to advantage in the laboratory.

King's College, June 30 WALTER NOEL HARTLEY

Temperature and Pressure

THE climate of the island of Jamaica is remarkably uniform, not only at the sea-level, but also at places having the same elevation, so that the connection between temperature and elevation, or barometrical pressure due to that elevation, is easily obtained; and since the surface of the island is broken up by innumerable radiating and intersecting mountain ranges, among or upon which the houses are scattered, this connection becomes one of the most important features in its meteorology; but what renders it especially interesting, however, is the fact that the rate of the decrease of temperature in ascending the hills in this tropical climate is equal to the average rate of decrease found by balloon ascents made in England, as far as the irregularities of the results obtained from those ascents will allow us to judge.

In order to show that this is the case, let t_0 be the temperature at any place where the pressure is p_0, the temperature being expressed in degrees of Fahrenheit's scale, and the pressure in inches of mercury at 32°; let t and p be the corresponding quantities at any other place above the former; then if λ be constant and equal to 3°·23, the equation

$$t_0 - t = \lambda(p_0 - p)$$

will represent the connection between temperature and pressure; or in words, for every inch the barometer may fall, the thermometer will fall 3°·23.

If we take mean annual values, at Kingston $t_0 = 78^\circ{\cdot}8$, $p_0 = 29{\cdot}97$in.; and at Newcastle, the garrison of the white troops, $t = 67^\circ{\cdot}0$, $p = 26{\cdot}31$in.; so that $\lambda(p_0 - p) = 11^\circ{\cdot}8$, which is exactly equal to the observed difference of temperature.

Again at Craigton, the residence of his Excellency the

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* Dr. Sanderson was not aware of this fact, and says he does not know any means by which the temperature of a fluid boiling briskly in a vessel from which the steam escapes only through a capillary orifice, could be accurately estimated. The method which I adopted some months ago seems to possess this merit. I had a small maximum thermometer made for the purpose, 2½ in. in length, and graduated from 95°–115° C. Having straightened the neck of one of my retorts (capable of holding about two fluid ounces), it was filled with some hay infusion and the thermometer was introduced in such a way that its bulb remained in the midst of the fluid, about three quarters of an inch away from the glass. The long neck of the retort having then been drawn out and broken off (so as to leave the usual capillary orifice) the fluid was boiled for five minutes before the vessel was sealed. The thermometer was found to stand at 103·33° C. The retorts employed in my previous experiments with Dr. Sanderson were of the same size, and their contained fluids were boiled under precisely similar conditions. If larger flasks, containing more fluid, were employed the temperature would doubtless rise to a still higher degree owing to a corresponding increase in internal pressure.

Governor, which is between the two former places with respect to both position and elevation, $t = 70^{\circ}\cdot5$, $p = 27\cdot41$ in., from observations kindly made for me by Captain Lanyon, A.D.C.; so that the calculated difference of temperature between Kingston and Craigton is 8°·3, the observed difference; and the calculated difference between Craigton and Newcastle is 3°·55, which is only 0°·05 too large. And since the equation has been found to hold good under different circumstances at lower elevation, we may suppose that it is strictly true for Jamaica.

With regard to balloon ascents, I have before me two tables, one compiled by Sir John Herschel, and the other by Prof. Loomis, from more recent observations, and these are brought into the same form in the following table in order to compare them; the first column contains the fall of the barometer in inches, the second contains the corresponding fall of temperature from Herschel's Meteorology, the third from Loomis's Meteorology, and the fourth contains the mean of the numbers in the second and third, which we shall consider to be the average results obtained from balloon ascents.

1	2	3	4	5	6
$p_0 - p$	H	L	$t_0 - t$	Calc.	Diff.
in.	°	°	°	°	°
2	3·0	10·1	6·6	6·3	+ 0·3
4	6·8	17·3	12·1	12·6	− 0·5
6	11·3	23·2	17·3	18·9	− 1·6
8	16·9	29·0	23·0	25·2	− 2·2
10	23·6	34·7	29·2	31·5	− 2·3
12	31·4	40·5	36·0	37·8	− 1·8
14	40·8	46·3	43·6	44·1	− 0·5
16	51·8	51·7	51·8	50·4	+ 1·4
18	63·7	56·1	59·9	56·7	+ 3·2

Now if we take $t_0 - t = \ (p_0 - p)$, we shall get nine equations of condition for finding λ; the most probable value of this quantity is 3°·15, which hardly differs from the value found in Jamaica. Again, if we calculate $t_0 - t$ and employ this value of λ, we get the fifth column, and it will be noticed that the differences in the last column between the observed and calculated quantities are very small when we consider the great differences between the second and third column.

Therefore the equation $t_0 - t = \lambda\,(p_0 - p)$ holds good for about two-thirds of the whole atmosphere, and if it holds good for the remaining third, by putting $p = 0$, we shall obtain the difference between the temperatures at the lowest and highest strata of the atmosphere; this difference is about 94°, so when the temperature at the surface of the earth is 50°, the temperature at the superior limit of the atmosphere must be −44°.

Since the temperature falls 3°·15 for every inch the barometer may fall, or for every 945 ft. we may ascend (when that temperature is about 50° and the elevation low), the temperature in England will fall 1° for every 300 ft.; this has been always acknowledged, and we now see that it is a consequence of the *more general law which connects temperature and pressure* throughout the atmosphere.

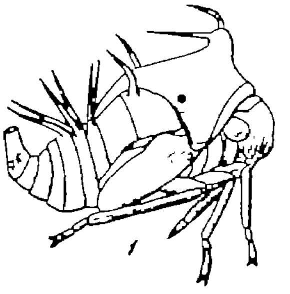
Now though we may suppose that λ has this value for all insular climates, yet it cannot have the same value for continental climates, on account of the higher temperature of the land; but still there is every reason for supposing that, at any given instant of time, λ is constant for all points in the same vertical line; and when it has been determined from the observed temperatures and pressures at any two points in that vertical, our equation becomes especially adapted for the barometrical measurement of the distance between them.

It only remains for me to say that I have already used the equation when making a series of observations among the hills in the north of England, and always found it true when the weather was settled, and sufficient time and care taken in obtaining the mean temperatures of the different strata of air.

Jamaica MAXWELL HALL

Larvæ of Membracis serving as Milk-cattle to a Brazilian Species of Honey-bees

THE connection between the ants and the Aphides has long since been generally known; in the proper season we always find ants very busy on those trees and plants on which the Aphides abound, and if we examine more closely we discover that their object in thus attending upon them is to obtain the saccharine fluid which they secrete from two setiform tubes placed one on each side just above the end of the abdomen, and which may well be denominated their milk (Kirby and Spence, "Introduction to Entomology," 7th edition, p. 335). It has also long been observed and described, that not only do the Aphides yield this repast to the ants, but also the Cocci, and that in the tropical regions of India and Brazil, where no Aphides occur, the ants milk the larvæ of several species of Cercopis and Membracis (Kirby and Spence, p. 336; Westwood, "Modern Classification of Insects," II. p. 434). Recently Prof. F. Delpino, of Vallombrosa, near Florence, observed the same connection between *Formica pubescens* and *Tettigometra virescens* ("Bolletino Entomologico," anno IV. Settembre 1872). But, as far as I know, it has never been observed hitherto that honey-bees also nourish themselves by the secretion of certain hemipterous insects. Hence the following observation, made some months ago by my brother, Fritz Muller (Itajahy, Prov. St. Catherina, Brazil) may be worth publishing.



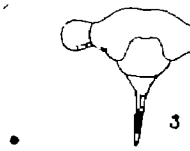


Fig. 1.—Lateral view of larva. Fig. 3.—Front view of head of imago.

Among the great number of species of Melipona and Trigona which, in the tropical and subtropical regions of America, as is known, occupy the place of our hive-bee, there is one small species of Trigona which has only once been found by my brother on flowers (of *Sicyos angulata*), and which seems to nourish itself in a very strange manner. He once found a multitude of them spread over the body, already strongly putrifying, of a large toad; the interior of the large open mouth of the toad was filled with these bees, probably sucking the putrid juice of the dead body. On another occasion he saw a great number of the same species of bees in the putrifying intestines of a hen. Repeatedly he saw them sucking the juice flowing out of trees.

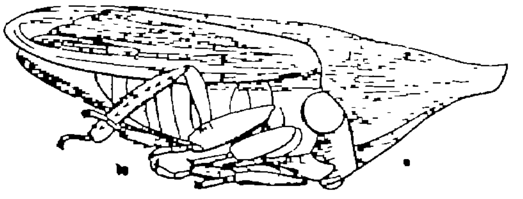


FIG. 2.—Lateral view of imago.

In *consequence of other observations this same species* of Trigona is supposed by my brother to suck the secretion of the larvæ of a certain hemipterous insect belonging to the genus Membracis, or to a closely allied one. As I do not precisely know the name of this supposed milk-cow, I here give the illustration of its larvæ and imago, drawn from specimens sent me by my brother.

He found the pedunculi of the flowers of *Cassia multijuga* pretty frequently occupied by societies of larvæ of this species closely crowded together. Amongst these larvæ there was present a great number of the above-mentioned Trigona, marching all the day long amongst and upon them. When taken between the fingers, the larvæ of Membracis immediately emitted a little drop of a limpid fluid from the upward bent tip of their abdomen—probably a sweet fluid, for the sucking of which the larvæ are visited by the Trigona.

Unfortunately the specimens of this Trigona, enclosed in a letter sent me by brother, arrived here quite broken, so as not to be determinable; but in a future number of this journal I hope to be able accurately to name both the supposed milker and the supposed milk-cow.

Lippstadt HERMANN MULLER

Free-Standing Dolmens

MR. LUKIS, in a paper recently read before the Society of Antiquaries, nominally "On certain Erroneous Views respecting the Construction of French Chambered Barrows," but really a method of criticising severely Mr. Fergusson's work on the "Rude Stone Monuments," states that it is an "*error*" to suppose that the Dolmens of that country were ever free-standing; in other words, he lays down the "rule," "there were no free-standing dolmens in France." The announcement that, with regard to monuments of whose fashioners we know absolutely nothing, a universal negative of this kind can be safely laid down as a law, would be startling, did it not come from one who is backed by such extensive inductive evidence as is Mr. Lukis. His "rule" was "established by the extreme rarity of the instances." This being the case, he calls those "in error" who would, from these instances, form a small class, or species of dolmen. As, in an essay on the Cornish sepulchral monuments, which you recently most kindly reviewed at length, I am committed to this latter view—one, by the way, which I had struck out for myself before the appearance of the "Rude Stone Monuments,"—will you kindly permit me to call your attention to one structure which I have ventured to place, and shall still venture to place, in the discarded class? I do so as a protest against the dictum of Mr. Lukis being extended to our British examples, before a careful scrutiny has been made of *every* monument of the kind from one corner of our islands to the other. On this single instance, such as it is, it must be clearly understood that I build no theory; it will be for others to judge whether it does not afford some evidence of the difference in construction and use of the dolmen or table-stone proper, and the kist-vaen cromlech; one thing only I will add, that, limited as my experience is to the monuments of Britain, I shall not be exposed to the temptation of explaining away any observed fact in order to reconcile a doubtful comparison. Without feeling that I am guilty of "dabbling in archeology," or of setting forth "any dogmatic expositions of hypotheses" (!!), or of "establishing my proposition from second-hand information," or in short of being the victim of any very "erroneous view" (all which faults Mr. Lukis finds in those who differ from him), I consider that the following facts justify my statement that the monument I am about to describe always was, as it is now, a free-standing dolmen.

At Lanyon, in the parish of Madron, Cornwall, stands a tripod dolmen, or cromlech, consisting of three slim pillars of unhewn granite supporting on their summits a horizontal stone over 40 ft. in circumference and averaging 20 in. thick. In 1815 it fell; but previous to its fall a man on horseback could sit upright underneath the cap-stone. In 1824 it was again set up; but two drawings had been made of it in its pristine condition, one by Canon Rogers in 1797, and the other by no less accurate a draughtsman, half a century before, namely, by my ancestor, Dr. Borlase. Both these drawings agree in representing the extreme slimness of the pillars; their distance apart; and the great height of the monument; features which render it not unlike a gigantic three-legged milking stool. Then, as now, there was no mound about it, as there is in the case of each and all of the kist-vaen cromlechs. It stood on a low bank of earth, and the area had been often disturbed by treasure seekers. No houses are near it which could have received the stones of a denuded mound. Added to this, it is difficult to see how a kist-vaen, or *septum* of any kind, could have been formed beneath the cap-stone. Had a wall of *small* stones been built up from pillar to pillar the weight of the superincumbent mound must have forced them inwards, a catastrophe which the "dolmen-builders" were always most careful to avoid. Secondly, had *large* stones placed on edge formed the walls of the kist, how is it they are *all* removed, while every other cromlech in the district retains them? But, laying aside this evidence, my strongest proof is yet to come. The interment in this instance was *not in the kist at all*. A grave had received the body six feet under the natural surface of the surrounding soil, and within the area described by the structure. This being the case, of what use could an enclosed kist have been; or why should the cenotaph be covered in at all? Add to this again, that on the southern side of the structure, and so near it that a mound over the monument must inevitably have covered it up, stands a little circular ring cairn of the ordinary type, in the centre of which I found the remains of an inner ring, which, though now rifled, had doubtless contained an interment. Must I then explain away in deference to superior experience or received opinion each and all of the above facts, in order to reconcile this monument with those which seem to be totally different structures, viz., the kist-vaens?[*] Should I not by so doing be sacrificing a fact to an hypothesis, and is not that hypothesis of such a nature that even a single instance well established must shake it to its foundation? Should I not incur a charge of erroneousness equal to, if not greater, than that which Mr. Lukis brings to bear on all who differ from him?

No one can wish more than I do to see errors expunged, and the truth in these matters arrived at; but I must confess that I cannot see how this will be brought about by confronting one hypothesis with another equally dogmatic, and more universally inclusive.

WILLIAM C. BORLASE

Castle Horneck, near Penzance

Fertilisation of the Pansy

I AM glad to be able to confirm, to some extent, from observation, Mr. Bennett's theory of the fertilisation of the Pansy, given in NATURE, vol. viii. p. 49. I watched a considerable number of specimens of *Viola tricolor* on a grassy hill-top where the smaller insects were very numerous and busy, and twice saw them entered by a minute fly. In the first case the insect was dusty with pollen when it arrived. It settled on the lower petal and walked up one of the black lines to the gap in the ring of anthers, through which it entered with some difficulty—leaving some of the foreign pollen on the stigma as it passed. When it came out it had still more pollen on it than when it went in, and again in passing the stigma it left some on it. It paused a moment on the lower petal to clean itself, and left a little ball of pollen on the hairs on one side of the stigma. In the second case, the insect alighted first on one of the upper unmarked petals, turned round and round as though seeking the guiding lines, and flew off to the lower petal, where, without hesitation, it followed the guiding lines as the other had done. After it had passed the stigma there was no pollen visible on its surface; but after it had come out, almost the whole of the lower half was covered. In each case the passage through the ring of anthers seemed rather a struggle. There were many bees about, but I did not see any of them visit the Violæ, although they were almost the only flower near.

A. T. MYERS

Penrith, June 30

European Weeds and Insects in America

A CANADIAN friend writes to me:—"I have heard or seen it mentioned as a fact that European weeds and insects introduced into America flourish for a while, but after fifty or sixty years gradually disappear: for instance, that the Hessian fly (so called from having been brought over by the Hessian troops in their hay in the war of independence) has died out or ceased to give trouble, though at one time it totally destroyed the wheat crops of New England. I do not know how far the facts have been tested, or how far they are owing to improved agriculture."

This statement, if true, is obviously of great importance. Can any of your correspondents confirm or disprove it?

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, July 4

CHLOROPHYLL COLOURING-MATTERS[†]

IT would be impossible for me not to look upon the appearance of such a work as the one recently published by Dr. Gregor Kraus with much satisfaction, since the chief object of the author is to call the attention of his countrymen to the value of the spectrum-microscope in studying the colouring-matters of plants. He commences with a description of the instrument, and says that, though originally designed for the examination of microscopical objects, it is not only as useful as any larger spectroscope for the study of the absorption of solutions, but indeed in many cases preferable. He describes two different kinds of eye-piece, viz., a simple form made by Merz, and the far more complete Sorby-Browning, with the method of measurement proposed by Mr. Browning, and expresses his regret that the value of such instruments has been almost altogether overlooked by German botanists. In treating on the application of the apparatus, the author very justly points out the great advantage of having a bright illumination, without too much dispersion, and the importance of being able to examine the spectrum of a leaf or any other object in its natural state, in order to ascertain whether the colouring matters dissolved out from a plant by any solvent do really occur in it, or are products of decomposition. I would also myself add that in some cases the difference between the spectrum of a substance in a free state and when dissolved is so considerable that care must be taken not to conclude that there has been actual decomposition, until the character of the spectrum of the solid substance, in a free state, has been ascertained; and even when the spectra are very nearly the same, the position of the absorption-bands may differ sufficiently to make it possible to determine whether a colouring-matter naturally exists in a free state or dissolved in water, or in an oil, according as it is or is not soluble in water. The fact of being thus dissolved or not is in some cases, probably, a question of considerable physiological importance, since the existence of solid particles along with, or even actually surrounded by, a liquid capable of dissolving them, points to a very different origin and relation to structure to those of a substance merely dissolved in the juices of a plant or an animal. The solution of such a colouring-matter is sometimes one of the first changes that occur in decomposition, as if set free from minute cells.

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[*] The only other tripod dolmen in Cornwall, viz., that at Caerwynen, is also a free standing one (within the memory of man, at least), whereas the *kist-vaens* are *one and all* partially covered by their envelope

[†] "On the Chlorophyll Colouring-Matters." ("Zur Kenntniss der Chlorophyllfarbstoffe und ihrer Verwandten"). By Dr. Gregor Kraus. (Stuttgart, 1872.)

Having explained the general methods employed, and given a list of the chief publications connected with the subject, the author proceeds to the consideration of various colouring-matters found in plants. If I had written this review immediately after the work was published, I should have expressed my agreement with the greater part of the author's conclusions; for they are those to which a most careful experimenter would be led by employing the methods generally known at that time; but during the last year I have devoted myself exclusively to this particular subject and have been led to employ almost entirely new methods of investigation, and the result is that I must now point out a number of particulars in which I think the author's conclusions are not altogether correct. These new methods consist chiefly in the more or less perfect separation of the different substances by means of bisulphide of carbon, alcohol, and water, used in varying proportions, and in a somewhat peculiar manner; in the employment of what I have named *photo-chemical analysis*, or the use of light as a reagent, so as to destroy some constituents, and leave others, which perhaps could not be separated by chemical methods; and in studying and comparing together all classes of plants, especially the lower cryptogamia, when growing in various conditions; and not only in examining them qualitatively but also in determining the relative amount of the different colouring-matters by a method of comparative quantitative analysis. I will not now enter into detail, but refer to a paper recently communicated to the Royal Society, on comparative vegetable chromatology, in which I have given a complete general description of the methods I have used, of the facts I have observed, and of the conclusions drawn from them, which have a very direct bearing on some of the most important questions in biology, and enable us to examine them from a new point of view.

One great value of the author's work consists in its giving a very complete account of the researches of previous investigators, which I have myself found extremely useful; since so much that has been written is difficult of access. At the same time, since the methods employed were often altogether unsuitable, and most of the experiments are now known to have been made with mixtures, many of the results are of very little more than historical interest. The work also contains three excellent lithographed plates of the spectra of the various colouring-matters in a natural or altered condition. The whole subject is treated in an admirable manner, and I trust that no one will think that I wish in any way to detract from the author's merit in taking this opportunity to illustrate the application of the methods which I think should be employed in such researches.

The coloured solutions obtained from leaves are very complicated mixtures. It is not at all unusual for them to contain as many as ten different coloured substances. The progress of our knowledge has to a great extent depended upon the application of improved methods, which have made it possible to distinguish the various constituents of these mixtures. The author has himself pointed this out, and shown that what was at one time called chlorophyll, and looked upon as a single substance, consists of a mixture of a bluer-green substance with a yellow substance. This kind of analysis had however previously been considerably extended. In a very short paper,* containing no description of the methods of experiment, or of the separate colouring-matters, Stokes said that his researches had led him to conclude that the chlorophyll of land plants is a mixture of four substances, two green and two yellow, and in my late paper I have shown that by the newer and improved methods it is easy to prove that there are not only these two green substances, one a blue-green and the other a yellow-green, having perfectly distinct and characteristic properties, though confounded together by nearly all other experimenters, but also four or even five perfectly distinct yellow substances. These various colouring-matters I have named *blue chlorophyll*, *yellow chlorophyll*, *orange xanthophyll*, *xanthophyll*, *yellow xanthophyll*, *orange lichnoxanthine*, and *lichnoxanthine*. They are all insoluble in water, and soluble in bisulphide of carbon, and besides one or two products of decomposition, they must all have been present in what has sometimes been called chlorophyll, and looked upon as a single compound. Now, almost the only points in which I feel compelled to differ from the author are those cases in which the new methods of examination prove that what he regarded as a single colouring matter is in reality a mixture of two or even more, which can be separated, and do occur separately in particular plants. Thus, for example, in Plate II. Fig. 1, he gives a drawing of the spectrum of the blue-green colouring matter of *Deutzia scabra*, showing six absorption-bands. Now, I feel persuaded that this colouring-matter must have been a mixture of three different substances, viz. my blue chlorophyll, my yellow chlorophyll, and the product of the action of acids on blue chlorophyll The bands numbered 1, 2, 3, and 6 are mainly due to blue chlorophyll. Part of No 1 and No. 5 are due to yellow chlorophyll, and the band No. 4 clearly indicates the presence of a small quantity of the product of the action of acids on blue chlorophyll. This is almost always present when the preparation is made in the manner adopted by the author, but by neutralising the acid of the juice by carbonate of ammonia, or still better by employing a plant that has an almost perfectly neutral juice, chlorophyll may be obtained which gives a spectrum almost absolutely free from any such band.

In the spectrum shown by Plate III. Fig. 1 of the blue-green colouring-matter of an *Oscillatoria*, the bands of yellow chlorophyll are absent, for it does not exist in such *Algæ*, but the broad band shown at about 500 of the author's scale, not seen in the spectrum of the chlorophyll of *Deutsia*, must have been mainly due to orange xanthophyll, which occurs in considerable quantity in *Oscillatoriæ*, but is relatively almost absent in green leaves, and would not be separated by the method employed by the author in making the preparation. Comparatively pure blue chlorophyll, prepared from olive *Algæ* by the method described in my late paper, gives a spectrum free from absorption over the whole of the green and a considerable part of the adjoining blue. The close resemblance, and yet decided difference, between the spectra of the blue green colouring matter obtained from the two above-named sources, did not escape the author's notice, but the methods employed were inadequate to prove that both contained the same principal blue-green substance, mixed in one case with one, and in the other case with another colouring matter. I may here say that the relative amount of blue and yellow chlorophyll differs very much in different classes of plants, and even in the same plant, when in different conditions, and the study of this variation leads to results of great interest in connection with vegetable physiology; since, amongst other things, it proves that leaves normally very yellow are quite unlike those that have turned yellow in autumn, but analogous to those which are abnormally yellow owing to absence of light, as though the deficiency of chlorophyll were in both cases due to weak constructive energy; and the comparative absence of yellow chlorophyll in such abnormally weak plants, belonging to the highest classes, causes their colouring to approximate much more closely to that of those of much lower organisation.

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* Proceedings of the Royal Society, 1864, xiii. p. 144.

I must say that I object to the term chlorophyll being applied, as by the author, to a mixture of the various yellow substances belonging to the xanthophyll group. with one or both of the above-named green substances, The green colour of leaves is due to them, and they are both actually green, one a blue-green and the other a yellow-green, so that the terms blue chlorophyll and yellow chlorophyll appear to me very appropriate. It would be better and extremely convenient to adopt some such word as *endochrome*, to express any mixture of coloured substances contained in the cells of plants, which has no reference to any particular tint of colour.

The very materially different position of the chief absorption-band of chlorophyll when in the leaves of plants and when in solution has been noticed by the author, and likewise the difference in its position when the chlorophyll is dissolved in different liquids. He attributes this entirely to the difference in the density of the liquid, and concludes that in the leaves the chlorophyll may be combined with or dissolved in some dense substance. The difference in the position of the bands of chlorophyll is very small compared with the difference seen in the case of some other colouring-matters, and by carefully studying the question I have come to the conclusion that the position of the bands does not vary directly with the density of the solvent, or with any other general property, but is so independent that it is desirable to look upon it as a special property, and to call it the *absorption-band-raising* power. The extent to which the bands are raised varies much according to the substance; but, as an apparent rule, if the position is altered, they lie nearer to the blue end when the substance is dissolved than when in a free state. In accordance with this view of the subject, it appears as though in the living plants chlorophyll and various other colouring-matters exist in a free state, not combined with or dissolved in any wax, fat, or oil, with which, however, they often combine when the plant is boiled in water, and with which they are combined when a solution is evaporated to dryness, so that the spectrum of such a dried-up material may, and often does, differ most materially from that of the endochrome in the living plants. As an illustration of the opposite case, I may refer to the spectra of yellow flowers, which often show that the endochrome is combined with, or dissolved in, a fat or oil. When not thus combined, the spectra are so different that the colouring-matter might be, and sometimes has been, looked upon as distinct, before the true cause of the difference was known. The microscope alone could not decide this question, since visible granules might not be the free colouring-matter, and, on the contrary, it might be free, and the particles too small to be separately visible.

H. C. SORBY

(*To be continued.*)

RECENT RESEARCHES ON THE PHYSIOLOGICAL ACTION OF LIGHT

THE arrangements by which the mind is brought into relation with the outer world are—(1) a terminal organ, such as the retina, or the intricate structures of the internal ear, or the touch corpuscles of Wagner, for the reception of impressions from without; (2) a nerve, endowed with a special sensibility peculiar to the sense for the conveyance of influences from the terminal organ to the brain; and (3) a sensorium or brain in which, on receiving these influences, changes occur which give rise to the phenomena of consciousness.

Nerves act, therefore, as conductors from the terminal organs to the brain. These terminal organs are specially fitted for the reception of specific stimuli, such as the vibrations of the ether, which, when received by the retina, induce a change which is transmitted to the brain, and gives rise to the sensation of light, or the condensations and rarefactions of the air which cause sound. But though specially fitted for these stimuli, the terminal organs may be affected in other ways. For example, mechanical pressure on the retina produces a sensation of light, and many diseases affecting the auditory apparatus by compression, cause agonising sensations of sound. The nerves in connection with the sense organs are termed nerves of special sense, because they are supposed only to convey influences which are derived from the special terminal organs with which they are connected. These nerves are, however, themselves not affected only by the special stimulus which affects their respective terminal organ. As is well known, the optic nerve is not affected by light—a fact easily demonstrated by Marriot's experiment showing that the retina at the entrance of the optic nerve is insensible to light.

The nature of the specific change produced on the terminal organs by the action of external stimuli has not hitherto been experimentally examined. Let us take the case of the eye. Numerous hypotheses have been advanced. The action of light on the retina has been conjectured to be a mere communication of vibrations, an intermittent motion of portions of the optic nerve, an electrical effect, a heating effect, or a photographic effect like that produced by light on a sensitive surface, but up to this time there has been no experimental evidence in support of either of these views.

The result of investigations made by Mr. Dewar and Dr. McKendrick, of Edinburgh, communicated to the Royal Society of Edinburgh, has been to show that the specific effect of light on the retina and optic nerve is a change in the electro-motive force of these organs. They have been able to demonstrate this by the following arrangements:—The eye of a frog rapidly killed by pithing is dissected out of the orbit, so as to leave the sclerotic entirely free from muscle, and a portion of optic nerve intact. This preparation is placed on the cushions of the well-known arrangement of Du Bois-Raymond for collecting electric currents from animal structures, consisting of two zinc troughs, carefully amalgamated on the inner surface, and containing pads of Swedish filter-paper moistened with a solution of pure neutral sulphate of zinc. To protect the eye from the irritating action of the sulphate of zinc, thin films of sculptors' clay, mixed with a weak solution of chloride of sodium, each worked out to a point, are placed on the pads of filter paper. From each of the troughs a wire passes to a key so as to enable the experimenter to stop the current at pleasure, and from thence the current passes to the galvanometer. They then lay the eye on a glass support between the cushions, and carefully adjust the clay-points so that the one touches the cornea and the other the transverse section of the optic nerve, or the one may touch the surface of the nerve and the other its transverse section. On opening the key, a deflection of the galvanometer needle is at once obtained to the extent of about 600° of the galvanometer scale, placed at a distance from the mirror of the galvanometer of about 26 inches. This deflection is a measure of the natural electro-motive force of the eye. The troughs are now covered over with an apparatus consisting of a double shell made of glass, and containing between the walls one inch of water so as to absorb all heat rays, and lastly a wooden box is placed over the whole, having a draw-shutter so as to enable the experimenter to admit light at pleasure. A gas flame is placed before the shutter. The arrangement is now complete. After observing that the deflection indicating the electro-motive force in the dark is constant, the shutter is now withdrawn so as to admit light. At that instant, that is, on the impact of light, a change is perceived in the electro-motive force. There is at first an increase, then a diminution, and on the removal of light there is another increase of the electro-motive force. Occasionally, in consequence of the dying of the nerve, there is only a slight increase, then a diminution, but the rise on the removal of light is always constant. The amount of change in the electro-motive force by the action of light is about 3 per cent. of the total. There has been no difficulty in demonstrating the effect in the eyes of the following animals, after removal from the body. *Reptiles*, Snake; *Amphibia*, Frog, Toad, Newt; *Fishes*, Gold Fish, Stickleback, Rockling; *Crustacea*, Crab, Swimming Crab, Spider Crab, Lobster, Hermit Crab. The greatest effect was observed in the case of the lobster, in the eye of which Messrs. Dewar and McKendrick found a modification in the electro-motive force by the action of light to the extent of about ten per cent. With the eyes of birds and mammals they had great difficulty. It is well known that in these animals the great source of nervous power is an abundant supply of healthy blood. Without this, nervous action is soon arrested. This law, of course, holds good for the retina and optic nerve. When, therefore, they removed the eye-ball with nerve attached, from the orbit of a cat or rabbit recently killed, and placed it in connection with the clay points, they found a large deflection which quickly diminished, but all sensitiveness to light disappeared within one or two minutes after the eye had been removed from the animal. This fact of itself shows that what has been observed is a change depending on the vital sensibility of the parts. It was therefore necessary to perform the experiment on the living animal under chloroform. By so fixing the head that it could not move, and by removing the outer wall of the orbit so as to permit the clay points to be applied to the cornea and nerve, the same results have been obtained in the case of the cat, rabbit, pigeon, and owl.

Without going into minute detail, which the space allowed for this short article will not admit of, the results of this inquiry have been as follows:—

1. That the specific effect of light on the eye is to change the electro-motive force of the retina and optic nerve.

2. That this last applies to both the simple and to the compound eye.

3. That the change is not at all proportional to the amount of light in lights of different intensities, but to the logarithm of the quotient, thus agreeing with the psycho-physical law of Fechner.

4. That those rays, such as the yellow, which appear to our consciousness to be the most luminous, affect the electro-motive force most, and that those, such as the violet, which are least luminous, affect it least.

5. That this change is essentially dependent on the retina, because if this structure is removed, while the other structure of the eye lives, though there is still an electro-motive force, there is no sensitiveness to light.

6. That this change may be followed into the optic lobes.

7. That the so-called psycho-physical law of Fechner does not depend on consciousness or perception in the brain, but is really dependent on the anatomical structure and physiological properties of the terminal organ itself, inasmuch as the same results as to the effect of light are obtained by the action of the retina and nerve without the presence of brain.

The method of investigation pursued by Messrs. McKendrick and Dewar is applicable to the other senses, and opens up a new field of physiological research. The specific action of sound, of the contact of substances with the terminal organs of taste, and of smell, may all be examined in the same manner; and we are in hopes of soon seeing results from such investigations.

ON THE FERTILISATION OF FLOWERS BY INSECTS AND ON THE RECIPROCAL ADAPTATIONS OF BOTH

II.

In what manner the hive- and humble-bees obtain the honey of the flowers

IN the last number the use the bee makes of its complex sucking machinery, when emptying the deepest honey-tubes or spurs accessible to it, was stated in detail; we have now to show the different movements and positions the separate parts of the mouth undergo, when the bee is obtaining honey less deeply placed, or when it is about to collect the pollen of flowers, or when it folds together the whole sucking apparatus into the cavity of the head in order to employ its jaws or to rest.

(2) In order to obtain the honey out of tubes or spurs of less depth the bee need not turn the cardines forward; these remain at rest in their backward position, the tongue remains consequently embraced by the maxillæ and labial palpi, and only the base of the tongue is alternately protruded and withdrawn, by which motion the terminal whorls of hairs are alternately immersed into the honey and withdrawn into the sucking-pipe.

(3) While the bee, in order to suck honey, flies from flower to flower, it carries its sucking apparatus stretched forward so as to be able to put it directly into the opening of the honey-tube, but its tongue is perfectly enclosed between the labial palpi and the maxillæ; the delicate whorls of hairs are protected by that from any injury they might receive, when introduced into the flowers, and the terminal joints of the labial palpi are not prevented from serving as feelers. Consequently during the flying from flower to flower the base of the tongue is folded into the extremity of the tubular mentum, the cardines are turned backwards, whilst the lora can be directed downwards (Fig. 4), forwards (Fig. 2) or backwards, in proportion as the bee is about to obtain the honey from shorter or longer tubes.

(4) The parts of the mouth must be held in the very same position when the bee wishes to pierce tender cellular textures by means of the tips of its maxillæ. It executes this sort of process, sometimes in order to obtain the fluids of juicy flowers which do not secrete nectar, as for instance *Hyacinthus orientalis*, *Orchis mascula*, *morio* and *latifolia*, sometimes in order to break open honey-tubes which are too deep to be emptied by the bee in the

regular way. Thus, for instance, *Bombus terrestris*, having of all our humble-bees the shortest tongue, forcibly opens the honey-tubes of *Aquilegia*, *Trifolium pratense*, *Pedicularis sylvatica*, and many other flowers; sometimes by piercing the corolla by the tips of its maxillæ, sometimes by biting through the corolla by means of its jaws, and then steals the honey by guiding its proboscis into the honey-tube through the self-made opening.

(5) When collecting the pollen of flowers the hive- and humble-bees moisten, as is well known, the pollen with honey before stripping it off with the brushes of their feet from the anthers and amassing it on the outside of the posterior tibiæ. During this process the maxillæ and the labium are commonly bent beneath the breast, as in inaction, almost as shown in Figs. 5 and 6, the jaws are opened, the labrum is raised, the opening of the mouth is brought near the pollen to be collected, and a drop of honey is spit out upon this pollen; often also the bee before moistening the pollen with honey frees it while still enclosed in the anthers by chewing the anthers with its jaws.

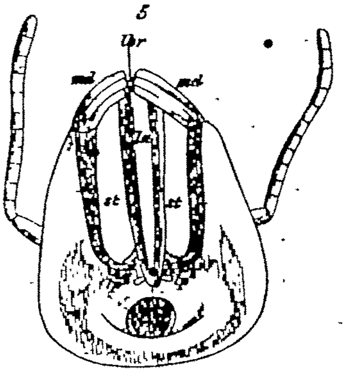

FIG. 5.—The sucking apparatus of a humble-bee (*Bombus hortorum*, L. ♀) placed in the hollow underside of the head, seen from beneath (7 : 1).

In quite a different manner I saw the hive-bee proceed when collecting the loose, dry pollen of *Plantago lanceolata*, so easily shaken out. By vehement movements of its wings the bee maintains itself, steadily humming, at the same place in the air, close before the anthers, the pollen which it is about to collect; in this position it has its sucking-apparatus stretched forward, but the tongue quite enclosed between the laminæ and labial palpi, and spits out of the sucking-pipe formed by these parts a drop of honey upon the anthers. Then it grasps very hastily, with the brushes of its anterior legs, amongst the anthers, and strips off the moistened pollen from them, while the dry pollen of the neighbouring anthers also shaken out, is disseminated, forming a little cloud of dust. Consequently, also in this case the bee carries the base of its tongue folded into the mentum, and the cardines turned backward, precisely in the same manner as when flying from flower to flower, or when piercing honey-tubes by the tips of the laminæ.

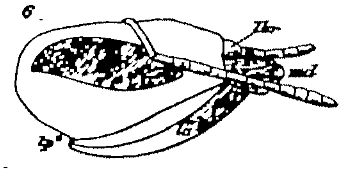

FIG. 6.—Lateral view of the same head.

Plantago lanceolata and other plants with equally loose, dry pollen, scattered by the wind, are honeyless; on the other hand the pollen of all honey-flowers is collected by the hive- and humble-bees when holding their sucking organs retracted, whilst the honey of these flowers is obtained by their sucking-organs stretched forward; hence it follows that hive-bees, humble-bees, and all the bees which are in the habit of moistening the pollen before collecting it, can never suck honey and collect pollen at the same time, but are obliged to perform alternately these two actions after having commenced with sucking honey, of which they are in need for moistening the pollen to be collected, whereas all the bees which collect the pollen without moistening it, as, for instance, the Andrena, Osmia, and Megachile, are often observed sucking honey and collecting pollen at the same time.

(6) When the bee is about to employ its jaws, or when it wishes to rest, it rests the whole sucking apparatus in the hollow in the under-side of the head, by effecting all the four foldings above described, and bends beneath the breast those parts which do not find any room in this excavation, viz., the tongue, and the labial palpi and laminæ enclosing it, as shown in Figs. 5 and 6.

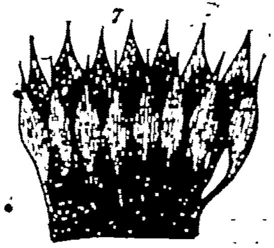

FIG 7.—Two whorls of scales of the terminal portion of the tongue of a blue Brazilian Euglossa (or Chrysantheda), the scales of each whorl alternating with those of the following one (80 : 1).

Everyone who has observed in nature the activity of the hive- and humble-bees will be surprised by the ease with which the numerous movements just described are effected by them. Nevertheless, when sucking honey out of tubes or spurs, they experience a sensible loss of time by so repeatedly protruding and retracting the tongue. This loss of time seems to be avoided by a very singular contrivance lately discovered in some Brazilian bees by my brother, Fritz Müller. In these bees all the rings of the terminal portion of the tongue, from the tip to the sheath, formed by the labial palpi and laminæ, are provided, as shown in Fig. 7, with whorls of narrow-stalked, broad scales instead of hairs, and these scales, lying closely upon one another, form together a tube around the prominent portion of the tongue which probably enables the bee to suck the honey out of the longest flower-tubes accessible to it without needing to retract the tongue.

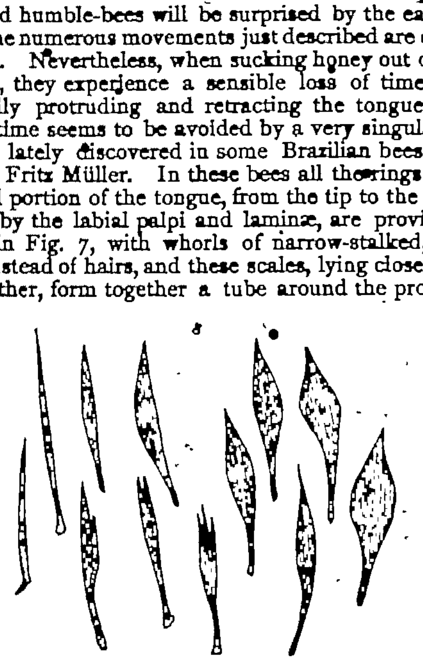

FIG. 8.—Gradations between hairs and scales.

The first scale-bearing rings within the sheath of the tongue, offering numerous gradations by which hairs and scales graduate into each other, as shown in Fig. 8, indicate precisely the degrees of variability by which natural selection arrived at the broad narrow-stalked scales clothing the prominent portion of the tongue.

HERMANN MÜLLER

ON THE ORIGIN AND METAMORPHOSES OF INSECTS[*]

VIII.

FOR the next descending stage we must, I think, look among the Infusoria, through some such genus as Chætonotus or Ichthydium. Other forms of the Rotatoria, such for instance as Rattulus, and still more the very remarkable form discovered last year by Mr. Hudson,[†] and described under the name of *Pedalion mira*, seem to lead to the Crustacea through the Nauplius form. Dr. Cobbold tells me that he regards the Gordii as the lowest of the Scolecida; Mr. E. Ray Lankester considers some of the Turbellaria, such genera for instance as Mesostomum, Vortex, &c., to be the lowest of existing worms; that is to say, if we exclude the parasitic groups. Haeckel[‡] also regards the Turbellaria as forming the nearest approach to the Infusoria. The true worms seem, however, to constitute a separate branch of the animal kingdom.

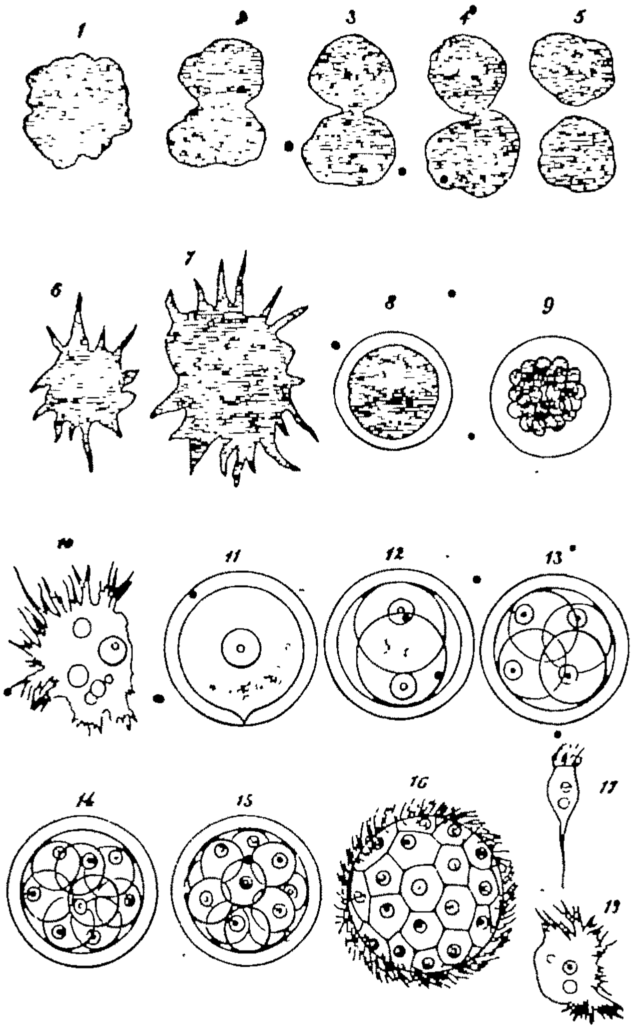

PLATE 5.

Plate 5.—Figs 1–5, Protamœba. 6–9, Protamyxa Aurantiaca. Haeckel Beit zur. Monog. der Moneren, Pl. 1. 10—18, Magosphæra planula Haeckel l.c., Pl. 5.

We may take the genus Prorhynchus,[§] for instance, as an illustration of such a low type (Fig. 59), which consists of a hollow cylindrical body 1½ to 2′′ long, containing a straight simple tube, the digestive organ.

But however simple such creatures as these may be, there are others which are far less complex, far less differentiated; which therefore on Mr. Darwin's principles may be considered still more closely to represent the primæval ancestor from which these more highly developed types have been derived, and which, in spite of their great antiquity, in spite of, or perhaps in consequence of their simplicity, still maintain themselves almost unaltered.

Thus the form which Haeckel has described[*] under the name of *Protamœba primitiva*, Pl. 5, Fig. 1—5, consists of an entirely homogeneous and structureless substance, which continually alters its form; putting out, and drawing in again, more or less elongated processes, and creeping about like a true Amœba, from which, however, Protamœba differs in the absence of a nucleus. It seems impossible to imagine anything simpler; indeed, as described, it appears to be an illustration of properties without structure. It takes into itself any suitable particle with which it comes in contact, absorbs that which is nutritious, and rejects the rest. From time to time a constriction appears at the centre (Pl. 5, Fig. 2), the form approximates more and more to that of an hour-glass (Pl. 5, Fig. 3), and at length the two halves separate, and each commences an independent existence (Pl. 5, Fig. 5).

In the true Amœbas, on the contrary, we find a differentiation between the exterior and the interior: the body being more or less distinctly divisible into an outer layer and an inner parenchym. In the Amœbas, as in Protamœba, multiplication takes place by self-division, and nothing corresponding to sexual reproduction has yet been discovered.

Somewhat more advanced, but yet of great simplicity, is the *Protomyxa aurantiaca*, discovered by Haeckel[†] on dead shells of Spirula, where it appears as a minute orange speck, which shows well against the clear white of the Spirula. Examined with a microscope the speck is seen to be a spherical mass of orange-coloured, homogeneous, albuminous, matter, surrounded by a delicate, structureless, membrane (Pl. 5, Fig. 8). It is obvious from this description that these bodies closely resemble eggs, for which indeed Haeckel at first mistook them. Gradually however the yellow sphere broke itself up into smaller spherules (Pl. 5, Fig. 9), after which the containing membrane burst, and the separate spherules, losing their globular form, crept out as small Amœbæ (Pl. 5, Fig. 6), or amœboid bodies. These little bodies moved about, assimilated the minute particles of organic matter, with which they came in contact, and gradually increased in size (Pl. 5, Fig. 7) with more or less rapidity according to the amount of nourishment they were able to obtain. They threw out arms in various directions, and if divided each section maintained its individual existence. After a while their movements ceased, they contracted into a ball, and again secreted round themselves a clear structureless envelope.

This completes their life-history as observed by Haeckel, who found it easy to retain them in his glasses in perfect health, and who watched them closely. It also coincides very closely with that of the Gregarinæ, another group of singularly egg-like organisms.

As another illustration I may take the *Magosphæra planula*, discovered by Haeckel on the coast of Norway.

In one stage of its existence (Pl. 5, Fig. 10) it is a minute mass of gelatinous matter, which continually alters its form, moves about, feeds, and in fact behaves altogether like the Amœba just described. It does not however remain always in this condition. After a while it contracts into a spherical form (Pl 5, Fig. 11), and secretes round itself a structureless envelope, which, with the nucleus, gives it a very close resemblance to a minute egg.

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[*] Continued from p. 167.

[†] "On a New Rotifer" *Monthly Microscopical Journal*, Sept. 1871.

[‡] Generelle Morphologie. V ii, p. lxxix

[§] Gegenbaur. Grund. d. Vergleich. Anat. p. 210. See also Beiträge Zur Naturg. der. Turbellarien Dr. M. S. Schultze, 1851. Pl vi, fig. 1.

[*] Monographie der Moneren, p. 43

[†] Monographie der Moneren, p 10.

Gradually the nucleus divides itself, and the protoplasm also separates into two spherules (Pl. 5, Fig. 12); these two subdivide into four (Pl. 5, Fig. 13), and so on (Pl. 5, Fig. 14), until at length thirty-two are present, compressed into a more or less polygonal form (Pl. 5, Fig. 15). Here this process ends. The separate spherules now begin to lose their smooth outline, to throw out processes, and to show amœboid movements like those of the creatures just described. The processes or pseudopods grow gradually longer, thinner, and more pointed. Their movements become more active, until at length they take the form of ciliæ. The spherical Magosphœra, the upper surface of which has thus become covered with ciliæ, now begins to rotate within the cyst or envelope, which at length gives way and sets free the contained sphere, which then swims about freely in the water (Pl. 5, Fig. 16), thus closely resembling Synura, or one of the Volvocineæ. After swimming about in this condition for a certain time the sphere breaks up into the separate cells of which it is composed (Pl. 5, Fig. 17). As long as the individual cells had remained together they had undergone no changes of form, but they now show considerable contractility, and gradually alter their form, until they become undistinguishable from true Amœbæ (Pl. 5, Fig. 18). Finally, according to Haeckel, these amœboid bodies, after living for a certain time in this condition, return to a state of rest, again contract into a spherical form, and secrete round themselves a structureless envelope.

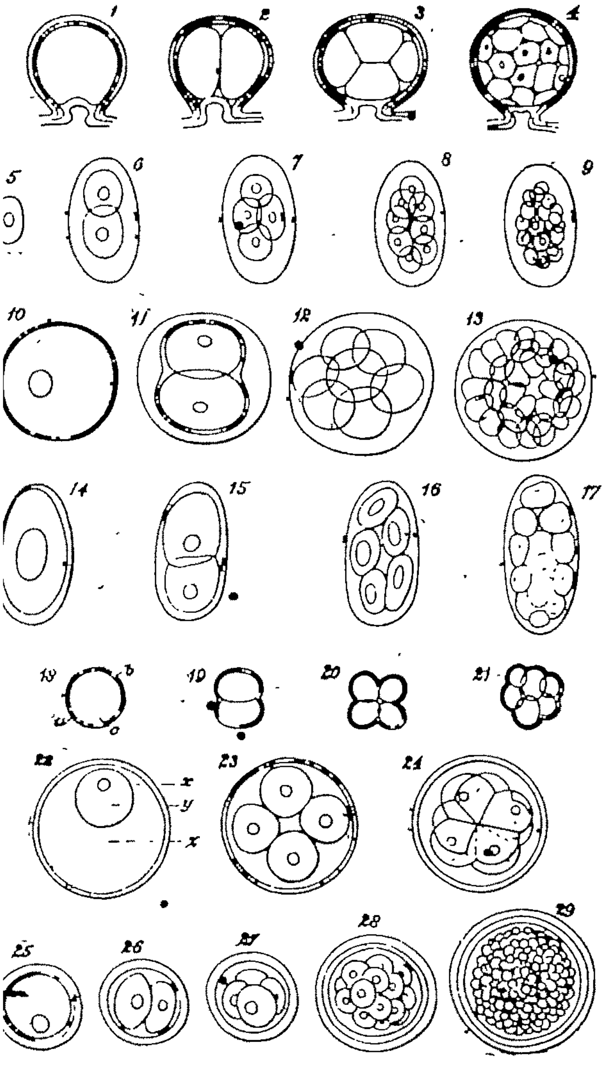

PLATE 6.

Plate 6.—Figs. 1—4, Yolk-segmentation in Laomedea; 5—9, in Filaria, 10—13, in Echinus; 14—17, in Lacinularia, 18—21, in Purpura; 22—24, Amphioxus, 25—29, Vertebrata.

It may be said, and said truly, that the difference between such beings as these and the Campodea, or Tardigrade, is immense. But if it be considered incredible that even during the long lapse of geological time such great changes should have taken place as are implied in the belief that there is any genetic connection between insects and these lower groups, let us consider what happens under our eyes in the development of each one of these little creatures, in the proverbially short space of their individual life.

I will take for instance the first stages, and for the sake of brevity only the first stages, of the life history of a Tardigrade.* As shown in Fig. 60, the egg is at first a round body, with a clear central cell—the germinal vesicle; it increases in size, and after a while the yolk and the germinal vesicle divide into two (Fig. 61), then again into four (Fig. 62), and so on, just as we have seen to be the case in Magosphœra. From the minute cells (Fig. 63) arising through this process of yolk-segmentation, the body of the Tardigrade is then built up.

It is true that among the Insecta generally, normal yolk-segmentation does not occur, though the first stages of development in Platygaster, as figured by Ganin (ante Figs.), closely resemble those of the Tardigrada.

Though I will not now attempt to point out the full bearing of these facts on the study of embryology generally, yet I cannot resist calling attention to the similarity of the development of Magosphœra with the first stages of development of other animals, because it appears to me to possess a significance, the importance of which it would be difficult to over-estimate.

Among the Zoophytes Prof. Allman thus describes† the process in Laomedea, as representing the Hydroids (Pl. 6, Fig. 1, represents the young egg):—"The first step observable in the segmentation-process is the cleavage of the yolk into two segments (Pl. 6, Fig. 2), immediately followed by the cleavage of these into other two, so that the vitellus is now composed of four cleavage spheres (Pl. 6, Fig. 3)." These spheres again divide (Pl. 6, Fig. 4) and subdivide, thus at length forming minute cells, of which, as in the previous cases, the body of the embryo is built up.

In Pl. 6, Figs. 5—9 represent the corresponding stages in the development of a small parasitic worm—the *Filaria mustelarum*—as given by Van Beneden.‡ The first process is that within the egg, which represents, so to say, the encysted condition of Magosphœra; the yolk divides itself into two balls (Pl. 6, Fig. 6), then into four (Pl. 6, Fig. 7), eight, and so on, the cells thus constituted finally forming the young worm. I have myself observed the same stages in the eggs of the very remarkable and abnormal *Sphærularia bombi*.§

Among the Echinoderms M. Derbès thus describes the first stages (Pl. 6, Figs. 10—13) in the development of the egg of an Echinus (*Echinus esculentus*):—"Le jaune, commence à se segmenter, d'abord en deux, puis en quatre et ainsi de suite, chacune des nouvelles cellules se partageant à son tour en deux."‖ Sars has observed the same thing in the starfish.¶

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* See, for instance, Kauffmann, Über die Entwickelung und systematische Stellung der Tardigraden. Zeits. f. Wiss. Zool. 1851, p. 220.

† Monograph of the Gymnoblastic or Tubularian Hydroids, by G. J. Allman. Ray. Soc. 1871, p. 86.

‡ V. Beneden, Mém. sur les Vers Intestinaux, 1858.

§ Natural History Review, 1861, p. 44.

‖ Derbès, Ann. des Sci. Nat. 1847, p. 90.

¶ Fauna Littoralis Norvegiæ, pl. viii.

In the Rotatoria, as shown by Huxley in Lacinularia,* and by Williamson in Melicerta,† the yolk is at first a single globular mass, the first changes which take place in it being as follows:—"The central nucleus becomes drawn out and subdivides into two, this division being followed by a corresponding segmentation of the yolk. The same process is repeated again and again, until at length the entire yolk is converted into a mass of minute cells." Among the Crustacea the total segmentation of the yolk occcurs among the Copepoda, the Rhizocephala, and Cirripedia. Sars has described the same process in one of the nudibranchiate mollusca ‡ (Tritonia), Muller in Entochocha,§ Haeckel in Ascidia,‖ Lacaze Duthiers in Dentalium.¶ Figures 18 to 21, Pl. 6, are taken from Koren and Danielssen's ** memoir on the development of *Purpura lapillus*.

Figs. 22-24 show the same stages in a fish (Amphioxus) as given by Haeckel, and it is unnecessary to point out the great similarity.

Lastly, figures 25 to 29, Pl. 6, are given by Dr. Allen Thomson,†† as illustrating the first stages in the development of the vertebrata.

I might have given many other examples, but the above are probably sufficient, and show that the processes which constitute the life-history of the lowest organised beings, very closely resemble the first stages in the development of more advanced groups; that, as Allen Thomson has truly observed,‡‡ "the occurrence of segmentation and the regularity of its phenomena are so constant that we may regard it as one of the best established series of facts in organic nature."

It is true that yolk segmentation is not universal in the animal kingdom; that there are great groups in which the yolk does not divide in this manner,—perhaps owing to some difference in its relation to the germinal vesicle, or perhaps because it has become one of these suppressed stages in embryological development, many instances of which might be given, not only in zoology, but, as I may state on the authority of Dr. Hooker, in botany also. But however this may be, it is surely not uninteresting, nor without significance, to find that changes which constitute the life-history of the lowest creatures, form the initial stages even of the highest.

Returning to the immediate subject of this work, I have pointed out that many beetles and other insects are derived from larvæ closely resembling Campodea, that other insects come from larvæ more or less like Lindia, and it has been shown over and over again that in many circumstances the embryo of the more specialised forms resembles the full-grown representatives of lower types. I conclude, therefore, that the Insecta generally are descended from ancestors resembling the existing genus Campodea, and that these again have arisen from others belonging to a type represented more or less closely by the existing genus Lindia.

Of course it may be argued that these facts have not really the significance which they seem to me to possess. It may be said that when Divine power created insects, they were created with these remarkable developmental processes. By such arguments the conclusions of geologists were long disputed. When God made the rocks, it was tersely said, he made the fossils in them. No one, I suppose, would now be found to maintain such a theory, and I believe the time will come when it will be generally admitted that the structure of the egg, and its developmental changes, teach us as truly the course of organic development in ancient times, as the contents of rocks teach us the past history of the earth itself.

JOHN LUBBOCK

NOTES

SIR CHARLES WHEATSTONE has been elected a Foreign Associate of the French Academy of Sciences in place of the late Baron Liebig.

MR. COLE's retirement from public service is now completed, and the Treasury have awarded him the full pension usually granted to officers who have completed fifty years of public service. Although Mr. Cole quits the South Kensington Museum, he will continue to assist in promoting the diffusion of Science and Art applied to productive industry as the Acting Commissioner for the estate purchased out of the surplus funds of the Exhibition of 1851. This estate at present comprehends the Horticultural Gardens, the buildings of the Annual International Exhibitions, and the Royal Albert Hall. Measures are in progress for forthwith commencing the National Training School for Music. A meeting of those interested in the Testimonial which it is proposed to present to Mr. Cole, will be held in Willis's Rooms to morrow at 3 o'clock. Those who know best how much Mr. Cole has done for the encouragement and advance of Science, will, we are sure, be the most ready to take part in this well-deserved testimony to the value of his services to the public.

AFTER the alarming rumours that have recently found their way into the newspapers, it is a great relief to receive what appears to be really authentic news of the safety of Sir Samuel and Lady Baker. It appears, from the message received by the *Daily Telegraph*, that they arrived at Khartoum on the 29th of June. It is stated that the party had been as far south as a place called Mosindi, near the chief village of Kamrasi, the King of Unyoro, which would be in about 1°45′ N. lat., and about 80 miles to the east of the shores of the Albert Nyanza. Here Sir Samuel is said to have been attacked by a chief named Kabriki, and, on his retreat, by a party of slave-hunters. He seems to have established another Egyptian station at a place called Fatiko, somewhere to the south of Gondokoro. The story about the Albert Nyanza and Lake Tanganyika being one, which forms part of the news published by the *Daily Telegraph*, is certainly very startling news, and must at present be received with great caution, though the *Telegraph* correspondent declares he received it direct from the lips of the Emancipator of Central Africa himself.

MR. AUBERON HERBERT's Select Committee on the Wild Birds Protection Act has met three times, and examined a good many witnesses. It would not be fair to take the report, published in the *Field*, of what passed at those meetings as strictly correct, but if it be at all true, the doubt, before expressed in these pages (NATURE, May 1, 1873), as to any real good resulting from the inquiry, can hardly be otherwise than justified. The questions put by the chairman indicate, as far as we perceive, that he has a very hazy idea of the bearings of the whole subject, and no one of the other members appears to have sufficient knowledge of any part of it to follow home by cross-examination any of the evidence offered in reply. By many of the witnesses birds are regarded as divisible into two groups—the useful and the noxious—a simple classification which will be amusing to naturalists. Such witnesses also think that the destruction of the latter should be encouraged and the former protected—being quite innocent of the fact that no laws in the world will make most "useful" birds more numerous than they already are. It seems to us that the only way in which an inquiry of this kind could be satisfactorily conducted would be by a Royal Commission, in which the scientific element, so unhappily lacking in a Parliamentary Committee, should be adequately represented. The birds which suffer a perfectly preventible persecution to such an extent that their extermination may shortly be expected, appear to be thought hardly worthy of the Committee's consideration, though it was to save them that the British Association's efforts were chiefly directed.

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* Trans. of the Microsc Soc of London, 1851.

† Quarterly Journal of Microsc Science, 1853

‡ Wiegmann's Archiv, 1840, p 196

§ Ueber die Erzeugung von Schnecken in Holothurien. Berlin, Bericht, 1851 Ann Nat Hist, 1852, v. ix Muller's Archiv, 1852

‖ Ann des Sci Nat. 1853, p 89

¶ Ann des Sci Nat, 1857, pl. vi.

** Naturliche, Schopfungsgeschichte, pl. x

†† Cyclopædia of Anatomy and Physiology Art Ovum, p 4

‡‡ Thomson, l c Article, Ovum p 139

The Highland and Agricultural Society of Scotland have taken a praiseworthy step in memorialising Government to do what is undoubtedly their duty to the country, appoint a Commission of competent scientific men to inquire into the causes of the ever-recurring potato-disease, a disease which is a national calamity. How far advanced is the American Government in matters concerning the national welfare is well shown by the memorialists, and even Portugal is far enough ahead of us to appoint a Government Commission to inquire into the vine-disease.

The Executive Committee of the Fund for erecting a memorial to the late John Stuart Mill have resolved that a portion of the funds raised be devoted to erecting a bronze statue of Mr. Mill in some public situation in the City of Westminster, which he for a time represented in Parliament, the remainder to the foundation of Scholarships, open to the competition of candidates of both sexes, in Mental Science and Political Economy; subscribers to the fund being invited to say to which of these purposes they wish their subscriptions to be devoted.

The Council of University College, London, has determined to throw open to women next session another of its ordinary classes, that of jurisprudence, conducted by Prof. Sheldon Amos.

We are glad to see from a circular which has been sent us, and which we would recommend to the attention of all teachers, and to all interested in science-teaching in schools, that the Charterhouse School of Science has met with signal success during the past, its first, session. There is an excellent staff of scientific lecturers, which we are glad to see is to be increased, the training is thorough and practical, and a large and well-fitted chemical laboratory, besides other scientific apparatus, is to be added to the School. The School is in connection with the Science and Art Department, and we hope that during next session, which commences on September 20, the attendance will be as satisfactory as during the past. Attached to the circular is a form to be filled up by intending students, and accompanying it is a well-drawn up time-table. The fees are remarkably low.

At a meeting of the Council of the Royal School of Mines, held on Saturday, July 5, the following gentlemen received the diploma of Associate of the Royal School of Mines:—Mining and metallurgical division—E. Jackson, J. A. Griffiths, C. Law. Mining division—A. G. Phillips. Metallurgical division—J. W. Westmorland, S. W. Davies, J. C. Jefferson, H. S. Bell. Geological division—G. Smith. The following Scholarships and Prizes were also awarded:—The two Royal Scholarships of 15*l.* each, for first year's students, to Mr. H. Carter and Mr. A. J. Meeze. To second year's students: H.R.H. the Duke of Cornwall's scholarship of 30*l.* for two years, to Mr. C. Lloyd Morgan, and the Royal Scholarship of 25*l.* to Mr. S. A. Hill. The Edward Forbes' medal and prize of books, for Natural History, to Mr. G. Smith. The De la Beche medal and prize of books, for mining, to Mr. Edgar Jackson. The Murchison medal and prize of books, for geology, to Mr. C. Lloyd Morgan.

Signor Augusto Righi, Demonstrator of Physics in the University of Bologna, has just published an elaborate memoir "On the Composition of Vibratory Motions" (*Tipi Gamberini e Parmeggiani, Bologna*). The memoir is of a high order, and is worthy the attention of all physicists specially interested in acoustics. The subject is mathematically treated, and is illustrated by twenty-one admirable plates.

Mr. W. Carruthers has just issued his official Report for 1872, of the Department of Botany in the British Museum. The additions to the Herbarium during the year are spoken of as large and important, rendering more and more pressing the necessity of increasing accommodation for the arranged Herbaria. The species included under several of the natural orders, both in the General and in the British Herbarium, have been entirely re-arranged during the year; and much use has been made of the Herbarium by botanists preparing monographs for a number of different publications. Numerous interesting additions have also been made to the Structural Series, both in the Fruit, the Fossil, and the General Collection.

We have received "Lecture Extra, No. 8" of the *New York Tribune*, containing twelve lectures by Prof. Louis Agassiz, on various important subjects connected with animal life; besides a lecture on "Vestiges of Antiquity," by Dr. A. Le Plongeon, "The Art of Dyeing," by Prof. Chandler, a long article on the Fossil Man of Mentone, and a detailed account of Prof. Marsh's discoveries in the Rocky Mountains. All these lectures and articles are copiously illustrated and well printed, and the whole is a marvellous pennyworth. The *Tribune* deserves the greatest praise for the important part allotted to science in its programme.

In the just published number of the *Journal of Anatomy and Physiology* there is a valuable paper by Prof. Rutherford, of King's College, on the cause of the retardation of the pulse which follows closure of the nostrils in the rabbit, in which he shows that this retardation is not the direct effect of reflex action, as previously supposed, but is due to the arrest of respiration which necessarily attends the blockage in the air-passage; for the retardation does not commence directly the nostrils are closed, but is delayed for about four seconds, and if the trachea is kept open it does not occur at all. Ammonia applied to the nose produces similar effect, because the animal ceases to breathe for a time, as it closes the nostrils in order to prevent the entrance of the irritating fumes. Prof. Rutherford finds that after the vagi have been divided, the arrest of respiration does not cause the pulse to become slower, which is in favour of the supposition that the retardation which normally occurs is produced by the action of the impure blood on the cardio-inhibitory centres in the medulla oblongata.

The *Journal of Botany* records the death of two British botanists of reputation, Mr. James Ward, of Richmond, Yorkshire, one of the most active and experienced botanists of the North of England, and Mr. James Irvine, of Chelsea, who wrote a "London Flora" in 1838, and was one of the editors of the old *Phytologist*.

A Flora of Cheshire is shortly to appear under the superintendence of the Hon. J. L. Warren.

We have to record the following earthquakes this week:—The Imperial Meteorological Observatory of Constantinople reports that on June 20 there were several smart shocks of earthquake at Bagdad at night, and again on the 21st at noon. A strong shock of earthquake was felt at Alpago, Italy on July 3. A volcanic eruption, accompanied by discharges of hot cinders, is stated to have commenced at Farra. The waters of the Lake Santa Croce, a few miles south-east of Belluno, were boiling. Three shocks of earthquake were felt at Buffalo, U.S., on the morning of July 6, causing the buildings and shipping to rock.

The Synopsis of Laboratory Work in Practical Organic Chemistry at the Teachers' Training Class at South Kensington for July, contains seventy practical problems in chemistry, with directions for their solution.

A great International Exhibition is to be held at Philadelphia in 1876.

IN the *Weekly Salt Lake Tribune* of June 7, a lecture is reported on the Sandwich Islands by Dr. Winslow, who resided there for several years. The light in which he represents the natives of Hawaii to regard the death of Captain Cook will be new to many of our readers. "The natives were astonished and distressed at their own barbarity, and they treated the remains of Cook as they did those of their highest chiefs and as if he had been a god. They dissected the big bones from his legs and arms, as a mark of the highest honour they could confer on their own beloved dead. They exposed the rest of his remains before their great idol in the temple, and sacrificed hogs and dogs to his memory and to appease the gods for his and their own sins. His entrails had been placed carefully in a calabash and left aside, in order for burning in some subsequent ceremony, when a boy (an intelligent old man of some 75 or 80 years in 1845, with whom the Doctor had conversed), supposing them to be the entrails of a hog, cut off a piece and roasted it on coals and ate it. When the officers of the ships, in their subsequent intercourse with the natives to recover the remains of Captain Cook, learned that nothing was left of them but the big bones, which were delivered up to them, they fancied his flesh had been devoured by the savages, and a howl went up from the British public and the Christian world that the newly-discovered Hawaiians were natives and cannibals. Such was not the case at first, and has never been the case. Their first experience with a Christian people was a bitter one, and the cup for them has been bitter from that time to this. The facts attending Captain Cook's death, and the treatment of his remains, the Doctor received from the mouth of an honest old native named Keha, on the island of Maui, a clear-minded man, and one of the hereditary historians of the Kings or Chiefs. The natives always regretted Cook's death."

THE German Arctic Navigation Society of Hamburg city has received a telegram from Tromsoe, dated July 6, according to which eighteen Norwegians who had passed the winter in Spitzbergen, have been found dead by the society's schooner *Tromsoe*, Captain Mack. They have been buried by the latter's direction.

THE latest novelty in literature is a farthing daily paper, in the shape of *The Penny-a-Week County Daily Newspaper*, a single copy of which may be had for a farthing, but which, by a little arrangement, will be supplied to any subscriber for a penny a week. It is intended as an organ for sowing broadcast the principles of the Conservative party, who, if they really have the welfare of their country at heart, ought to make use of this splendid opportunity for elevating the classes whom they want to influence, by serving up a daily modicum of useful knowledge methodically arranged,—*i.e.*, Science.

WE have received from A. Ernst his careful paper on the Meteorology of the Carácas, based on three years' observations by Señor Agustin Aveledo.

THE "Transactions of the Royal Society of Arts and Sciences of Mauritius" for 1871, which has just reached us, shows that that body is in excellent working order, and is quite alive to the interests of Science in that hybrid colony, especially in the department of Natural History. The curious mixture of French and English in the volume is significant of the history of the island and the mixed nationality of the colonists. The longest, and one of the most valuable and interesting papers in the volume, is Colonel Pike's account (in English) of a visit he paid to the Seychelle Islands, containing important details on the natural history of this remote and little-known group. The Society has been the means of successfully introducing into the Mauritius the cultivation of the silkworm, and an association has been formed for the manufacture of textile fabrics from native plants, especially from the *Agave*.

THE "Fourth Annual Report of the State Board of Health of Massachusetts," deserves the attention of all who are interested in the public welfare so far as sanitary matters are concerned. Detailed reports on all subjects connected with public health are given, and some humiliating and curious revelations made as to adulteration of food and drink, which seems to be nearly as universal in Massachusetts as in our own enlightened and very moral country; as is also ignorance of the use and preparation of food. Reports such as these show how lamentable and wide-spread is ignorance of the science of living, and with what a host of adverse influences in the way of adulteration, bad drainage, and such like, civilised man is surrounded.

WE have received Memoirs, by Prof. Asa Gray, of the late Mr. John Torrey and Mr. W. S. Sullivant, written for the American Academy of Arts and Sciences.

PART I. of vol. ix. of "The Journal of the Royal Agricultural Society of England and Wales," contains many statistics and papers of great value connected with the subject of Agriculture. Besides a variety of statistics as to grain, Cattle, Sheep, Pigs, Dairy Produce, Prices, &c., the Journal contains the following papers: On the Characters of Pure and Mixed Linseed-Cakes, by Dr. Augustus Voelcker, F.R.S.; Report of the Judges on the Trials of Portable Steam-Engines at Cardiff; Report of Experiments on the Growth of Barley for Twenty Years in succession on the same Land, by J. B. Lawes, F.R.S., and J. H. Gilbert, F.R.S.; Record of Rainfall at Rothamsted (parish of Harpenden) and Harpenden Village, near St. Alban's, Herts, in 1872 and the 19 preceding years; Report on the Trade in Animals, and its Influence on the spread of Foot-and-Mouth and other Contagious or Infectious Diseases which affect the Live Stock of the Farm, by H. M. Jenkins, F.G.S., Secretary of the Royal Agricultural Society; Further Report by the Judges on the Competition for Prizes for Plans of Labourers' Cottages in connection with the Cardiff Meeting, 1872; The Potato Disease, by William Carruthers, F.R.S., Consulting Botanist to the Society; On Dodder, by W. Carruthers, F.R.S.; Annual Report of the Consulting Chemist for 1871; Quarterly Report of the Chemical Committee, December, 1872; Quarterly Report of the Principal of the Royal Veterinary College.

THE death of Mr. J. A. Gordon, Superintendent of the Crystal Palace Gardens, is announced. Mr. Gordon was in part trained under Sir Joseph Paxton, and was well known as a contributor to the *Gardener's Magazine*.

MR. J. L. HADDEN, C.E., superintended the electric light arrangements on the occasion of the late *fêtes* at Constantinople for the Sultan's accession. The next morning on awaking he found himself quite blind. The medical men had hopes of his restoration to sight.

THE additions to the Zoological Society's Gardens during the past week include a Rock-hopper Penguin (*Eudyptes chrysocoma*), from the Falkland Islands, presented by Mr. J. M. Dean; a tuberculated Lizard (*Iguana tuberculata*), from the West Indies, presented by Mr. J. B. Spence; a Greater Sulphur-crested Cockatoo (*Cacatua galerita*), from Australia, presented by Mr. R. Dean; four black-necked Swans (*Cygnus nigricollis*) hatched in the Gardens; a Beaver (*Castor canadensis*), born in the Gardens; two crimson-faced Waxbills (*Pytelia melba*), from Africa; a Tawny Eagle (*Aquila naevioides*), purchased; a black-tailed Antelope (*Nanotragus nigricaudatus*), an Ariel Toucan (*Ramphastos ariel*), and a West India Rail (*Aramides Cayennensis*), deposited.

ON THE GERM THEORY OF PUTREFACTION AND OTHER FERMENTATIVE CHANGES.*

AFTER some introductory remarks referring to the various other theories which had been entertained on this subject, viz., the oxygen theory, the theory of spontaneous generation, and that of chemical ferments, the author stated that the researches of Pasteur had long since made him a convert to the germ theory, which attributes the alteration experienced by exposed organic substances to the development within them of minute organisms springing, like larger living beings, from parents like themselves; and that this belief had been since continually strengthened by the results of the antiseptic system of treatment in surgery, which he had founded on that theory as a basis.

But his attention had been afresh directed to the subject about a year and a half ago by a remarkable paper by Dr. Burdon-Sanderson,† in which experiments were recorded, leading to the conclusion that Bacteria, unlike the spores of fungi, are deprived of vitality by mere desiccation at a moderate temperature, so that while a drop of water from ordinary sources or the contact of a moist surface is sure to lead to Bacteric development and consequent putrefaction in an organic substance susceptible of that change, the access of dust from exposure to the atmosphere induces merely the growth of fungi and comparatively insignificant chemical alteration.

If this were true it would be needless to provide an antiseptic atmosphere in carrying out the antiseptic system of treatment; and all that would be requisite in the performance of a surgical operation would be to have the skin of the part about to be operated on treated once for all with an efficient antiseptic, while the hands of the surgeon and his assistants and also the instruments were similarly purified; a dressing being afterwards used to guard against the subsequent access of septic material. Thus the use of the spray might be dispensed with, and no one would rejoice more than himself in getting rid of that complication.

Such being the practical importance of the conclusion referred to, he determined to subject it to a searching experimental test.

The material first employed was urine, not boiled, as it had commonly been in previous investigations, but obtained, by a very simple antiseptic process, perfectly uncontaminated in its natural condition, in which it proved a far more favourable nidus for the development of organisms than in the boiled state, as indeed might have been anticipated, since it contains unaltered the complicated organic substance termed the mucus, which has been sometimes regarded as a chemical ferment of urine. Nevertheless, when a wine-glass, together with a small porcelain evaporating dish, to serve as a cover, had been heated, like the vessels used by Dr. Sanderson, far above the boiling-point of water, and allowed to cool (a process conveniently designated by the term "heated"), and afterwards charged with the unboiled urine, and placed under a glass shade as an additional protection against dust, it was found that the fluid remained free from organic development or putrefactive change for months, till at last it dried up into a saline mass. On the other hand, if a glass so charged was exposed to the air by removing the shade and cover for a while, organisms appeared in it of various kinds, and among the rest, in several instances, Bacteria. Thus it was shown on the one hand that Bacteria might arise from atmospheric exposure, and on the other hand that a porcelain cover and glass shade afforded absolute security against the introduction of organisms from without. If, therefore, the exposure of such a glass for a limited period chanced to lead to the introduction of any one organism unmixed with others, the opportunity was afforded of studying the behaviour of that organism, either in the same medium for a protracted period or in other media in similar glasses, inoculated by means of a heated pipette or glass rod. For it was found as a matter of experience that exposure for the few seconds or fractions of a second necessary for performing the inoculation or withdrawing a little fluid for examination did not involve any considerable risk of accidental contamination.

Early in the investigation it was ascertained that the putrefaction of urine might take place without the occurrence of Bacteria, in presence of minute granules in irregular groups, in such numbers as to make the liquid milky; their organic nature being clearly proved by fissiparous generation observed to take place in them, though in a different manner from that which is seen in Bacteria. To this form of organism the name "Granuligera" has been provisionally applied.

In one of the experiments related, two drops of water from the tap having been added to a glass of Pasteur's solution, the result was not in the first instance the general opalescence due to Bacteria in a liquid, but a deposit which proved to be a minute filamentous fungus producing abundant spores (conidia) on its branches. These spores after separation often produced young plants like their parents; but there were also seen in abundance precisely similar spores multiplying by pullulation like a Torula (to retain the old use of the name as applicable to organisms like the yeast plant).* And there were also present multitudes of more slender filaments which were seen to break up into Bacteria, while in several instances these filaments were observed springing from spores undistinguishable from those of the fungus.

The view that some filamentous fungi may give origin to both toruloid and Bacteric forms was soon afterwards confirmed by another experiment.† A "heated" wine-glass was taken into the open air during a drizzling rain, and the cover being lifted, some rain-drops were allowed to fall into it, after which uncontaminated urine was introduced. The result was the production of a pullulating delicate Torula, totally different from the yeast plant; forming a granular deposit on the sides of the glass, and an abundant scum, both in the urine and also in Pasteur's solution on repeated inoculations. Portions of both liquids containing this organism having been set aside under circumstances permitting only very slow evaporation, they were examined again eight months later, when a delicate filamentous fungus was found in both, bearing conidia resembling the cells of the Torula, while similar spores were seen multiplying by pullulation, and some of the buds were in a slender form undistinguishable in character from the Bacteria which in the case of the urine were observed swimming in the liquid.

An organism which in the first instance was observed for weeks together growing as a mere Torula having thus, as it appeared, developed into a filamentous fungus after remaining for months in the same solution, hopes were excited that a corresponding observation might be made with regard to the yeast-plant, and this led to a careful examination of a low white mould, referable to the genus Oidium, which was observed in a glass of Pasteur's solution to which yeast had been added several weeks previously. The hope was disappointed, but some interesting facts were elicited: for the fungus was found to vary remarkably according to the quality of the medium in which it grew, having sometimes the aspect of an Oidium with fructifying filaments, sometimes a purely filamentous structure, sometimes a loosely-jointed growth producing abundant oval spores destitute of nuclei, and often pullulating like a Torula, and lastly a purely toruloid form of an entirely different aspect, composed of spherical nucleated cells, occurring in urine, and operating as a powerful putrefactive ferment upon that fluid. Yet totally dissimilar as the different forms of this fungus might appear, their identity was demonstrated by observing with the microscope the actual growth of one from another when transferred to a new medium on a slide of thick glass excavated round a central island, so as to provide a sufficient supply of oxygen to last the growing fungus for a long period. The slide and its thin covering glass were heated between metallic plates to diffuse the heat and prevent cracking of the glass, so arranged as to guard against the entrance of dust during cooling, and all instruments, such as forceps and needles, employed in the subsequent manipulations, were "heated" before being used, the thin covering glass being luted down with melted paraffin applied with a hot steel pen. "Glass gardens" of this construction stocked with various organisms in various media proved extremely useful means of investigation. Samples of the organism introduced were sketched with camera lucida immediately after introduction, and their subsequent development observed with perfect precision. In this way, in the case of the Oidium, spherical nucleated cells of the toruloid form of the organism were observed to sprout into beautiful filamentous fungi, and these again, as the fluid became vitiated by the growing fungus, were found to reproduce as conidia the spherical toruloid cells.

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* Abstract of a communication made to the Royal Society of Edinburgh, April 7, 1873, by Prof. Joseph Lister, F.R.S.

† See 13th Report of the Medical Officer of the Privy Council.

* The toruloid pullulation of spores of some minute filamentous fungi had been previously observed by De Bary. *See* "Morphologie und Physiologie der Pilze," &c., von Dr. A. de Bary, p. 183.

† This view has been expressed by various other authors, but has been hitherto incapable of demonstration in consequence of the uncertainty whether things which seem to grow from one organism may not be merely the result of the accidental presence of others.

Among other *media inoculated with this Oidium* was a solution of albumen obtained by treating a fresh-laid egg with a solution of carbolic acid, to destroy any organisms adhering to the shell, and then breaking it with carbolised fingers into a "heated" vessel containing water that had been boiled and allowed to cool protected from dust, the solution being afterwards cleared by passing it through a boiled filter in a "heated" funnel with "heated" cover. This fluid had remained during the half-year which had since elapsed, free from putrefaction or any other change, except where organisms had been introduced, although the air had free access to it;* a fact which indicates pretty clearly that the putrefaction of eggs, which has been regarded as a stumbling-block in the way of the germ theory, must somehow or other be brought about by the penetration of ferments through the shell and membrane. This, indeed, becomes intelligible enough if we admit that Bacteria may be formed from fungi, and remember how the filaments of some parasitic fungi perforate the epidermis of leaves.† In a glass of this albuminous fluid the Oidium grew very slowly and feebly, but its development was accompanied by a remarkable alteration in the liquid, which, in the course of six weeks, changed from the colourless purity of spring water to the dark brown, almost black, appearance of porter. Yet the dark brown liquid remained perfectly free from smell, proving, what the author had long suspected as the result of experience in antiseptic surgery, that an albuminous fluid may undergo fermentation with odourless products.

Another experiment given in full detail was performed with milk upon the same principle as those with urine and albumen, in the hope of removing another stumbling-block in the path of the germ theory. For, according to the high authority of Pasteur, milk forms an exception to organic liquids in general, in the circumstance that a greater elevation of temperature than the boiling-point of water is required to kill Bacteria contained in it.‡ But the advocates of the theory of spontaneous generation reply that any Bacteria present would be certainly killed by boiling, and therefore the subsequent appearance of living Bacteria in the boiled milk in Pasteur's experiments is proof of their spontaneous evolution from the chemical constituents of the liquid. If, however, by the use of antiseptic means, milk could be obtained uncontaminated from the cow, there being no organisms to kill, boiling might be dispensed with, and the milk, like the unboiled urine, should remain free from organic development or fermentative change, if kept protected in "heated" vessels. Accordingly, five flasks with glass caps, and six test-tubes with wider test-tubes to cover them, having been heated, and allowed to cool under glass shades in the stable where the experiment was performed, the udder and adjacent skin of a cow were well washed with a strong watery solution of carbolic acid, which was also applied with a small syringe to the outlets of the milk ducts, the teat being held in the finger and thumb to prevent the entrance of the solution into the udder, and a milkman with his sleeves tucked up, and his hands and arms washed with the *antiseptic lotion,* was *directed to milk into the* glasses as their covers were successively raised. The cow did not give milk at all freely, and a considerable time was occupied in charging the flasks, but the small quantity required for each test-tube was got by a single squirt from the teat, with almost momentary exposure. Yet not only in all the flasks but in all but one of the test-tubes organisms made their appearance. In one of the test-tubes, however, the unboiled milk had hitherto (for a quarter of a year) remained entirely unaltered. One such success was as clear evidence against the hypothesis of spontaneous evolution of organisms as if all the glasses had remained free from them, and their occurrence in the other ten proved a most fortunate circumstance. For no two of them were alike in the organisms they contained, and in several instances there was apparently only some one species unmixed with others, so that the opportunity was afforded of studying various different organisms as modified by other media, and as regards any fermentative influence which they might exert upon those media. Among the organisms in the milk glasses were Bacteria of different species, to judge from their size and other appearances, as well as numerous kinds of fungi; and when they were introduced into a series of glasses of the albuminous liquid before described, it was found that while some of the fungi grew in it others refused to do so, and while Bacteria obtained by adding a drop of water to urine throve in the albuminous fluid, not one of four inoculations of Bacteria from four milk glasses was followed by any result. Thus was afforded, it is believed, for the first time, distinct physiological proof of real differences among Bacteria. But what was still more unexpected was the fact that when the inoculation was practised in a series of glasses of urine, two of the Bacteria refused to grow even in that liquid, which had been previously regarded as a peculiarly favourable nidus for Bacteric development. This fact, besides serving still more clearly to differentiate the various species of Bacteria, suggested a possible explanation of the failure of experiments with milk in the hands of others. For if organisms thrive in milk which cannot grow in urine at all, milk must be a more difficult fluid to work with in experiments which aim at excluding organic development. Hence it seemed worth while to try again the effect of boiling milk, but in doing so to adopt more rigorous precautions against the entrance of organic germs. There could be little doubt that the organisms which appeared in the various milk glasses of the experiment above related entered during the cooling, which though it took place just as in the successful experiments with urine, led to failure in the case of the milk, partly from the favourable nature of that liquid for organic development, and partly no doubt from the atmosphere of the stable being much more loaded with organic germs than that of the author's study. The new precautions adopted were in the main these. The small wine glasses (liqueur glasses) into which the fluid was to be decanted were covered, together with their glass caps, while still very hot, with cotton wool secured by fine iron wire tied tightly round below the cap, so as effectually to filter the air that entered during cooling; after which the cotton was carefully removed and the glass placed under a small glass shade on a separate piece of plate glass. For heating the flask in which the milk was to be boiled a very high temperature was requisite to ensure destruction of all life in the considerable volume of air which it contained; and this was arranged for by binding asbestos with wire round the junction of the neck of the flask and the glass cap, and then roasting the flask over a large Bunsen's burner. The asbestos, which proved as good a filter as cotton wool, was removed after cooling, and the cap being lifted, a long "heated" funnel was passed quickly into the flask and the milk poured in through it after wrapping a piece of carbolised rag round the funnel and neck of the flask to exclude septic dust: scrupulous care being taken to avoid touching the neck of the flask with the spout end of the funnel as it was withdrawn. By this means security was obtained against the presence of any living organism inside the flask except in the fluid at the bottom of the vessel. The cap was then re-applied and carbolised cotton wool tied over it to filter regurgitant air during the boiling. The necessity for the air-filter was made very manifest during ebullition from the great tendency of milk to froth, involving the necessity of frequently removing the flame, fresh air entering on every such occasion: *another peculiarity of milk which served further to explain the* failure of previous experiments. But the efficiency of the means employed was shown by the appearance of the flask as exhibited to the Society. For although seven weeks had passed since it was filled, the milk was seen to be perfectly fluid and with no appearance of alteration.

All trouble occasioned by frothing, involving constant watching to prevent the froth from wetting the cotton, was afterwards avoided by acting on the suggestion of Mr. Godlee, of University College, London, who happened to be assisting the author at the time, and immersing the flask in boiling water above the level of the liquid, instead of applying the flame directly. This method had the further advantage of avoiding any risk of "burning" the milk, and also any loss by evaporation. A second flask "heated" and charged with milk like the other and similarly covered with cotton wool, was kept in this way at 212° F. for an hour, and, after cooling, its contents were decanted off into twelve "heated" liqueur glasses, and in these it had remained during the seven weeks that had since passed perfectly free from change except when organisms had been intentionally introduced. To illustrate this the author drank, before the Society, the contents of one of the uninoculated glasses, which proved perfectly sweet and good.

It was a curious circumstance that on the morning following the night on which the liqueur glasses were thus charged with boiled milk, the author received from Dr. Roberts, of Manchester, a copy of his paper describing how he had got over all the difficulties, as regards milk, by a different and very simple method.* But beautiful as Dr. Roberts's method was, and perfectly conclusive against the theory of spontaneous evolution, it would not have answered the author's purpose, as it was essential for his investigations that the liquid should be decanted from the flask into the liqueur-glasses. The decanting was effected by means of a "heated" syphon, with special precautions against the entrance of living organisms, as was fully explained to the Society.

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* Dr. Burdon-Sanderson had previously preserved unboiled white of egg unchanged for six months in a "heated" tube containing air, hermetically sealed.

† See De Bary, op. cit., page 216

‡ See Annales de Chimie et de Physique, 1862, p. 60.

The same plan of "heating" the vessels and decanting was afterwards followed with turnip infusion and with urine; and in proof of the security of the method, flasks containing the residual stock of these fluids after decanting into twelve glasses from each nearly six weeks before, were shown to the Society quite unchanged. And as further evidence of the trustworthiness of the system pursued, it was mentioned that out of six series of wine-glasses with about twelve in each series, containing albuminous fluid, urine (in two series), Pasteur's solution, boiled milk and turnip infusion, although portions of the contents had been often removed for investigation or inoculation, only two instances were known to have occurred in which any organism (a filamentous fungus) had made its appearance which had not been arranged for either by inoculation or prolonged exposure.

(*To be continued.*)

SCIENTIFIC SERIALS

Ocean Highways for July is a very interesting number. The first article, on the "Voyage of the *Polaris*," accompanied by six small maps, shows that notwithstanding the disastrous results of Captain Hall's venture, it proves more strongly than ever that a well-equipped Arctic expedition, taking the route of Smith's Sound, would be attended with results of the highest value. "In the present day," the writer concludes, "when the true methods of exploring are well known, and men of science have clearly enumerated the important problems that will be solved, and the numerous valuable results that will be derived from the labours of an Arctic Expedition, the reasons for despatching one have acquired tenfold force." This is followed by a long and extremely valuable and interesting account of "Personal Experiences of Venomous Reptiles and Insects in South America," by Mr. Richard Spruce, who has spent fifteen years in Equatorial Africa for the purpose of investigating the natural history of that region. The author's account of his experiences gives a vivid idea of the many dangers and trials to which devotees of science are exposed, in their endeavours to add to the sum of human knowledge. We would strongly recommend Mr. Spruce's interesting article to all who take an interest in the subject, on which, our readers may remember, there was recently some correspondence in NATURE. H. H. Giglioli contributes two very valuable letters from Dr. Beccari on his explorations in Papuasia, which are likely to be attended with very important results. Other papers in this number are "On Settlements on the Gold Coast," with a map; a paper on Khiva, by Rev. G. P. Badger, consisting of a catena of extracts from several eminent Arabic writers; the "Footpaths of London," a sort of popular geological lecture, by Mr. H. P. Malet; and the second part of Prof. H. Mohn's article on the Meteorological Institute of Norway.

Bulletin de la Société de Geographie, May. The first article in this journal is by M. Charles Maunoir, on the work of the French Geographical Society, and the Progress of the Geographical Sciences during the year 1872.—Mr. W. Huber contributes an interesting paper on the telegraphic network of the globe, with a map showing at a glance how much has already been done in this way to annihilate distance, and how much remains to be done to complete this important work.—This is followed by the conclusion of M. Balansa's paper on New Caledonia, the present instalment treating specially of the Loyalty Islands.—M. Edouard Sayons gives an abstract of the contents of M. Hunfalvy's very interesting work on the Finnish Provinces of the Baltic; the work is published in Hungarian, and is an account of the author's explorations in the districts mentioned in the year 1870.

SOCIETIES AND ACADEMIES

Royal Society, June 19.—"On a newly discovered extinct Mammal from Patagonia (*Homalodotherium Cunninghami*)," by William Henry Flower, F.R.S., Hunterian Professor of Comparative Anatomy, and Conservator of the Museum of the Royal College of Surgeons.

The author describes the complete adult dentition of a new genus of Mammal, founded on remains discovered by Dr. Robert O. Cunningham in deposits of uncertain age, on the banks of the River Gallejos, South Patagonia. The animal appears to have possessed the complete typical number of teeth, *i.e.* twenty-two above and below, arranged in an unbroken series, and of nearly even height, and presenting a remarkable gradual transition in characters in both jaws, from the first incisor to the last molar. The molars more clearly resemble those of the genus *Rhinoceros* than any other known mammal; and, judging by the general characters of the teeth alone, the animal would appear to have been a very generalised type of Perissodactyle Ungulate, allied through *Hyracodon* (a North-American Miocene form) to *Rhinoceros*, also more remotely to *Macrauchenia*, and, though still more remotely, to the aberrant *Nesodon* and *Toxodon*. The generic name *Homalodotherium* was suggested for this form by Prof. Huxley in his Presidential Address to the Geological Society in 1870.

"The Diurnal Variations of the Wind and Barometric Pressure at Bombay," by F. Chambers. Communicated by Charles Chambers, F.R.S., Director of the Colaba Observatory, Bombay.

The object of this paper is to bring to notice a remarkable relation that has been found to exist between the diurnal variations of the wind and the barometer at Bombay.

The observations made use of are the records of a Robinson's anemograph during the first three years of its performance, viz. from June 1867 to May 1870, and the corresponding hourly observations of the barometer and the dry- and wet-bulb thermometer, made at the Government Observatory, Bombay.

The mean results for each hour of the day during the whole period, and the mean diurnal relations of each element are tabulated and graphically represented by figures. The diurnal variation of the wind is then investigated, the most influential part of which is attributed to the land- and sea-breezes which blow from E.S.E. and W.N.W., and are shown to follow mainly the same law of progression as the temperature of the air, thus affording confirmatory evidence of the truth of Hadley's theory of the trade-winds as applied to land- and sea-breezes.

Some peculiarities of the curve representing the land- and sea-breezes are then pointed out, and these the writer concludes are due to the superposition of another distinct variation having two maxima and two minima in the twenty-four hours like the barometer variation; and he supports his views by a reference to the variation of the east components of the wind in the months of July and August, when the land- and sea-breezes have almost disappeared. This is found to exhibit a decided *double* period. The north components of the land- and sea-breezes are then approximately eliminated from the north components of the whole variation, and the variation which then remains exhibits a very decided *double* period in this direction also. These variations with double periods are regarded as indicative of the existence of a double diurnal variation in the general movements of the atmosphere. Upon this hypothesis typical diurnal variations of the wind are deduced for north and south low latitudes; that for north latitudes exhibiting a double diurnal right-handed rotation, and that for south latitudes a double diurnal left-handed rotation, and from these the diurnal variation of the barometer is deduced.

The movements of the wind-vane at Bombay are then analysed, and the writer concludes that the greater part of the excess of "direct" over "retrograde" rotation of the vane at Bombay is due to the *diurnal variation* of the wind.

Extracts are given from observations made at St. Helena, Toronto, and Falmouth, showing the character of the diurnal wind-variations at those places, and their greater or less agreement with the deduced typical curves. The writer maintains that these variations afford independently a possible, if not a probable explanation of that movement of the air which Dové had called the "Law of Gyration;" and in conclusion he points to the extent of their applicability in deducing weather probabilities, and to the method of discussing storms.

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* See NATURE, Feb. 20, 1873.

A postscript is added, giving the mean diurnal variation of the wind at Sandwick Manse, Orkney, and pointing out its general conformity with the results deduced from the Bombay wind-observations.

"On the Mathematical Expression of Observations of Complex Periodical Phenomena, and on Planetary Influence on the Earth's Magnetism," by Charles Chambers, F.R.S. and F. Chambers.

"Observations of the Currents and Undercurrents of the Dardanelles and Bosphorus, made by Commander J. L. Wharton, of H.M. Surveying Ship *Shearwater*, between the months of June and October, 1872." From a Report of that Officer to the Hydrographer of the Admiralty. Communicated by Admiral Richards, C.B., V.P.R.S.

Geological Society, June 25.—Joseph Prestwich, F.R.S., vice-president, in the chair. The following communications were read:—"On six Lake-basins in Argyllshire," by his Grace the Duke of Argyll, F.R.S., president. The author referred to the part ascribed to glacial action in the formation of lake-basins, and described the basins of six lakes in Argyllshire, the characters presented by which seemed to him inconsistent with their having been excavated by ice. Among these lakes were Loch Fyne, Loch Awe, Loch Leckan, and the Dhu Loch.—"Description of the Skull of a dentigerous Bird (*Odontopteryx toliapicus*, Owen), from the London clay of Sheppey," by Prof. Richard Owen, F.R.S. The specimen described by the author consisted of the brain-case, with the basal portion of both jaws. The author described in detail the structure and relations of the various bones composing this skull, which is rendered especially remarkable by the denticulation of the alveolar margins of the jaws, to which its generic appellation refers. The denticulations, which are intrinsic parts of the bone bearing them, are of two sizes—the smaller ones about half a line in length, the larger ones from two to three lines. The latter are separated by several of the smaller denticles. All the denticles are of a triangular or compressed conical form, the larger ones resembling laniaries. Sections of the denticles show under the microscope the unmistakable characters of avian bone. The length of the skull behind the fronto-nasal suture is 2 inches 5 lines; and from the proportions of the fragment of the upper mandible preserved, the author concluded that the total length of the perfect skull could not be less than between 5 and 6 inches. The fossil seems to approach most nearly to the Anatidæ, in the near allies of which, the Goosanders and Mergansers, the beak is furnished with strong pointed denticulations. In these, however, the tooth-like processes belong to the horny bill only, and the author stated that the production of the alveolar margin into bony teeth is peculiar, so far as he knows, to *Odontopteryx*. He concluded, from the consideration of all its characters, "that *Odontopteryx* was a warm-blooded, feathered biped, with wings; and further, that it was web-footed and a fish-eater, and that in the catching of its slippery prey it was assisted by this pterosauroid armature of its jaws." In conclusion, the author indicated the characters separating *Odontopteryx* from the Cretaceous fossil skull lately described by Prof. O. C. Marsh, and which he affirms to have small, similar teeth implanted in distinct sockets.—"Contribution to the Anatomy of *Hypsilophodon Foxii*, an Account of recently acquired Remains of this Dinosaur," by J. W. Hulke, F.R.S. The author communicated details of its dentition, the form of its mandible, and that of the cones of the shoulder and fore limb, and of the haunch and hind limb, hitherto imperfectly or quite unknown. The resemblance to *Iguanodon* is greater than had been supposed, but the generic distinctness of *Hypsilophodon* holds good.—"On the Glacial Phenomena of the 'Long Island,' or Outer Hebrides," I., by James Geikie, F.R.S.E., of H.M. Geological Survey of Scotland. The author commenced by describing the physical features of Lewis, which he stated to be broken and mountainous in the south, whilst the north might be described as a great peat moss rising gradually to a height of about 400 ft., but with the rock breaking through here and there, and sometimes reaching a higher elevation. The north-east and north-west coasts are comparatively unbroken, but south of Aird Laimisheader in the west and Stornoway in the east, many inlets run far into the country. The island contains a great number of lakes of various sizes, which are most abundant in the southern mountain tract and in the undulating ground at its base. The greater part of Lewis consists of gneiss, the only other rocks met with being granite and red sandstone, and conglomerate of Cambrian age. The stratification of the gneissic rocks is generally well marked; the prevalent strike is N.E. and S.W. with S.E. dip, generally at a high angle. The author described in considerable detail the traces of glaciation observed in the lower northern part of Lewis, and inferred from his observations that the ice passed from sea to sea across the whole breadth of this district, and that it not only did not come from the mountainous tract to the south, but must have been of sufficient thickness to keep on its course towards the north-west undisturbed by the pressure of the glacier masses which must at the same time have filled the glens and valleys of that mountain region. After describing the characters presented by the bottom-hill in the northern part of Lewis, the author proceeded to notice those of the lakes, some of which trend north-east and south-west, while those of the mountain district follow no particular direction. The lake-basins of the first series he regarded as formed at the same time and by the same agency as the *roches moutonnées* and other marks of glacial action; they are true rock-basins or hollows between parallel banks wholly of till, or of till and rock. The N.E. and S.W. lakes coincide in direction precisely with the strike of the gneiss; and the author explained their origin by the deposition of till by the land-ice in passing over the escarpments of the gneiss facing the north-west. The lakes of the mountain district are regarded by the author as all produced by glacial erosion. The author considered that the ice which passed over the northern part of Lewis could only have come from the main land. Referring to the glaciation of Raasay, he showed that the ice-sheet which effected it must have had in the Inner Sound a depth of at least 2,700 ft., and taking this as approximately the thickness of the *mer de glace*, which flowed into the Minch, which is only between 50 and 60 fathoms in depth, no part of this ice could have floated, and the mass must have passed on over the sea-bottom just as if it had been a land surface. Ice coming from Sutherland must have prevented the flow of the Ross-shire ice through the Minch into the North Atlantic, and forced it over the low northern part of Lewis; and the height to which Lewis has been glaciated seems to show that the great ice-sheet continued its progress until it reached the edge of the 100-fathom plateau, 40 or 52 miles beyond the Outer Hebrides, and then gave off its icebergs in the deep waters of the Atlantic.—"Notes on the Glacial Phenomena of the Hebrides," by J. F. Campbell, F.G.S. The author stated that, on the whole, he was inclined to think that the last glacial period was marine, and that heavy ice came in from the ocean, the local conditions being like those of Labrador. The author regarded most of the lake-basins of the Hebrides as formed by ice-action, and considered that the ice by which those islands were glaciated came from Greenland.—"On Fossil Corals from the Eocene Formation of the West Indies," by Prof. P. Martin Duncan, F.R.S. The specimens were collected from limestone and coral conglomerates, which are covered by, and rest upon volcanic *débris* and ejectments in the island of St. Bartholomew. The determination of the forms of the associated Mollusca and Echinodermata permit the following deposits being placed on a general geological horizon—the limestone and conglomerate of St. Bartholomew, the dark shales beneath the Miocene of Jamaica, the beds of San Fernando, Trinidad. These were probably contemporaneous with the Java deposits, the Eocene of the Hala chain, the great reefs of the Castel Gomberto district, the reefs of Oberberg in Steiermark, and the Oligocene of Western Europe. The affinities and identities of the fossil forms with those of contemporaneous reefs in Asia and Europe, and the limitation of the species of the existing Caribbean coral fauna, point out the correctness of the views put forth by S. P. Woodward, Carrick Moore, and the author, concerning the upheaval of the isthmus of Panama after the termination of the Miocene period.—"Note on the Lignite-deposit of Lal-Lal, Victoria, Australia," by R. Etheridge, jun., F.G.S. The lignite is almost entirely composed of remains of coniferous plants not now existing in Victoria; and the author considered that it is nearly of the same age as the lignite deposit of Morrison's Diggings, which has been regarded as Miocene.

Entomological Society, July 7.—Henry T. Stainton, vice-president, in the chair. Mr. Weir exhibited specimens of *Agrotera nemoralis*, taken near Lewes.—Mr. McLachlan exhibited a remarkable instance of hermaphroditism in a specimen of a fly (one of the *Syrphidæ*) taken at Black Park.—Mr. Trovey Blackmore exhibited specimens of a gall found on oaks near Tangier, which were taken possession of for a habitation by a species of ant (*Crematogaster scutellaris*, Oliv.)—Mr. William Pryer exhibited some fine species of Lepidoptera from

China.—Sir S. S. Saunders communicated a paper "On the habits and economy of certain Hymenopterous Insects which nidificate in briars; and their parasites." The insects were exhibited at the last meeting, and Sir Sydney Saunders further exhibited a specimen of a *Raphiglossa*, which he had suffocated with cyanide of potassium, whilst asleep, showing the remarkable position of the insect during repose, as described in the paper.—Mr. Butler communicated a list of the species of *Galeodides*, with description of a new species in the British Museum.

PHILADELPHIA

American Philosophical Society, March 7.—Hector Orr made a communication on the microscopic slide of Mr. Holman.—Dr. Leller exhibited a modification of apparatus for showing the vibration of molecules in light.—Prof. J. P. Lesley presented a map of the subterranean portions of the collieries of Wilkesbarre, Pennsylvania.—Prof. P. E. Chase read a paper on Planetaxis, the relation of the rotation of the sun and interior asteroids to the sun-spot period, and on the relative velocities of light and gravity.

March 21.—Prof. P. E. Chase pointed out the precise acordance of the wave-length of the Fraunhofer F line with the wave-length of the F note in the 26th musical octave. The other Fraunhofer lines also correspond very closely with the musical notes which are designated by corresponding letters. If this accordance indicates that the luminiferous æther is a material medium, it appears that Winnecke's estimate of the sun's distance is the most accurate of those that have been based on astronomical observations.—Prof. Persifor Fraser exhibited an apparatus for the better manipulation of the lime-light.—Mr. Holman exhibited a slide for the microscope, designed for the better observation of substances suspended in fluids, especially the different corpuscles of the blood. The slide contained two concavities on its face, which were connected by a groove, and covered by a thin plate of glass. It was highly sensitive to changes of temperature.—A resolution was adopted recommending the passage of a bill by the Legislature of Pennsylvania, inaugurating a new Geological Survey of the State.

April 4.—Prof. P. E. Chase showed that, by making the differences symmetrical at each extremity of the planetary series, the supposed failure of Bode's law in the case of Neptune was only apparent, and that it gave the rule a higher generality. He also gave two new planetary series, based, like his modification of Bode's law, on laws of oscillation. If the mean distance of Neptune be divided by successive powers of the ratio of a circumference to its diameter, the points of division will fall in alternate planetary orbits, Saturn, Asteroid, Earth, Mercury. The last term of this first series brings us to the orbital axis of the centres of gravity of the sun and Jupiter. The second series is in regular harmonic progression. Taking Jupiter's perihelion distance as the unit,

$$\tfrac{1}{3},\ \tfrac{1}{5},\ \tfrac{1}{7},\ \tfrac{1}{9},\ \tfrac{1}{11},\ \tfrac{1}{13},\ \tfrac{1}{15},$$

respectively designate orbital positions of Mars, Earth, Venus, ?, Mercury's aphelion, Mercury's mean, Mercury's perihelion. Saturn, Uranus, and Neptune are also in harmonic progression beyond Jupiter. If we express this spheral harmony by musical intervals, they are generally such as to produce chords between any two adjacent planetary positions. But where quarter tones occur, the discordant vibrations seem to have broken up or disturbed the tendencies to planetary aggregations, thus aiding in producing the asteroidal belt, giving Mars and Mercury their diminutive masses and great eccentricity, and obliterating the theoretical planet between Mercury and Venus.—Prof. W. C. Kerr, State Geologist of North Carolina, communicated a paper on Topography of the Earth's surface, as affected by the rotation on its axis. He pointed out that the rivers of southern and eastern North Carolina flowed towards the ocean in a south-easterly direction, and that their south-western banks are elevated and bluffy, while the north-eastern descend very gradually to the water. They flow through, yielding materials of the cretaceous and tertiary formations, and have apparently undergone change of location, in the course of which they have excavated their south-western banks.—Prof. Kerr exhibited some mathematical reasons why this change might have been effected by the earth's rotation.—Prof. E. D. Cope read a paper on the flat-clawed carnivora of Wyoming. This group embraced two genera, *Mesonyx* Cope, and *Synoplotherium* Cope, which bore some resemblance in dentition to *Hyaenodon*. In both the claws were broad, flat, and fissured above, and without projecting endinous insertion below, and hence little prehensile use. In *Mesonyx* the astragalus has two distal facets; in *Synoplotherium* the scaphoid and lunar bones were distinct. The genera were thought to be of aquatic habit.

PARIS

Academy of Sciences, June 30.—M. de Quatrefages, president, in the chair.—During the meeting the Academy proceeded to elect a Foreign Associate in the place of the late Baron Liebig. Sir Charles Wheatstone obtained 43 votes, M. d'Omalius d'Halloy, 2; Sir C. Wheatstone was therefore declared duly elected.—The following papers were read:—Reflexions on Lagrange's memoir on the problem of three bodies, by M. J. A. Serret.—A comparison of the refraction indices of several isomeric compound ethers, by MM. Pierre and Puchot. The authors have found these indices sensibly the same when calculated for temperatures equally distant from the respective boiling points of the bodies in question.—On the analytical theory of the satellites of Jupiter, by M. Souillart.—Researches on the reflexion of solar heat at the surface of Lake Leman, by M. L. Dufour.—On the transplantation of the marrow of bones in sub-periosteum amputations, by M. Félizet.—New observations concerning the presence of magnesium round the entire disc of the sun, by M. Tacchini.—On the want of agreement between the old theory of the thrust (*poussée*) of earth and experiment, by M. J. Curie. This was a paper dealing with fortification.—Note on magnetism, by M. J. M. Gaugain.—On the cooling and freezing of alcoholic liquids and wines, by M. Melsens.—On the decomposition of metallic carbonates by heat, by M. L. Joulin.—On the calculus of the moments of inertia of molecules, by M. G. Hinrichs.—On the production of glycerin starting from propylene, by MM. Friedel and Silva.—On a glycerin of the aromatic series, by M. E. Grimaux.—On the estimation of sugar by Barreswil's method, by M. Loiseau.—Erythrophenic acid, new reaction of phenol and aniline, by M. Jacquemin.—On crystallised mercurous iodide, by M. P. Yvon.—A summary of the state of silk culture in 1873, by M. E. Guérin-Méneville.

DIARY

FRIDAY, July 11.

Quekett Club, at 8.

SATURDAY, July 12.

Botanic Society, at 3.45.

TUESDAY, July 15.

British Horological Institute, at 8.30.—Anniversary.

PAMPHLETS RECEIVED

English.—Official Guide-Book to the Brighton Aquarium: W. Saville Kent, F.Z.S.—Third Annual Report of Devon and Exeter Albert Memorial Museum Schools of Science and Art.—Quarterly Weather Report of the Meteorological Office, Part III., July to September, 1871.—Reports and Proceedings of the Miners' Association of Cornwall and Devon for 1872-3.

Australian.—Notes on the Climate of Victoria: Robert L. J. Ellery.—Record of Results of Observations in Meteorology, Terrestrial Magnetism, &c. taken at the Melbourne Observatory during February 1873: Robert L. J. Ellery.

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THURSDAY, JULY 17, 1873

THE PAY OF SCIENTIFIC MEN

THERE are a good many points of interest attaching to the Parliamentary paper referring to the pay of the officers of the British Museum, which, thanks to Lord George Hamilton, has been issued during this week.

It shows in a striking manner what the Government thinks of Science and its votaries; nor is this all: it shows in a not less striking manner how it behoves men of Science, if they consider that there should be a career for Science at all, to at once take some action, in order that their real claims may be conceded. Mr. Lowe, in defending not long ago the high rate of pay of Treasury clerks, who "begin" at 250*l.* a year and rise quickly to 1,200*l.* (if they are unfortunate enough not to get a staff appointment with much higher pay, long before they would, in the ordinary course of promotion, reach the senior class), stated that what was principally wanted at the Treasury, over and above the ordinary qualities of a clerk, was a certain "freemasonry," which was best got at the public schools. For this "freemasonry" Mr. Lowe is willing to pay 150*l.* a year over and above the 100*l.* which is the usual commencing pay of a junior clerk in the other Crown offices.

Perhaps it is too much to say that this "Freemasonry" is required in the British Museum. But there is certainly something required in the case of the scientific appointments there, of as special a character; and that is a knowledge of Science.

What then does Mr. Lowe do to secure this specialty? He gives the man of Science who enters the Museum the magnificent sum of 90*l.* per annum on entrance, with the still more magnificent—but, unfortunately, very distant—prospect of attaining an income of 600*l.* So that:—

Public School Freemasonry : Scientific Attainments : : 250*l.* : 90*l.*

This state of things has recently been brought home to the Trustees by petitions from all grades in the Museum, and a sub-committee of the Trustees has reported that, "owing to the insufficiency of the salaries, the slowness of their progressive rise, and the lowness of their maximum, the trustees are losing, and will continue to lose, their best men."

As a result of this report, in which we consider that higher ground might have been taken, the Trustees have proposed a new scale to the Treasury, the only fault of which is that—with the exception of the case of principal Librarian, who is not a specialist, who has no special work to do which could not be done by the keepers acting in turn as Dean, and who already has just double the salary of the most highly-paid keeper—it is far too modest. As the *Daily News* has well put it, a maximum of 500*l.* is "certainly not a too lavish position for a man who must be a scholar and linguist, an archæologist, naturalist, or chemist, and must in most cases be already in middle life."

The men upon whose heads, hands, reputation, and work the success and fame of the Museum depend, are the keepers, whose pay, even as revised, is a mere pittance for such service as they render.

Altogether, the eventual *total* increased annual expenditure would amount to 5,700*l.* a year—the pay of *one* political or legal placeman, who has properly employed his "Freemasonry."

Here is the Treasury reply:—

"Treasury Chambers, March 28, 1873

"My Lords and Gentlemen,—The Lords Commissioners of Her Majesty's Treasury have had before them two letters from Mr. Winter Jones, dated the 4th instant, submitting recommendations for the grant of increase of salary to the principal Librarian and Secretary, and to various other officers of your establishment, and they desire me to say that, after giving their most careful consideration to all the statements put before them, they regret that they would not feel warranted in acceding to any alteration in the present scale of salaries.

"I have, &c.

(Signed) "WILLIAM LAW"

We trust that some determined stand will be made by the Trustees—among whom is the Right Hon. Robert Lowe—against this monstrous letter; and we trust also that some general protest will be made by men of Science and Culture generally against this latest valuation of these acquirements by the Government.

The man of Science serves his country as well as the politician, the lawyer, the soldier, or the sailor, although perhaps his claims are not stated in so blatant a manner, nor are at present so generally acknowledged, whether they will be in the future must to a large extent depend upon men of Science themselves: but whether this be conceded or not, surely in a country where the State remuneration for services performed is extraordinarily high in the upper appointments, our scientific chiefs in the public service should at all events receive the means of a decent livelihood, and such men as are employed in the British Museum, many of whom have world-wide reputations, should at least be treated as well as Government clerks.

Surely this is not to ask too much? Nay, it is already conceded by the Government in many departments where special scientific knowledge is required of no higher order than that which is so shabbily treated in the one Institution of which we have the greatest reason to be proud.

THE "POLARIS" ARCTIC EXPEDITION

WE have just received the printed Report, presented to the President of the United States by the naval authorities, of the result of their examination of those of the crew of the *Polaris*, who, in October last, were severed from that ship, and drifted on an ice-floe from about 80° north latitude during the whole of the winter until, 600 miles south from their starting-point, they were picked up on April 30, of this year, by the *Tigress* off the coast of Labrador. The Report furnishes material for one more of those thrilling narratives of Arctic adventure, which will be the delight of the boyhood of all generations, and which, commencing in the 10th century with that of Bjorne the Norseman, have been accumulating in increasing proportion, and will never fail to be added to until not a shred of mystery remains to unravel within the Arctic circle. The advocates of Arctic exploration by way of Smith's Sound, needed only the narrative furnished in this Report, to render their arguments invincible.

The *Polaris*, an ordinary wooden vessel, left New London, Connecticut, on July 3, 1871, well furnished with provisions, but otherwise ill fitted for an Arctic expedition, under the command of Captain Hall, an enthusiastic explorer, who firmly believed he was "born to discover the pole," but apparently deficient in the firmness and decision necessary to manage a crew amid the trials of an Arctic winter; the officers and crew, moreover, seem to have been collected at haphazard, and were by no means well assorted. The second in command, Captain Buddington, who has now the command of the *Polaris*, ought never to have been taken on such an expedition, and, even though the most lenient construction be put upon his conduct, is deserving of the severest reprehension. After a delay of a week at St. John's, Newfoundland, the *Polaris* sailed for the West Coast of Greenland, and after calling at several places on that coast, arrived at Disco, which she left on August 17. After calling at the settlements of Upernavik and Tessiusak, the latter in 73° 24′ north lat., the *Polaris* commenced her exploring work in earnest, leaving Tessiusak on the 24th August. Hitherto there had been no difficulty whatever in navigation, nor was the vessel destined to meet with any obstruction until passing through Smith's Sound and Kennedy Channel, she reached 82° 16′ N. lat., a point far beyond the limits of previous navigation. This she did on August 30, within a week after leaving Tessiusak. After making unsuccessful efforts to find a way through the ice, Captain Hall resolved to return and take up winter quarters, which he did on September 3, in a small sheltered cove or bend of the coast in what he called Polaris Bay, the "Open Polar Sea" of Kane, where the ship was protected by a stranded iceberg—Providence Berg. This was in 81° 38 N. lat., 61° 44′ W. long. Had the vessel been specially built for Arctic exploration, it appears to us that Captain Hall by good management could have pushed even farther north before requiring to return to winter-quarters: as it is this is one of the most wonderful and successful Arctic cruises on record, considering the distance accomplished in less than a week so far within the ice-bound region. It affords the strongest ground for hope that with a vessel specially fitted for ice-navigation, a skilful captain may ere long complete the 8° that remain to be traversed before the North Pole be brought within the sphere of the known.

From Polaris Bay on October 10 Captain Hall left the *Polaris*, accompanied by Mr. Chester, first mate, and Hans the Esquimaux with two sledges and fourteen dogs. In the progress of the journey he discovered, as appears by his despatch, a river, a lake, and a large inlet. The latter, in latitude 81° 57′ north, he named "Newman's Bay," calling its northern point "Cape Brevoort," and the southern one "Sumner Headland."

Captain Hall, it appears, had hoped, when he left the *Polaris* on this journey, to advance northward at least a hundred miles; but after having gone about fifty he was compelled, by the condition of the shore and of the ice, and by the state of the climate, to return and await the approach of spring for another attempt. He reached the ship on October 24, apparently in his usual fine health, but was attacked the same day with sickness, and, taking to his bed, the next day was found to be seriously ill. After rallying once or twice he died on November 8, and was buried on the shore. The commissioners who examined the crew reach the unanimous conclusion that the death of Captain Hall resulted naturally from disease, without fault on the part of anyone. After this sad event, the command of the expedition devolved upon Captain Buddington, who expressly declared, according to the evidence, that he had no inclination and no intention to pursue discovery further; he determined to make his way south to the United States as soon as the ice would permit. During the winter little was done, and on August 12, 1872, the *Polaris* began to move southwards. On the 16th of August the ship was made fast to a large floe of ice in the latitude of 80° 2′ north, and longitude about 68° west, and while still fast to this floe drifted south through Smith's Sound nearly to Northumberland Island. On the night of the 15th of October, 1872, in about latitude 79° 35′ north, during a violent gale of wind and snow, the ship was suddenly beset by a tremendous pressure of ice, which was driven against her from the southward and forced under her, pressing her up out of the water, and by successive and violent shocks finally throwing her over on her beam-ends. In the words of the Report,—

> Captain Buddington directed the provisions, stores, and materials which had been put in readiness on deck, to be thrown over on the ice, and ordered half the crew upon the ice to carry them upon a thicker part to the hummocks, where they would be comparatively safe. He also sent all the Esquimaux, with their kyaks, out of the ship, and lowered the two remaining boats upon the floe. While so engaged, in the darkness of an Arctic night, in the midst of a fierce gale and driving snow-storm, the hawsers of the *Polaris* failed to hold her, and she broke adrift from the floe, and in a few minutes was out of sight of the party who were at that moment busily at work on the ice.
>
> From October 15, 1872, until April 20, 1873, when they were picked up in latitude about 59° north, these nineteen men, women, and children remained through the whole of the dark and dreary winter upon the ice. In their first endeavours to reach the land, they occupied for a time different pieces of floating ice, but, forced finally to abandon all hope in this direction, they rested at last upon the floe upon which the *Polaris* had made fast.
>
> At the time of their separation from the *Polaris* every one belonging to the expedition was in good health. She had plenty of provisions, but not much coal—probably about enough to last through the winter. She was last seen, apparently at anchor, under Northumberland Island, where it is most likely she remained for winter-quarters.

Mr. Robeson has already given preparatory orders to the United States steamer *Juniata*, now at New York, to proceed, at the earliest practicable moment, to Disco, and if possible to Upernavik, for the purpose of carrying forward the necessary coal and supplies, communicating with the authorities of Greenland, obtaining information, and, if practicable, sending forward some word of encouragement to those on board the *Polaris*. This last will most likely be impossible, but an attempt will be made.

It is also proposed to fit out at once an expedition of relief, to be sent to Northumberland Island, where the *Polaris* was last seen, in the *Tigress*, about 200 tons burden, built and fitted to contend with the ice, and the same ship by which the nineteen persons were rescued.

The following, in the words of the Report are a brief summary of some of the scientific results of the ill-managed expedition :—

While the records of the astronomical, meteorological, magnetic, tidal, and other physical departments of the exploration appear to have been extremely full, and the observations in each appear to have been conducted according to approved methods, the collections of natural history are shown to have been not less extensive, the store-rooms of the *Polaris* being filled with skins and skeletons of musk-oxen, bears, and other mammals ; different species of birds and their eggs : numerous marine invertebrata ; plants, both recent and fossil, minerals, &c. Not the least interesting of these collections are specimens of driftwood picked up on or near the shores of Newman's and Polaris Bays, among which Mr. Meyer thought he recognised distinctly the walnut, the ash, and the pine. Among the numerous facts that appear to be shown by the testimony elicited on the examination, we may mention as one of much interest that the dip of the needle amounted to 45°, and its deviation to 96°, being less than at Port Foulke and Rensselaer Harbour, as given by Dr. Kane and Dr. Hays. Auroras were frequent, but by no means brilliant, generally quite light, and consisting sometimes of one arch and sometimes of several. Streamers were quite rare. Shooting-stars were so constantly seen that, although no special shower was observed, it was scarcely possible ever to look at the star-lit sky without noticing them in one direction or another. The rise and fall of the tides were carefully observed, the average being about five and a half feet. The greatest depth of water noticed was about 100 fathoms. The existence of a constant current southward was noted by the expedition, its rapidity varying with the season and locality. The winter temperature was found to be much milder than was expected, the minimum being 58° in January, although March proved to be the coldest month.

The prevailing winds were from the north-east, although there were occasionally violent tempests from the south-west. Light winds were noticed, however, from all points of the compass. Rain was occasionally observed, only on the land, however, the precipitation presenting itself over the ice in the form of snow. During the summer the entire extent of both low lands and elevations are bare of both snow and ice, excepting patches here and there in the shape of the rocks. The soil, during this period, was covered with a more or less dense vegetation of moss, with which several arctic plants were interspersed, some of them of considerable beauty, but entirely without scent, and many small willows scarcely reaching the dignity of shrubs. The rocks noticed were of a schistose or slaty nature, and in some instances contained fossil plants, specimens of which were collected. Distinct evidence of former glaciers were seen in localities now bare of ice, these indications consisting in the occurrence of terminal and lateral moraines.

Animal life was found to abound, musk-oxen being shot at intervals throughout the winter.

Wolves, also bears, foxes, lemmings, and other mammals, were repeatedly observed. Geese, ducks, and other water-fowls, including plover and other wading-birds, abounded during the summer, although the species of land-birds were comparatively few, including, however, as might have been expected, large numbers of ptarmigan or snow-partridge. No fish were seen, although the net and line were frequently called into play in the attempt to obtain them. The waters, however, were found filled to an extraordinary degree with marine invertebrata, including jelly-fish and shrimps. Seals are very abundant. Numerous insects were observed, also, especially several species of butterflies, specimens of which were collected ; also, flies and bees and insects of like character.

The geographical results of the expedition, of which the accompanying map will give a good idea, so far as they can now be ascertained from the testimony of Messrs. Tyson, Meyer, and their comrades, may be summed up briefly as follows :—

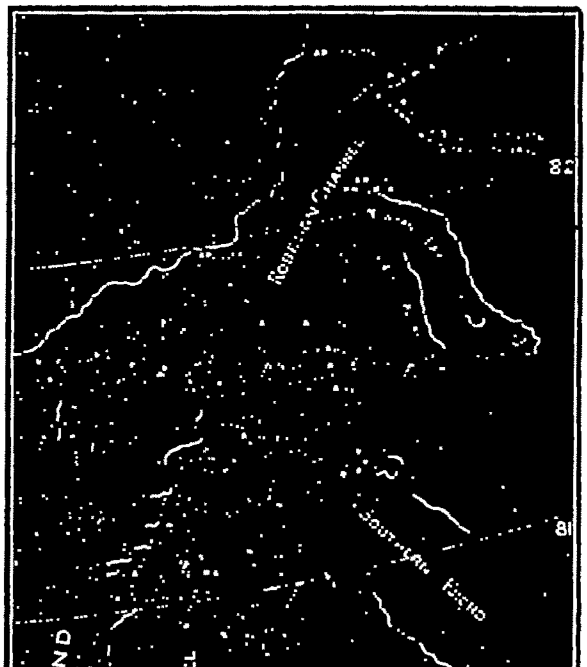


Diagram of the Explorations of the *Polaris*. (Drawn by F. Meyer, Signal Service, U.S.A.)

The open polar sea laid down by Kane and Hayes is found to be in reality a sound of considerable extent formed by the somewhat abrupt expansion of Kennedy's Channel to the northward, and broken by Lady Franklin's Bay on the west, and on the east by a large inlet or fiord, twenty-two miles wide at the opening, and certainly extending far inland to the south-east. Its length was not ascertained, and Mr. Meyer thinks that it may be, in fact, a strait extending till it communicates with the Francis Joseph Sound of the Germania and Hansa expedition, and with it defining the northern limits of Greenland. This inlet was called the Southern Fiord. North of it, on the same side, is the indentation of the shore called Polaris Bay by Captain Hall.

From Cape Lupton the land trends to the north-east, and forms the eastern shore of a new channel from twenty-five to thirty miles wide, opening out of the sound above mentioned, to which Captain Hall gave the name of Robeson Straits. North-east of Cape Lupton, in lat. 81° 57′, is a deep inlet, which Captain Hall called Newman's Bay, naming its northern point Cape Brevoort, and its southern bluff Sumner Headland. From Cape Brevoort the north-east trend of the land continues to Repulse Harbour, in lat. 82° 9′ north—the highest northern position reached by land during this expedition.

From an elevation of 1,700 ft. at Repulse Harbour, on the east coast of Robeson Straits, the land continues north-east to the end of those straits, and thence east and south-east till lost in the distance, its vanishing point bearing south of east from the place of observation.

No other land was visible to the north-east, but land was seen on the west coast, extending northward as far as the eye could reach, and apparently terminating in a headland and near latitude 84° north.

Mr. Meyer also states that directly to the north he observed, on a bright day, from the elevation mentioned, a line of light apparently circular in form, which was thought by other observers to be land, but which he supposed to indicate open water.

Of course the full scientific results of the *Polaris* expedition cannot be known until that vessel shall have been found and brought back with the treasures she has gathered, and the records and details of her Arctic explorations. But enough is told by the witnesses whom we have examined to excite expectation and encourage the hope of large and valuable additions to the domain of human knowledge.

Enough has been said to show that the way to the North Pole is clear and practicable: it remains for Britain to consummate the glory she has already acquired by sending out an expedition so equipped that it cannot fail to return with the solution of the Arctic mystery, whose bourne is being pushed further and further back every year. We would recommend the Report to the Joint Committee of the Royal and Geographical Societies now considering the subject of an Arctic Expedition.

SCIENCE AND ANGLING

Flies and Fly Fishing, with Hints on Minnow and Grasshopper Fishing. By Capt. St. John Dick. (Hardwicke.)

IT is doubtful whether much real progress has been made in the art of angling since the time of Walton, whose "Complete Angler" was published in 1653. A great improvement has taken place in fishing-tackle and implements, and we have much better rods, reels, lines, and lures now, than could have been got in old Isaac's time. Of late years the number of rod-fishers has enormously increased, and there is quite a plethora of popular treatises on the art of fishing. But in all the books we have seen, including the one whose title is at the head of this notice, there is a striking absence of any guiding principles to go by; and notwithstanding the marked improvement in the mechanical appliances referred to, and the increased number and activity of anglers, we repeat that it may be fairly doubted whether the latter are more successful fishers than their representatives 200 years ago. The cause of this is probably owing to the fact that hitherto attention has been almost exclusively directed to the mere practice of the art, and that angling as a science has been all but completely ignored. We have *ad nauseam*, empiric and dogmatic rules for the guidance of the tyro, but few of these are based on sufficient data, and most of them are quite untrustworthy. There is no statement, for example, more frequently made in books on angling than that if the wind be from the east trout will not rise to the fly; and yet there are lakes (notably Loch Leven, Kinross-shire, probably the best trouting lake in Great Britain), in which the fish take best when the wind blows from that quarter. Another generally accepted canon is that fish will not rise freely during a thunderstorm, or when "there is thunder in the air;" but in our own not very large experience, we have again and again proved the falsity of this rule. It would be easy to multiply examples of the worthlessness of such empiric directions. What is wanted is a scientific treatise on angling. A principle in Science, some one has said, is a rule in art; and it is such rules that are desiderated. The object of this paper is rather to indicate this want than to supply it; and we have little hopes of much progress being made in the "gentle art" until it is carefully studied and treated scientifically. Until this is done there are many difficult problems connected with angling which must, we fear, remain unsolved. One day, for example, fish will take greedily any fly that is offered them, for an hour or two; and before or after this, their feeding time, the most skilful angler will practise all his wiles in vain. Another day, only flies of a particular colour or shape have any chance of taking. Again, it does happen occasionally that a veteran Waltonian will return from his favourite stream or lake, under the most auspicious influences of sky, wind, and water, with a very light basket, or it may be, an empty one. It is also a fact that the most successful day's fishing is sometimes achieved by going dead against all recognised rules and imitations of Nature. These are only a few of the things that require to be explained, and in the explanation of which a careful study of the nature and habits of fishes—how they are affected by atmospheric influences, &c.—would probably greatly assist. Of course, there are scientific anglers who have picked up their science under difficulties, and as they best could; and their number might be indefinitely increased if greater facilities were afforded for acquiring scientific knowledge. Such anglers will be sure to have the indispensable qualities of patience and perseverance; but they must also be careful observers of Nature, of the conditions of the water, of the appearance of the sky, and of meteorological phenomena in general; and in addition to all these they will be found to possess an intimate acquaintance with some special branch of Natural History.

There is a point connected with angling which is raised by Captain Dick, but not for the first time, and which demands investigation. It seems to be beyond question that, over the whole of Great Britain, trout are every year becoming scarcer. It is very seldom that the angler now-a-days makes a basket equal to what would have been called a very common take a score of years ago. So alarming has been this decrease that district associations are being formed for the purpose of watching and protecting the spawning grounds in their neighbourhood. The falling off is probably due to a variety of causes, such as over-fishing, pollution of streams, want of protection of spawning fish and spawning beds, the prevalence of pike, &c. It is certain that many streams and lakes, easy of access to populous districts, suffer from being over-fished; but the example of Loch Leven, already referred to, shows what may be done if proper precautions be taken. This lake is only 3½ miles by 2½, and 9 miles in circuit, and is open to anglers from all quarters (by paying a certain sum per hour) during the four months May, June, July, and August. The rest of the year the lake is closed, and the spawning grounds are carefully watched. There are both pike and perch in the lake, but nets are freely used to keep down these marauders. The results of these measures are worthy of notice. For the last fifteen years the takes have been gradually increasing, and last year upwards of 17,000 trout were taken by the rod. During the months of May and June this year nearly 9,000 have been taken, and it may be added that the average weight of Loch Leven trout is a little under 1 lb. What has been done by private enterprise for this lake might and should be done by Government for all the lakes and rivers in the country. There is no reason, that we know of, why there should not be a close time for trout as well as for salmon. The pollution of rivers by public works is a more difficult question to deal with; but surely something could be done to prevent such wholesale destruction as that, for example, which took place in the first week of July this year in the rivers Teviot and Ribble. In the former of these rivers tens of thousands of fish, including trout, smelt, grayling, and even salmon, were poisoned in one day. Unless some action be taken by Government strictly prohibiting manufacturers from sending their poisonous refuse into our rivers, not only will the fish in these soon become extinct, but the rivers themselves thus impregnated will act as open sewers generating and propagating disease in every direction. With a little judicious legislation, the quantity of fish obtained in fresh water might be so largely increased as to become important as an item of food for the people. We have indicated how this might be done with regard to trout, &c. With regard to salmon, all that is necessary to do is to blast the rocks at the Falls of the Tummel, the Gary, and the Spean, in Scotland, and of the Axe, and other rivers in England, and the area of the spawning grounds of this monarch of our rivers would at once be doubled. This could be done at little expense, due allowance being of course made for vested rights and any interests involved

A single glance at any page of Captain Dick's book is sufficient to show that he is more accustomed to wield the rod than the pen; indeed we fail to see the *raison d'être* of the gallant captain's work. He has, it is true, mentioned one or two things worth setting down in an article or essay, but not worth writing a book about. His list of artificial flies is very full and may be of service. The only contribution to Natural History we can find is his statement—which we are inclined to accept as fact—that "although fish generally lie with their heads pointing up stream, they never, by any chance, take a fly in that position, but always make a decided turn in the act of rising, and take the fly with their heads pointing down stream." He adduces this as a reason for fishing down stream, of which practice, in opposition to the best anglers, he is a strenuous advocate. As to fishing with minnows, he prefers the ordinary metal kill-devils to natural minnows and to all other imitations. In this, also, experienced anglers will generally disagree with him. There is no lure more deadly for large trout, in certain seasons, than the natural minnow, and next after that, we should say, is the phantom minnow. In his remarks on pike-fishing, the author does not refer to the spoon-bait, which nevertheless, in lakes, especially in dull weather, may safely be backed against any other lure. Why does the author almost always use the word "fisherman," and only once the much more precise term "angler"? Strictly speaking, "fisherman" is a generic term, and applies equally to net and rod-fishers, but by common usage is generally employed to denote the former; whereas "angler" is a distinctive term which can be applied only to the rod-fisher.

MIVART'S "ELEMENTARY ANATOMY"

Lessons in Elementary Anatomy. By St. George Mivart, F.R.S. Pp. 535. (Macmillan, 1873.)

THIS modest volume is one of the series to which Huxley's "Physiology," Oliver's "Botany," and Roscoe's "Chemistry" belong. Like them it has the indispensable merit of being an elementary manual written by a master of the subject; for while special investigations may be often well performed by advanced students, primers and text-books can only be properly written by experienced teachers.

The plan of the book is to describe in a popular manner the various bones and other parts of human anatomy, excepting the reproductive organs, and then to point out the chief variations among other vertebrata. It would perhaps have been better to have called it "Elementary Lessons in the Comparative Anatomy of Vertebrate Animals:" for as all the organs are used to illustrate those of man, consideration of non-vertebrate classes is very reasonably omitted. Moreover, for reasons given in the preface, with which every teacher of the subject will probably agree, the largest space is given to the account of the endoskeleton. The whole forms a collection of facts, accurate in detail, carefully arranged, and clearly described. One would think there must be slips among so many isolated statements, but we have failed to detect one in a careful perusal of about 300 pages. The sixth chapter contains a review of the general morphology of the vertebrate skeleton, and here Mr. Mivart's well-known views, communicated to the Linnæan and Zoological Societies, are expounded fully but simply. Without admitting all his positions, as for example the homology of the *trabeculæ cranii*, most of what is stated in this chapter is well enough established to form part of a manual for students of comparative anatomy.

But who are these students? No one could follow the closely printed pages of description here given, without a good general acquaintance with human anatomy and a thorough knowledge of the human skeleton. For this reason we think it would have been better to have curtailed or even omitted the preliminary accounts of each organ in man, because they are not sufficient alone, and there are many excellent treatises on this subject already. If it is answered that the book is really intended for boys and girls at school, then the details given, especially in osteology, are far too numerous: in fact they would be unintelligible without a good museum, and learning zygosphenes and hypapophyses without seeing them is far worse mental training than *Barbara Celarent*, or the verbs in -μι. For the second class of readers mentioned in the preface, teachers, medical students, and others acquainted with human anatomy, this little treatise will be found just what they want in order to learn "its more significant relations to the structure of other animals." The only defect they will find is the omission of the organs of reproduction and the structure of the ovum.

The woodcuts are generally sufficient, and some of the diagrams are remarkably ingenious and useful. Some are, however, much too small, *e.g.* the diagram of the skull, Fig. 197, and all the figures of entire skeletons, as 200; while others, as 137, representing the shoulder-girdle of *Hemidactylus*, after Parker, greatly need the shading and tinting of the original drawing. The plan of repeating an illustration whenever it is referred to is not often adopted in English books, but on the whole it is, we think, the most convenient.

Experience will show what class of students will really make most use of Mr. Mivart's Lessons. We heartily recommend them to all medical students and zoologists who have access to a good museum. P. S.

OUR BOOK SHELF

Die Robbe und die Otter (Phoca vitulina *et* Lutra vulgaris) *in Ihrem Knochen-und Muskel-skelet.* Eine anatomisch-zoologicgische Studie von Dr. J. C. G. Lucae. 102 pp. 15 plates. (Frankfort-on-the-Main, 1873.)

UNDER this title the distinguished anatomist, Prof. Lucae, has contributed to the "Transactions of the Senckenbergian Society of Naturalists," an elaborate treatise upon the anatomy of the Common Seal (*Phoca vitulina*). The osteology of *Phoca* is minutely described; and every part of its skeleton compared with the corresponding portions of that of the Otter—one of its nearest allies among the terrestrial Carnivora. Comparisons with other mammals are also given.

Fifteen well-executed plates illustrate this excellent memoir, which, when completed (the first part being only now before us) will leave little to be added to our information as to the osteology of the true Seals (*Phocidæ.*) To our knowledge of the structure of the two other families of the marine Carnivora (the Trichecidæ and Otariidæ) we have lately received a valuable contribution in the shape of Dr. Murie's Memoirs on the Walrus and Sea-lion, published in the Zoological Society's "Transactions," so that great progress has lately been made towards a perfect understanding of the osseous structure of the marine Carnivora.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Agassiz and Forbes

MR. GEORGE FORBES has, in NATURE of May 22, given his version of the controversy between Agassiz and Forbes. I had no intention, in a former note, of reviving, for the benefit of the readers of NATURE, this unpleasant subject; but simply wished to protest against the *ex cathedra* statements of the reviewer of Tyndall. The materials for an impartial discussion of the history of glacier work are accessible to all investigators, and when it comes to be written, Agassiz and Forbes will obtain due credit for their share of the work. One of the points at issue between Forbes and Agassiz is not a matter "of facts to be proved or disproved by facts." The conversation between Agassiz and Forbes (Heath as witness for Forbes) held on the first day of their sojourn on the Aar Glacier, refers simply to *a difference of opinion on the explanation of certain bands* (observed previously by several persons, and well known to Agassiz). The nature of these bands has to this day remained problematical, and why Agassiz, when writing to Humboldt that *he had observed these bands at a depth of* 120 *ft. in the body of the glacier*, should give any credit to Forbes passes all understanding. This observation was made after Forbes's departure, and Agassiz certainly needed no "reconciliation with his conscience" to describe this as "le fait le plus nouveau que *j'ai* observé." The testimony of Mr. Heath is of no value, for it certainly would be the height of presumption, in a man without any previous acquaintance with glaciers, to undertake to decide in a few lines, a point to this day a subject of controversy among investigators of glaciers; his endorsement of the claims of Forbes is as ridiculous as the attempt made by a prominent Swiss geologist (who gives his testimony in favour of Forbes), to ignore the claims of Agassiz, by passing his name over in silence when writing the history of geological science in Switzerland.

I would also remind Mr. George Forbes and the editors of the "Life and Letters of Forbes," that Agassiz's affirmation carries as great weight as that of Forbes or Mr. Heath. Forbes is entitled to whatever credit there is in his explanation of these bands and no more, an explanation which has not been adopted by Agassiz for the very good reason that he did not deem it a satisfactory one, and did not attach to it the same importance which Forbes did. Agassiz expressed to Forbes considerable surprise at the appearance of the bands which presented that morning peculiar conditions, not usually seen except after a hard rain, and on the strength of this surprise Forbes lays claim to the discovery of the bands, and boldly accuses Agassiz of knowing nothing about them at the time of his visit. In investigations carried on for several years, as those of the glacier of the Aar under Agassiz, it was most natural that special points should not always be uppermost in the mind of the investigator, however interesting they might appear to a visitor; this will fully account for any want of interest shown by Agassiz on first meeting Forbes and discussing the veined structure of the ice.

Agassiz certainly owed nothing to Forbes, who was an invited guest on the glacier of the Aar, a novice in glacial work. No attack was made upon Forbes, as is stated by Mr. George Forbes; it originated with him. In a letter addressed to Forbes by Agassiz when he first discovered that Forbes had published, independently as his own, observations made upon the glacier of the Aar, during his stay with the Swiss party, he says: "the idea that in thought you conceived the project of an independent publication did not come to me for an instant. I should have thought I did you injustice by such a supposition." Agassiz felt *he* "had been deeply wronged" by the course taken by Forbes; he made no answer to Forbes, and paid no further attention to the subject, not because there was "no room for discussion," but because the tone adopted by Forbes was so insulting and overbearing as to render all further discussion impossible without its degenerating into the personalities afterwards indulged in by Forbes, in his letters to his friends, which the editors of his Life and Letters have taken special pleasure in reproducing.

Forbes did not hesitate to bring Mr. Heath uninvited to the glacier of the Aar, probably to act as his witness and swel the party, yet both he and his son regard the presence o friends of Agassiz, to assist him in his work, a most monstrous circumstance. Pioneers usually find it difficult to explore the way, but when the track is once blazed it is easy enough to follow and find the path.

As I do not wish to fill the pages of this journal with personal explanations, my contributions in NATURE to this subject must cease with this note. It is not my purpose at present to refute the imputations cast upon Agassiz by the editors of Forbes's Life and Letters; he can well afford to pass them over as he has done thus far, in silent contempt, the more so since, fortunately for Agassiz, the editors have given us from Forbes's own letters all that was necessary to show a course of duplicity, on Forbes's part, towards the man with whom "he served his apprenticeship in glacier observation," which is happily rare among scientific men.

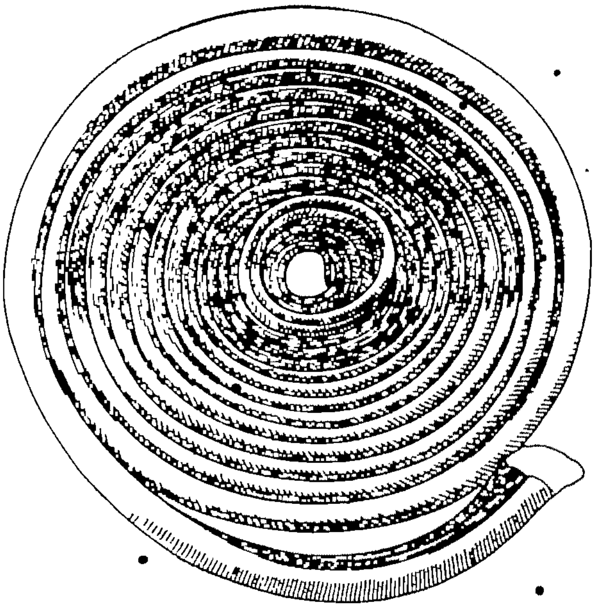
ALEXANDER AGASSIZ

Probosces capable of sucking the Nectar of Anagræcum sesquipedale

MR. W. A. FORBES, in the number for June 12 started the question, whether moths are known to inhabit Madagascar with probosces capable of such an expansion, as to obtain the last drops of the nectar secreted in the lower part of the whip-like nectaries of *Anagræcum sesquipedale*.

As long as a direct answer to this question has not been given, it may be of some interest to state in general the existence of moths provided with probosces sufficiently long for the honey-spurs in question.

Some days ago I received a letter from my brother, Fritz Müller (Itajahy, Prov. St. Catharina, Brazil), in which he says: "I recently caught a Sphinx (not determinable by Burmeister's "Brazilian Sphingidæ"), the proboscis of which has a length of about 0·25 metres—a length not approached by any honey-tube of this country known to me. I enclose the proboscis." Being unable to get the name of this species of Sphinx, I append the illustration of its proboscis, magnified in the proportion 7 : 1.



This proboscis, in its contorted condition forming a roll of 10–11 millimetres in diameter, and showing at least 20 elegant windings, in its expanded condition attains a length of between 10 and 11 inches, and would consequently be adapted to the nectaries of *Anagræcum sesquipedale*, which have been found by Darwin 11½ inches long, with only the lower inch and a half filled with nectar. Darwin indeed says, with regard to the fertilisation of *Anagræcum sesquipedale* (p. 198 of his work on Orchids): "there must be moths with probosces capable of extension to a length of between 10 and 11 inches."

Lippstadt, July 1

HERMANN MULLER

An Order of Merit

YOUR leading article in the last number of NATURE on the subject of a proposed "Order of Merit for Scientific Men," recalls the views (in exact correspondence with your own) entertained by my brother-in-law, the late J. Beete Jukes. These were expressed by him in no uncertain terms on the occasion of publishing an address on the Geological Survey, delivered in Dublin in 1865.

I take the liberty of sending you a print for your perusal, and to refer to note B, at p. 21. I was glad to see the subject so well dealt with in your article.

ALF. H. BROWNE

5, West Hill, Highgate

"Men of science have of late years pandered too much to the utilitarian quackery of the age, and it is time that some one should stand up to protest against it. Government and the House of Commons should be told that Science must be supported and encouraged for her own purely abstract purposes, independently of all utilitarian applications. The necessary preliminary, indeed, to these utilitarian applications is the discovery and establishment of abstract scientific truth by men who look to that alone, and whose whole faculties and lives are devoted to it. The men who afterwards make the practical applications of it often attain, indeed, far wider reputations than the real men of science, and become to the popular gaze the representatives of Science itself. The higher class are rarely much known to the public during their lives, and are not usually men who would experience any satisfaction if they were nick-named Knights or labelled with C B, or would feel inclined to accept any other crumbs that might fall from the table of the politically great and powerful. Nor would they commonly care much for pecuniary rewards, unless as a means to enable them to do their work without drudging for the support of themselves or their families. They are the men, however, who in the end rule the world, and doubtless they are often sustained in their labours by a consciousness of this fact.

"It would manifestly conduce to the public good and the national honour if such men, when they do arise amongst us, should be sought out, recognised as public benefactors, and allowed means to do that work which their faculties, and theirs only, enable them to perform" ("Her Majesty's Geological Survey of the United Kingdom," &c., by J. Beete Jukes, F.R.S. 1867.)

Geological Subsidence and Upheaval

SIR J. HERSCHEL thought that the earth's crust floats upon an ocean of molten matter, and that the washing of detritus from the land into the sea, by altering the relative weight of different portions of the shell, occasions a subsidence of the ocean's bed and an upheaval of the land, which may be either gradual and insensible, like the process of denudation, or spasmodic and by fits and starts producing earthquakes and sometimes volcanic eruptions.

This theory was at one time adopted, at least partially, by Sir C. Lyell, but is not mentioned in the latest edition of his "Principles," and is generally rejected by geologists as at variance with the opinion held by Sir W. Thomson and others in regard to the internal solidity of the earth. But this objection may be avoided by modifying Sir J. Herschel's theory. We may repudiate his hypothesis that a great fiery ocean exists below the outer crust. We may arrive at many of the important conclusions which he drew from this hypothesis, and which he described as all that a geologist could require, by admitting either that *solid rocks are plastic*, or that *some of the lower and warmer strata of the earth are more pliable than the upper.*

As to the plasticity of solid bodies, it may be sufficient to refer to the experiments of M. Tresca (Comp. Rend. de l'Acad., 1864–65, and Annales du Conservatoire, No. 21). Dr. Tyndall (Glaciers of the Alps, p. 9) suggests the possibility that the contortions of the strata in the valley of Lauterbrunnen may have been produced by pressure acting throughout long ages on the rocks in their present hard and solid condition.

Again, the lower strata of our globe may be rendered more pliable than the superincumbent rocks by the great internal heat, although it may be insufficient to fuse them or even to maintain them in a viscous condition. Many of the geological effects of a molten ocean may thus be produced.

The theory that volcanic eruptions are caused by water percolating through superficial cracks may, perhaps, give a clue to the reason why volcanoes often occur in a great circle round the globe and in diametrically antipodal positions. When other causes concur to modify the form of the earth, the tidal strain occasioned by the sun and moon may often be required to overcome the *vis inertiæ;* this strain being greatest in the great circles of the globe perpendicular to the direction in which the sun and moon happen to be, cracks would probably occur most readily in these circles.

It seems at least a curious coincidence that some areas of recent subsidence, *e.g.* coral reefs and islands, are parts of the earth's surface which have lately increased rapidly in weight; and it may be worthy of consideration whether coral and volcanic islands have contributed to deepen the bed of the ocean.

J. F. ANDERSON

Cauterets, Hautes Pyrenees, July 12

Curious Rainbow

AN unusual atmospherical effect was witnessed here to-day, which I had a good opportunity of observing. The sun was about 8° from the horizon, shining brightly upon a heavy shower which had a background of dark clouds. The result was, of course, a double rainbow of remarkable brilliancy. In addition, however, to the ordinary circular and concentric bows, there was a third of an elliptical form, the two ends of which respectively sprang from the two ends of the inner arc, while the elliptical curve cut the outer arc at each extremity of a chord, which was parallel to, and which intersected the normal radius at a point about two-thirds of its length above, the diameter that formed the common base. The top of the elliptical bow was thus the outermost of the three, but the space between its inner margin and the outer margin of the second bow, although quite distinct, was not large.

The appearance of the third bow was due to light reflected from the sea. The sun being low, the resulting line of reflection was long, and it was the linear character of the source of light which gave the elliptical form to the bow it occasioned.

Dunskaith, Ross-shire, July 10 GEORGE J. ROMANES

CHLOROPHYLL COLOURING-MATTERS*

II.

I THINK there can be no doubt that the spectra of the various yellow substances given in Pl. II., Figs. 3, 4, and 6 of Dr. Kraus's work, are due to a variable mixture of xanthophyll, yellow xanthophyll, and lichnoxanthine. These can be separated, and do occur in different kinds of plants, either alone or mixed in such variable proportions that the spectra of the solutions show the absorption-bands, not only in variable positions, but also much less distinctly in some cases than in others. This difference is ascribed by the author, not to a variation in the relative proportion of two or more substances, each having definite and unvarying characters, but to the modification of one single substance, due to some unknown cause, assigning as a reason for this supposition that the chemical reactions are the same, and that the positions of the absorption-bands vary so gradually from one extreme to the other that no distinct demarcation can be detected. Now this is so very fundamental a question in such studies, and, according as it is decided, would modify the conclusions so much, that it is requisite to discuss it somewhat fully. No doubt the position of the absorption-bands seen in the spectra of solutions in different liquids does differ very considerably, but I feel persuaded that the spectrum of the same chemical compound, dissolved in the same liquid, is the same in all cases; and that, if there is any difference between the spectra of two similar solutions, it is due to a difference in the substances themselves. I would restrict the term *modification* to those changes sometimes produced by the action of weak alkalis or acids, or by deoxidising reagents, which are only of a temporary nature, so that when the solution is restored to its original state, the spectrum is seen to be just as at first. We really do require such a term, and I have myself constantly used it in this sense. There is, however, no such relation between the different colouring-matters belonging to what I have called the xanthophyll group; and, though the presence or absence of oily substances may, and sometimes does, materially influence the position of the absorption-bands seen in the spectra of plants themselves, yet, when dissolved in a relatively large quantity of a solvent, this effect is altogether overcome. As I have shown in my late paper the position of the absorption-bands in the different members of the xanthophyll group is very different, and yet it would be easy so to mix them as to have a perfect series of connecting links, and in my opinion the variations from what appear to be independent compounds may be explained in an extremely simple and satisfactory manner, without supposing that the optical characters are subject to any such variations as are ascribed to them by the author. Whenever I have met with these variations I have looked upon them as presumptive evidence of there being a mixture, and have always been able to prove the truth of this principle by subsequent conclusive experiments. The following example will serve very well to explain my views. Many yellow flowers are coloured by a variable mixture of what I have called xanthophyll, yellow xanthophyll, and lichnoxanthine. The former occurs separately in the Alga, *Porphyra vulgaris*, the second in such pale yellow flowers as the yellow *Chrysanthemum*, and the last in the yellow fungus, *Clavaria fusiformis*. The absorption-bands of these two kinds of xanthophyll are in a very different position, and the lichnoxanthine gives no bands, only an uniform absorption, extending over about one half of the spectrum from the blue end. The chemical reactions are also equally distinct. On dissolving each in absolute alcohol, and adding a little hydrochloric acid, the first fades slowly, without being first changed into another yellow substance, and without turning blue or green; the second is first altered into another yellow substance, giving a spectrum with two absorption-bands in a different position, and then turns to a deep blue, whilst the last remains unchanged for a much longer time, and fades very slowly. Now, of course, if all these were mixed together in variable quantities, we should get results varying according to the relative amount of each. The absorption-bands due to the two kinds of xanthophyll would lie in an intermediate position, according to the relative amount of each constituent, and would be more or less indistinct, according as there was more or less of the lichnoxanthine; and on adding a little hydrochloric acid to the solution in alcohol the colour would turn to a more or less blue green, and subsequently fade to a pale or deeper yellow, according to the relative quantity of each constituent.

In order to make my meaning more clear, let us suppose that we were to take a mixture of equal quantities of xanthophyll and yellow xanthophyll. Using the notation I have so often explained in former papers, the centres of the absorption-bands of the spectra of a solution in bisulphide of carbon would then be—

Xanthophyll	$6\frac{1}{2}$	8
The above mixture	$6\frac{3}{4}$	$8\frac{1}{4}$
Yellow xanthophyll	7	$8\frac{1}{2}$

Now on exposing solutions of xanthophyll or yellow xanthophyll to the sun both fade, and if examined when very little colour was left undecomposed, the bands would be seen to be in the same position as at first, the solution being in fact just as if a large part of the colour had been removed, or as if it had been much diluted. In the case of the mixture this would not be the case. Xanthophyll is more rapidly decomposed than yellow xanthophyll, so that when very little colour was left the bands would be no longer in the original position, but in the same place as those of yellow xanthophyll, showing that a small quantity of this is left, when all the other has been destroyed. If some lichnoxanthine had been mixed with the solution, after longer exposure to the sun no absorption-bands would be seen, only the general absorption due to that substance. Moreover if we took equally deep coloured solutions in absolute alcohol of the same three different specimens, and added a little hydrochloric acid to each, the xanthophyll would fade till it was colourless, the yellow xanthophyll would turn to a fine blue, and the mixture would also turn blue, but of only about half the depth of colour. If lichnoxanthine had been present it would have caused the colour to be green; and, after the blue product had faded, it would remain as a residual yellow. By experimenting with such known mixtures we therefore see that, independently of being able to partially separate the constituents, the evidence of the solution being a mixture consists in the difference in the position of the absorption-bands, in the change in their position, or disappearance, when partially decomposed by light, and in the relative quantity of blue substance formed by the action of hydrochloric acid, and of the residual yellow. Such, then, being the case, we know what kind of methods to employ in studying natural coloured solutions, suspected to be mixtures; and on applying them to the investigation of the solutions obtained from leaves and flowers, I find that they behave exactly like such artificial mixtures, and not only so, but there is generally no difficulty in more or less perfectly separating the constituents, so as to correspond more or less closely with the different substances in their more pure state. The evidence of their being mixtures is therefore as good as could be expected. Kraus seems never to have made such experiments, and yet he strongly criticises what I had said about the existence of several distinct kinds of xanthophyll; but I contend that by adopting the principles I have described, we can completely explain the various facts on perfectly simple principles, without supposing that the optical characters of any single substance are subject to variations from some unknown, and, as I believe, altogether imaginary cause.

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* Continued from p. 204.

The flowers of different varieties of *Eschscholtzia californica* are also a good illustration of my views. The very yellow petals are coloured by yellow xanthophyll, with a very little xanthophyll and lichnoxanthine, and thus correspond with many other similar flowers; but the more orange-coloured petals, and the orange-coloured portions of the yellower petals, contain in addition, another colouring-matter, giving the absorption-band in the green shown in Plate II. Fig. 7, at 1 *d*, of Kraus's work which, however, he did not look upon as evidence of a mixture—merely of what he calls a modification. Now, on exposing such a solution in bisulphide of carbon to the sun, this orange-coloured substance is more rapidly decomposed than the others, and in a while a yellower solution is left, which gives exactly the same spectrum as that due to the colouring-matter from the yellow petals. According to this view of the subject we therefore see that the yellow flowers are of the usual type, and that the more orange-coloured portions of the petals, and the whole of the orange-coloured varieties differ only in there being developed an unusual and independent substance, which in this case is of orange-colour, whereas in the flowers of some other plants, such additional colouring-matters are red or blue, as the case may be, and instead of being allied to xanthophyll, differ in almost every particular.

In conclusion I would say that the yellow colouring-matters, soluble in bisulphide of carbon, which exist in green leaves, are the above-named xanthophyll, yellow xanthophyll, and lichnoxanthine. This is probably the reason why this is also the normal type of yellow flowers, and why only in particular cases one or both of these substances are absent. To this I attribute the statement of the author that the chemical reactions are the same, for he has apparently never examined those plants which yield them in an approximately pure state.

In Pl. III. Fig. 2, Kraus gives a representation of the spectrum of a coloured solution obtained from certain species of *Oscillatoriæ*. This he has named *phycoxanthine*; but I am persuaded that the solution must have contained three perfectly distinct colouring-matters, which can be separated by chemical and photo-chemical methods, and do occur almost, or quite, separately in other plants. For one of these substances I have adopted the author's name phycoxanthine. It may be obtained in the most pure state from the lichen *Peltigera canina*, when growing in such a damp and shady situation, that very little orange lichnoxanthine is developed. When dissolved in absolute alcohol and hydrochloric acid is added, it fades without turning blue. Another constituent of the mixture is what I have called *fucoxanthine*, which occurs quite free from phycoxanthine in *Fucus* and other olive *Algæ*, and even in the same species of *Oscillatoriæ*, growing where there is very little light, as those which contain phycoxanthine, if growing well exposed to the sun. When dissolved in absolute alcohol and hydrochloric acid is added, it turns to a splendid blue. The third constituent of the mixed solution is what I have named *orange lichnoxanthine*, which can be obtained by itself from lichens, and is left when such a mixed solution as described by the author, in bisulphide of carbon, is exposed to the sun under green glass, until the phycoxanthine and fucoxanthine have been destroyed. When dissolved in absolute alcohol and treated with hydrochloric acid it fades very slowly. The relative amount of this is greatest in those specimens of *Oscillatoriæ* which grow very much exposed to the sun and air, and I have found by careful comparative quantitative analyses that the relative quantity of these various substances, which together constituted the author's phycoxanthine, varies in such a manner that, as far as the fundamental colouring-matters are concerned, the same or closely allied species of *Oscillatoriæ*, growing exposed to a varying amount of light, furnish a most interesting series of connecting links between olive *Algæ* and lichens. When their vitality and constructive energy are very much reduced by want of light, their type of colouring closely approaches to that of olive *Algæ*, whereas when they are exposed to much air and light, the type approaches to that of such lichens as *Peltigera canina*. I have met with other analogous cases, and if more extended research should still further confirm the existence of this analogy between the results due to abnormally reduced or increased vitality in the same kind of plants, and the normal characters of lower and higher classes of plants, it would certainly be remarkable, as showing that the vegetative energy of the lower classes is in some way or other of a lower type than that of the higher classes, and would present a striking analogy to the relation between the structure of animals whose development has been arrested, and that of those of lower organisation.

The fact of being able to prove that a coloured solution obtained from a plant is really a mixture of a number of different substances, may at first sight appear to be of very little consequence, but I trust that some of the conclusions deduced from this method of study will justify me in looking upon it as very well worthy of attention. When we come to study the various classes of plants growing under various conditions, with the view of constructing such a general science as that I have named comparative vegetable chromatology, these details become not only of the very greatest importance, but absolutely essential. By making qualitative and comparative quantitative analyses of the colouring-matters, carefully distinguishing the fundamental from the accidental, there seems every reason to believe that the petals and the foliage of plants can be brought into morphological agreement, and many of the leading classes of plants distinguished, and at the same time connected together, so as to form a continuous series, advancing from the lowest classes of animals to the highest classes of plants; whereas, if we were to look upon mixtures as independent colouring-matters, and were not to distinguish well-marked species, the whole vegetable kingdom would appear broken up and disjointed, without any chromatological continuity.

H. C. SORBY

THE NEW LABORATORIES OF THE NATURAL HISTORY MUSEUM, PARIS *

IN order to provide every facility for the higher scientific education, and induce young men to devote themselves to scientific research, the French Government have established a school of advanced study, in the form of a suite of laboratories in which young men receive a practical education *par excellence*; they are trained there in manipulations and dissections, and initiated in all those delicacies of touch, those turns of the wrist, which are traditional in the green-rooms (*coulisses*) of science, but which cannot be taught in the theatre.

Without noticing at present the zoological laboratories under the zealous management of M. A. Milne-Edwards, and through which have already passed several students desirous of taking the degree of licentiate in natural science; or the physiological laboratory, at the head of which is the eminent M. Claude Bernard, or the laboratories of comparative anatomy and geology, we shall take the reader through the Rue de Buffon, into the new buildings which contain the chemical laboratory of M. Fremy, the botanical laboratory of M. Brongniart, and the laboratory of vegetable physiology and anatomy of M Decaisne.

M. Fremy had already, for many years, assembled his pupils in the old Museum buildings, badly lighted, small, confined, where they were very uncomfortable; now, on the contrary, they are installed in a new building where they are furnished with every convenience for their work.

As soon as we enter the court, we find on the right and left, platforms (*paillasses*) in the open air with a glass roof, where all experiments can be made, of a nature to taint the atmosphere of the laboratories. On each side are ranged buildings, one specially intended for beginners, the other for more advanced students. The latter is provided with furnaces, by means of which the highest temperatures may be obtained. Each pupil has his place marked out, his name inscribed upon the frame above his work-table, which is furnished with a set of drawers and a rack for holding the *matériel* appropriate for his special work. The laboratory of the assistant naturalist, M. Terreil, and the preparatory laboratory, are situated in a line with the pupils' laboratory.

Laboratory of Vegetable Physiology in the Paris Museum of Natural History

The bottom of the court opens into a lobby which communicates with the two wings of the building; here are conveniences for depositing the clothes which the students exchange for their working garb on entering the laboratory. A door in this corridor gives access to an antechamber into which open the laboratories of M. Fremy, and that of his special assistant, placed side by side.

The first and second stories of the buildings on the right and in the centre are intended for the botanists of M. Brongniart, who have not yet obtained complete possession; the left wing belongs as yet to chemistry; on the first story is the lecture-hall, on the second the library.

M. Fremy has realised the foundation of a true school of chemistry; not only does he lavish on his pupils his instructions, but he sees that their education is complete. Every day at three o'clock work in the laboratory ceases, and oral instruction begins, the lecture-hall, moreover, being open to the public. M. Fremy gives instruction in general chemistry, with a well-known power of exposition; M. Terreil has charge of analysis; M. Ed. Becquerel, of the Institute, initiates the students in the management of physical apparatus; Jannetaz, assistant in mineralogy, gives instruction in that branch; and lastly, M. Stanislas Meunier, already known by his researches upon meteorites, treats of all the parts of geology which are connected with chemistry. Examinations are held by the lecturers for the purpose of testing the work of the pupils, who are rewarded at the close of their studies with certificates testifying to their diligence and their acquirements.

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* From an article in *La Nature*, No. 1.

All this instruction is absolutely gratuitous. M. Fremy wishes to remain faithful to the old motto of the museum, "Tout est gratuit dans l'établissement," though this excessive liberality is perhaps open to criticism.

Behind the magnificent chemical rooms we found the modest laboratory of M. Decaisne. Descending a few steps we reach a garden set apart to experiments in culture, having on the left a glazed gallery: this is the laboratory of vegetable anatomy and physiology. M. Decaisne superintends and advises the anatomists during his daily visits; M. Dehérain, who is well known for his researches in agricultural chemistry and vegetable physiology, directs the work of the laboratory represented in our illustration. It is a long apartment, perfectly lighted, into which stream the rays of the sun, that plays so important a part in all the phenomena of vegetable life; on the right, ventilators carry off all the strong-smelling gases which the chemist is obliged to employ; long tables, furnished with earthenware vessels, extend along the middle of the apartments as well as underneath the windows. Everything is scrupulously tidy.

This laboratory of agricultural chemistry will no doubt yield to agricultural chemistry important results. The man of science will have here the means of preparing at pleasure true artificial soils; he will see plants of various kinds grow under his eyes; he will nourish them with organic and mineral substances whose composition is known to him. He will follow step by step the various phases of vegetable life; he will study the yet mysterious laws of vegetable life. Indeed it is difficult to state all the powerful resources that are in the hands of the experimenter.

AËRIAL SPECTRES

IN an article on the above subject in *La Nature*, No. 4, M. G. Tissandier gives the following account of what he saw from a balloon on February 16, last.

At mid-day we quitted the earth wrapped in a thick mantle of fog; after traversing the mass of the clouds, we were suddenly dazzled by torrents of light which shot from a tropical sun, a stream of fire, in the midst of an azure sky. Neither the *mer de glace* nor the snowy fields of the Alps, give an idea of the plateau of mist which stretched under the car like a glassy circle, in which valleys of silver appeared in the midst of flakes of gold. Neither the sea at sunset nor the ocean waves when lighted up by the orb of day at noon, approach in splendour this array of circular cumulus, but which have, in addition, "the light that never was on sea or land."

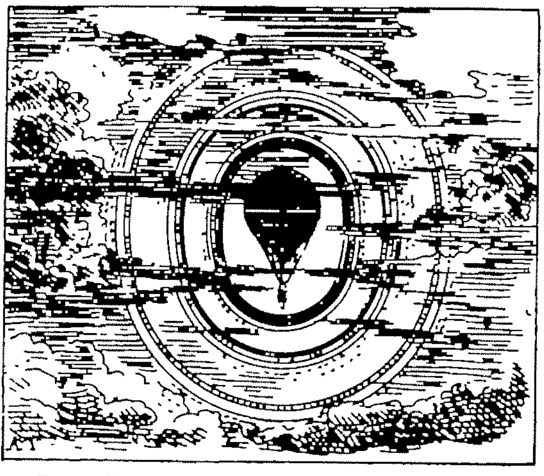

FIG. 1.—Shadow of a balloon surrounded by three aureoles.

When our balloon had passed about 50 metres beyond the plain of clouds, its shadow was projected with remarkable precision, and a magnificent circular rainbow appeared round the shadow of the car. Fig. 2 gives a very exact idea of the phenomenon. The shadow of the car formed the centre of rainbow-coloured concentric circles, in which were distinctly seen the seven colours of the spectrum, violet, indigo, blue, green, yellow, orange, and red. The violet was inside, and the red on the outside, these two colours being at the same time those which were seen with the greatest distinctness. We were, at the time the observation was made, at a height of 1,350 metres above the level of the sea.

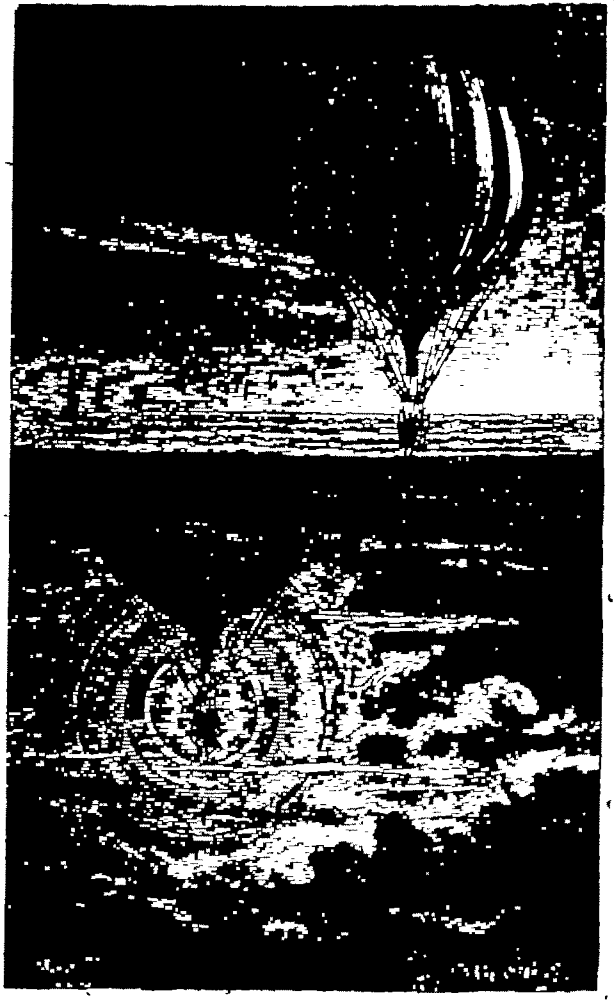

FIG. 2.—Optical phenomenon observed from a balloon.

The balloon, the gas in which expanded under the heat of the sun, continued to rise rapidly in the air, its shadow visibly diminishing; soon, at a height of 1,700 metres, the rainbow-circle enveloped it entirely, and disappeared from around the car. A little later, at about 1h 35m, we approached the bed of clouds, and the shadow was girt this time by three silver-coloured aureoles, elliptical and concentric, as shown in Fig. 1.

Nothing can give an idea of the purity of these shadows, which are cut out in an opaline mist, or of the delicacy of tone of the rainbow which surrounds them. The complete silence which reigns in the aerial regions, where this play of light is seen, the absolute calm which exists there, above clouds transformed by the sun into flakes of light, adds to the beauty of the spectacle, and fills the soul with inexpressible admiration.

We do not yet know exactly to what cause to attribute the production of a luminous contour around the shadow projected upon vapours or mists. Some observers have thought that these phenomena are due to the diffraction of light, but it is possible that they have a common origin with the rainbow. What tends to confirm this opinion is the necessity for the presence of the vapour of water as a necessary condition of the phenomenon: if it is the result of diffraction, it ought to appear as well upon a white wall, or any kind of screen, as upon a cloud. It is possible, moreover, to study these curious phenomena by means of experiments upon the earth; by suitably arranging screens of silk or muslin saturated with water. which resemble a cloud, we may expect to be able to produce the phenomenon. M. Leterne points out another excellent method of studying it. On a spring morning, when the sun, about 15 or 20 degrees above the horizon, has warmed the atmosphere a little, and has produced a light condensation of vapour upon the grassy borders of the roads, one may see his silhouette projected upon the humid verdure, surrounded by a luminous contour, in which is seen the colours of the spectrum, the red, however, being strongest.*

THE GEOLOGICAL SURVEY OF INDIANA

GEOLOGY is a branch of Science which specially commends itself to the fostering care of Governments, paternal or otherwise. More particularly is this true of a new country, where, in the imagination of the settlers, untold wealth has yet to be dug out of the earth, if only they could discover in what quarter best to look for it. Accordingly, in not a few of our colonies and in a number of the States of the Union, geological and mineralogical surveys have long been at work, originated and continued at the public expense. In most cases, of course, the first aim of such surveys, and in fact the very justification of their existence in the eyes of practical and by no means scientific legislators, is the finding of mineral wealth. If they were begun from the lofty scientific point of view they would fail, and deservedly. But when a really able scientific man gets the charge of one of them, and has at the same time that mother-wit and knowledge of the world which scientific men so often lack, he may not only attend to the rigid economics of his paymasters, but do great service to geology. His aim is to show the public that a strictly scientific basis is the only one on which a mineral survey to be of any value can be conducted. And this is so obvious that if it is simply and clearly stated, it for the most part commends itself to the common-sense of public men. In laying this necessary basis and then in carrying out the survey for economic minerals the geologist may both pave the way for an enormous increase to his country's industry and wealth, and add much of permanent interest and importance to the common stock of geological knowledge.

Perhaps the most notable illustration of the successful accomplishment of this double mission is furnished by the career of Sir William Logan, whose practical kindly ways enabled him to triumph over the shortsightedness of colonial obstructionists, and whose patient and sagacious labours among the rocks of Canada have made his name honoured and familiar all over the world, and have conferred distinction also upon his country. In the United States, too, fostered by the liberality of the Legislatures, a number of admirable State surveys have been made, or are still in progress. Under the auspices of such men as James Hall, Owen, the Hitchcocks, the brothers Rogers, Hayden, Whitney, Blake, Cook, and others, not only have maps been constructed, but elaborate reports have been published, embracing, in addition to the paramount economics, much valuable information in geology, mineralogy, and palæontology.

One of the latest of these State surveys is that of Indiana, which was started some four years ago under the direction of Prof E. T. Cox. Like those already referred to, it was organised by the authorities "for the purpose of collecting information designed to promote the interests of agriculture, arts, manufactures, and mining." But it was furnished at the same time with an analytical laboratory "for analysing such ores and substances as may be deemed useful to the State," and with space "to build up a geological and natural history cabinet," while in order to render its labours as speedily serviceable as possible, an annual report of progress was required to be issued.

Prof. Cox has evidently a hard task before him. He has been invited to become a kind of depository of all the mining information in the State. He is to see that trustworthy mineral surveys are made, and at the same time he is expected to look after the laboratory and infant museum at Indianopolis and—perhaps most laborious but not least useful of all—to receive everybody who wants to know about coal, iron, or other mineral produce, and to collect and furnish to such inquirers all the information procurable. He generously says in one of his reports that this latter part of his duties "has always given him pleasure," though he confesses that it has consumed a considerable portion of his time. Fortunately he can count on the help of a small but apparently able staff of assistants, and notwithstanding all the obstacles in his way he has succeeded in getting through a large amount of work which, though not yet of high scientific value, must bear most importantly upon the future development of Indiana.

Three volumes of reports with maps have been published, bringing the account of the progress of the Survey up to the end of last year. Each of these neatly printed and not too bulky octavos describes several counties of the State with reference chiefly to the distribution of economic minerals; and the maps which accompany it, though roughly and cheaply executed, are clear and must be of infinite service to the many speculators and others who every year come in increasing numbers into the state in search of mineral investments. The coal-field of Indiana, though only a part of the larger basin of Illinois, is estimated to equal more than half of the area of the whole of the coal-fields of Great Britain and Ireland. Some of the coal-seams are of excellent quality, specially that known locally as "block-coal," which is said to be unrivalled for iron-furnaces. Abundant iron ore likewise occurs. Hence not only coal-pits but iron-works are springing up in rapidly increasing numbers. Not a little of this wonderful rapidity of growth is attributed by Prof. Cox, and no doubt justly, to the extended and more accurate knowledge of the minerals which the Survey has been able to publish. In the course of two or three years tracts of "primeval forest" have vanished, and in their place the visitor would now see clanking engines and mining villages, crowded with a population as busy and begrimed as any to be met with in Staffordshire or Lanarkshire. And yet vast though this change is, it may be said to have only just begun. Before many years are over the coal-bearing part of the formerly quiet agricultural state of Indiana will become one of the most active centres of industry in the Union, with railways diverging in all directions to carry away its mineral produce.

Prof. Cox and his assistants have not only been successful in pointing out the mineral resources of the various counties. In looking through his reports one can see that he continues from year to year to slip in more of general scientific interest. This is notably the case with the volume lately published. In addition to a series of elaborate analyses of coals, we find that in the coal-pit sections the names of characteristic fossils have found their way into the text, that notices are given, not merely of the economically useful minerals, but of the geological formations which have no special industrial value,—Silurian, Drift, River-terraces, &c. The volume contains also meteorological tables and notices of recent geological changes. But by far the most interesting contribution to science in its pages is a "Report on the Wyandotte Cave and its Fauna," contributed by Prof. E. D. Cope, with an account of the geology of the cave, by Prof. Cox himself. This remarkable cavern runs through the "sub-carboniferous" limestone in numerous branches which are said to have a total length of twenty-two miles, and greatly to excel the more famous Mammoth cave of Kentucky in the number and beauty of their stalactites. It contains a peculiar fauna, numbering at least sixteen species, which show a general resemblance to those of the latter cave, and include one species of blind fish (*Amblyopsis spelæus*) which lives in the subterranean waters of Kentucky.

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* *Comptes Rendus*, t. lxxvi. p. 786.

In these Reports each county is described separately, so that the same geological facts require to be frequently repeated. This is, doubtless, the most useful arrangement for those for whom the volumes are primarily intended. But it would be a service to other readers if a good table of contents were given, and if the index were made much fuller, especially in matters of general geological interest. The volumes are eminently praiseworthy, and we hope to see them followed, before long, by a good map and a general geological Report of the whole State of Indiana.

A. G.

INTELLECT OF PORPOISES

A SINGLE visit to the Brighton Aquarium would suffice to convince a recent correspondent, Mr. Mattieu Williams, that the intellect of the porpoise, as foreshadowed by its convoluted brain, exceeds, beyond comparison, that of the cod-fish or any other representatives of the piscine race. Of the two specimens now inhabiting the largest tank in the building, over one hundred feet long, the first-comer so readily accommodated itself to its altered conditions, that on the second day it took its food, smelts and sprats, from its keeper's hand, and has continued to do so ever since. The later arrival was, at first, less sociably inclined; but both have latterly become equally tame, and frequently, while receiving fish from my hand with the gentleness of pet dogs, have permitted me to pat and stroke their slippery india-rubber-like backs.

During feeding-time it is amusing to watch the avidity with which these porpoises take their food; one, the more active of the two, usually securing the lion's share, and displaying marked sagacity by frequently snatching a second or third morsel before disposing of the first.

The keeper in charge of these interesting animals is now in the habit of summoning them to their meals by the call of a whistle; his approaching footsteps, even, cause great excitement in their movements, and recent experiments have proved them to be acutely sensitive to the vibrations of sound. By the physiologist a more pleasing spectacle can scarcely be witnessed than the graceful actions of these cetacea, as they swiftly pursue their course up and down their spacious tank, ascending to the surface of the water at intervals of fifteen or twenty seconds, to breathe, each inspiration being accompanied by a spasmodic sob-like sound, produced by the rush of air as a breath is rapidly liberated and inspired through the single central blow-hole.

Onward progress is effected in these animals, as in all other cetacea, exclusively by the action of the horizontal caudal fin; the development of muscle at the "wrist" of the tail on which this action depends being enormous and plainly visible externally; the pectorals are devoted principally to the purpose of steering the creature to the right or left, aiding it also in rising to the surface of the water.

The fact alone of the porpoise suckling and evincing much maternal solicitude for the welfare of its young indicates the superiority of its position in the zoological scale above that of the other representatives of the finny tribe; and to this, in addition to the remarks just made upon their sagacity when feeding, many other facts may be cited, pointing in the same direction. The curiosity attributed to these creatures, as illustrated by the experiences of Mr. Mattieu Williams, receives ample confirmation from their habits in confinement. A new arrival is at once subjected to the most importunate attention, and, advancing from familiarity to contempt, if disapproved of, soon becomes the object of attack and persecution. A few dog-fish, *Acanthias* and *Mustelus*, three or four feet long, placed in the same tank, soon fell victims to their tyranny, the porpoises seizing them by their tails, and swimming off with and shaking them in a manner scarcely conducive to their comfort or dignified appearance, reminding the spectator of a large dog worrying a rat. The fine sturgeon, six feet long, now sharing an adjoining tank with the cod, was first placed with these animals, but in a short time was so persecuted that for safety it had to be removed; while to this day the lacerated condition of its tail bears witness to the pertinacious attention of its former comrades. Some large skate (*Raja clavata* and *maculata*), while they maintained their usual habit of lying sluggishly on the floor of the tank, escaped molestation; but no sooner did these fish display any unwonted activity than the porpoises were upon them, and, making a convenient handle of their characteristic attenuated tails, worried them incessantly. On one occasion I witnessed the two *Cetacea* acting evidently in concert against one of these unwieldy fish, the latter swimming close to the top of the water, and seeking momentary respite from its relentless enemies, by lifting its unfortunate caudal appendage high above its surface. It need scarcely be remarked that the skate were removed before further mischief could be done, leaving the porpoises, with the exception of a few conger, which during the day-time mostly lie hidden in the crevices of the rock-work, turtles, and a huge monk-fish (*Rhina squatina*) sole occupants of this colossal tank.

While far behind the porpoises in display of intellect, it may be hereafter shown that the representatives of the *Gadidæ*, or cod-family, are by no means the least intelligent of fish.

W. SAVILLE KENT

AN INTERNATIONAL COINAGE

A PROPOSITION has been made for holding a private conference for an International Coinage at Vienna in the course of next September, and to consider more particularly the following points:—

1. The question of Valuation.
2. The principal Coins.
3. The Unit of Value, and its Sub-divisions.
4. The charge for Coining, the rate of alloy, and other technical questions.
5. The preservation of the full value of the principal Coins in circulation, and the coining of others.
6. The different modes of introducing a new money-system.

The prime mover and most active agent in the promotion of this conference is Mr. A. Eggers, Consul in Bremen, The declared object is to bring together a limited number of semi-official or private representatives of the various countries, with a view of a full discussion of the subject; and a committee has been constituted consisting of several French and German gentlemen who are interested in the question of the International Coinage.

Mr. Eggers has recently paid a visit to this country with a view of inducing some of the English advocates of an International Coinage to take part in the proposed conference. It was suggested by Mr. J. B. Smith, M.P., that a private meeting should be held to enable Mr. Eggers to explain his views, and this meeting was accordingly held on the 25th ult. at the Standards Office, 7, Old Palace Yard. But few persons, however, attended; amongst them were Dr. Leone Levi and Mr. Hendricks; Mr. J. B. Smith was himself absent from illness.

The principal propositions of Mr. Eggers, which seem to be fully explained in his printed pamphlet, entitled "Die Geldreform," published at Berlin, were—

1. That the International Coins should be of a round metric weight.
2. As common units of value, a dollar of fine gold $1\frac{1}{2}$ gramme, and a coin of 25 grammes of silver $\frac{9}{10}$ fine.
3. As nearly corresponding with the pound sterling, a coin of 5 dollars, or a new sovereign of $7\frac{1}{2}$ grammes of fine gold.

And he suggested that such a gold dollar and sovereign might be first introduced in Canada, as very nearly agreeing in value with the American gold coinage.

The objections raised against these propositions were, first, that if the fine gold in the dollar weighed $1\frac{1}{2}$ grammes, the addition of $\frac{1}{9}$ alloy would make the actual weight of the dollar $1\frac{2}{3}$ grammes, which is not a round metric weight. There would be the same result with the new sovereign of $7\frac{1}{2}$ grammes fine gold, as $\frac{1}{9}$ alloy would make the actual weight $8\frac{1}{3}$ grammes.

A far more serious objection was that the difference between the $7\frac{1}{2}$ gramme fine gold in the proposed new sovereign, and 7·32238 grammes in the existing sovereign, equal to 0·17762 grammes, would increase the value of the sovereign more than $5\frac{1}{2}$*d.*, which was quite inadmissible.

The question of a silver International Coin was not discussed, the general opinion being that the difficulties of agreeing upon a single gold unit were already sufficiently great, and that until they could be overcome, it was almost hopeless to expect that any International Coinage could be established. The adoption in the German Empire of the 20-mark piece as the gold coin unit, and containing 5·04*d.* less in value of fine gold than the sovereign, together with the very large amount of the new German gold coinage, appears to offer at the present time an insuperable obstacle to the common adoption of an International Coinage, however desirable it may be.

NOTES

AT the meeting of the Paris Academy of Sciences on the 7th instant, three elections to the Section of Anatomy and Zoology took place. The places to be filled were those of Mr. Agassiz, elected a Foreign Associate, and MM. Pictet and Pouchet, deceased. In the first case M. Steenstrup obtained 38 votes and Mr. Darwin 6; in the second Mr. Dana obtained 35 and Mr. Darwin 12; in the third Dr. Carpenter obtained 35, Mr. Darwin 12, and Mr. Huxley 1 vote. Messrs. Steenstrup, Dana, and Carpenter were therefore declared duly elected.

THE Professorship of Anatomy at King's College, London, rendered vacant by the death of Mr. Partridge, was refilled on Friday last by the appointment of Dr. Curnow, a former student of the College, whose medical career at the University of London has been one of the most brilliant on record. After having obtained the scholarships and gold medals in Anatomy and Materia Medica at the first M.B., he was equally successful at the second M.B., gaining the same honours in Medicine and Obstetric Medicine. At the M.D. examination Prof. Curnow also obtained the gold medal. We cannot but think that the Council of King's College have made a judicious selection, and have gracefully recognised talent in one of their most promising pupils.

THE Royal College of Science for Ireland, in connection with the Science and Art Department, South Kensington, has conferred the diploma of associate on the following gentlemen:—Faculty of Engineering: G. P. Culverwell, E. P. Culverwell, R. W. Frazer, and E. Barrington. Faculty of Manufactures: Thomas Abbott. The two Royal Scholarships were awarded to John O. Hicks and James Patterson. The silver medal to F. A. Caldwell.

"IT never rains but it pours." Prof. Agassiz, as representing the Anderson Natural History School, of Penikese Island, has been presented by Mr. C. W. Galloupe, of Swampscott, with a handsome yacht of 80 tons, estimated to cost 20,000 dollars. The vessel will be used for dredging, temperature soundings, &c., along the coast in the neighbourhood of the island; its presentation makes perfectly complete the apparatus for practically training the students of the finest natural history school in the world.

AMONG the "Innocents" slaughtered yesterday in the House of Commons we are sorry to notice the Weights and Measures (Metric System) Bill, which was withdrawn by Sir Thomas Bazley, in the absence of Mr. J. B. Smith. No notice had been given of this step, which naturally drew forth some protests.

THE Report of the College of Physical Science of Newcastle-upon-Tyne, at the end of the second year of its existence, is altogether satisfactory. The classes have been augmented from four to eleven, and the number of students shows a considerable increase over the previous session; the attendance at the evening classes is also satisfactory. The number of students attending instruction in practical chemistry has been so great as to render it necessary to make arrangements for materially increasing the laboratory accommodation. The Council are very sanguine of the success of the college, though they feel the necessity of founding more professorships and obtaining more accommodation, and think that the wealthy manufacturers and merchants of Newcastle and the North of England ought to render much more assistance than they do. We hope the wealthy manufacturers of the North will see it to be their duty, as it certainly is their interest to contribute to the success of such an institution in their midst. It would certainly be a disgrace to Newcastle if its Science College should, in the midst of enormous wealth, not attain the greatest possible measure of success. There is no reason why this institution should not be made as successful as Owens College, Manchester, and we hope that ere long similar institutions will be established in all the large towns of England. It would be a pity that those who are concerned in the management of the Newcastle institution should mar its success by any antiquated restrictions as to a knowledge of ancient languages by those who have shown themselves deserving of a degree in science.

WE regret to announce the death of the eminent engineer, Mr. J. R. McClean, M.P., F.R.S.

OUR readers have no doubt heard of the recent miserable thefts of living Italian coral from the Crystal Palace Aquarium. It is really difficult to find words to characterise the despicable meanness of the act. Mr. Lloyd says that these things are never taken when working people are present. Meantime the public must suffer for the act of an individual, for it has been thought necessary so to secure the corals under lock and key, that they cannot be so well seen as before, when in open tanks. We can only hope that the petty thief will be discovered: happily such acts are rare in our places of public resort.

A new part of the quarto "Transactions of the Zoological Society," just issued, contains three papers by Prof. Owen. The last of these is of special interest, as containing the first account of a new extinct Struthious form from Australia, proposed to be called *Dromornis australis*, for the full description of which we must refer our readers to the paper in question.

The post tertiary fauna of Australia is extremely rich in *Macropodidæ*, or Kangaroo, many of which greatly exceed any of the existing species in size. Professor Owen has lately described a large series of these in a memoir presented to the Royal Society, and has divided them into numerous genera, founded upon somewhat minute distinctions in the characters of the teeth. We have just received from Mr. Gerard Krefft, Curator of the Sydney Museum, a photograph of the teeth of a giant of the race, the four molars together measuring from before backwards as much as three inches. It is unaccompanied by any description, and pending the publication of Prof. Owen's memoir, we are unable to say whether it belongs to either of the species described therein.

The tank containing the Spring Lobster or Sea Crayfish, *Palinurus vulgaris*, at the Brighton Aquarium, No. 26, is invested with special interest at the present moment, on account of the appearance, during the last few days, of innumerable young. Until within late years, the early condition of this, the largest of our British crustacea, was regarded as a distinct species, allied to *Squilla*, representing the Stomapodous instead of the Podopthalmous order of their class; it was thus described by Leach under the name of *Phyllosoma commune.* The celebrated Belgian naturalist, Prof. Van Beneden, was one of the first to establish the identity of these two forms, and the result of his praiseworthy investigations was simply and amply confirmed by the recent arrivals at the Brighton tanks. In this "*Phyllosoma*" phase, the ovate body is so remarkably transparent and flattened out, that even when several inches in length they can scarcely be distinguished at the surface of the sea, where they often float in countless numbers. Some very fine examples of these crustacea, illustrating this interesting stage of their development, are exhibited in the typical invertebrate series in the Royal College of Surgeons. The specimens at the Brighton Aquarium just excluded from the egg are very minute, scarcely exceeding half-an-inch in total length, and although swarming in their tank are, on account of their extreme pellucidness, only visible on the most close inspection. The "berried hen" producing this large brood of young, was added to the collection about a month ago. An adjoining tank, No. 28, is teeming in a similar manner with the young of the Common Lobster, *Homarus vulgaris.*

The number of the "Proceedings of the Asiatic Society of Bengal," containing a report of the annual meeting, has just been received. The chief feature of this meeting was the admirable address of the president, Dr. T. Oldham, from which we are glad to see that under the auspices of this Society, a very large amount of valuable work continues to be done to the literature, archæology, ethnology, and natural history of India. For years the Indian Government ignored the acknowledged claims which this Society had upon it, in return for the Society's handing over to Government its invaluable collection. It is gratifying to be told by the president that the Government of India have acceded in full to the claims of the Society. This gives us some hope that the Government, who have, the president tells us, sanctioned the necessary expenditure for photographic observations of the forthcoming Transit of Venus, will, as the Society desires, maintain and render permanent the small establishment about to be fixed for this object on some elevated spot, for the special purpose of solar observation in connection with meteorology. The British Association at its last meeting requested the Society to urge the Indian Government to establish and maintain an observatory for this purpose in India. The direct value, both to science and to commerce, of the work of such an observatory would be incalculable; and we hope the Society will continue importunate until the Government accede to its wishes. We are moreover glad to see that a committee of the Society has been organised to supplement the work of the *Challenger* by exploring the Indian seas, an almost virgin soil; the necessary funds for the purchase of instruments have been granted, and we hope the ship, which is all that is wanting, will be forthcoming when the instruments are ready. Altogether the Society must be congratulated on the work it does amid many discouragements.

Telegraphic intelligence has been received in Berlin announcing that the English steamer conveying the German African exploring expedition to Congo has been wrecked off Sierra Leone. There was no loss of life, but all the effects and scientific instruments of those on board were lost.

Shocks of earthquake occurred on the morning of July 12 at Rome, Frosinone, Alatri, and several other places. No damage was done. The shocks and subterranean roaring continue in the neighbourhood of Alpago. A rather strong shock of earthquake occurred on the same day in the Valley of Lira, at Isola. The workmen left the manufactories, and several houses were damaged.

Mr. J. L. Hadden, C.E., who was blinded by watching the electric light at Constantinople, is reported as having recovered.

On June 15, according to the official journal of the Viceroyalty of Konieh, in Asia Minor, snow fell heavily on the mountain called Bulgardagh, in the Kaza of Erkeli. In some places the snow was five feet deep.

We have already referred to the U.S. exploring expedition to Montana, in connection with the survey for the Northern Pacific Railroad. The correspondent of the *New York Tribune*, writing from Fort Rice in the Upper Missouri, near a newly-founded town called Bismarck, gives details concerning the organisation of the expedition, which was expected to set out from Fort Rice at the end of June. There is a large military escort as a protection against the Indians, and the scientific party is well equipped. It is expected that the waggons which carry out supplies will return loaded with specimens of the natural products of the region, especially of the Yellowstone Basin, to be arranged systematically, and deposited in the National Museum of the United States. The results of this expedition, so liberally fitted out by the American Government, are likely to be of great service to science.

The *Times of India* contains an account of the death of a huge boa-constrictor which infested some marshy ground at the foot of the hills near Poodoocottah. The animal was regarded as sacred by the natives, who would not molest it, although only on the morning when Dr. Johnstone and Mr. Pennington, with great danger to themselves, bravely hunted it up and shot it, it had swallowed a young child. The animal is about 21 feet long, and its stuffed skin is to be deposited in the Madras Museum.

As might be expected, Mr. G. J. Symons' "British Rainfall for 1872," considering the unusual wetness of the year, is of great interest to meteorologists. The author deserves great credit for the immense trouble he has taken in putting together in a handy and useful form such a multitude of statistics, and the great care he appears to have taken to secure accuracy. The greatest rainfall in the three kingdoms during 1872 occurred at The Stye in Cumberland, 1,077 ft. above the sea-level, where it reached the extraordinary amount of 243·98 in.; the smallest amount was at Silsoe in Bedfordshire, where it was only 26·18 in., unusually small as compared with most other places. The volume, besides rainfall statistics, contains much that is of interest to meteorologists, including some statements on the supposed connection between rainfall and sunspot frequency, that are worthy of attention.

"THE U.S. Sanitary Commission in the Valley of the Mississippi during the War of the Rebellion, 1861—1866," is the title of a very interesting volume, giving a detailed account of the organisation and working of this benevolent commission during the American civil war. It seems to have been on the whole well organised and successful in carrying out its object, thus doing much to alleviate the miseries of that unfortunate war.

MR. FREDERICK AYRTON, barrister-at-law, long resident at Cairo, who died in London recently, has bequeathed to the British Museum a splendid library of caligraphic writings in Arabic, Persian, and Turkish, collected during many years' residence in Egypt, and the market value of which probably exceeds 3,000*l.* Mr. F. Ayrton was a perfect connoisseur in the Oriental science of caligraphy, of which so little is known, artistically, in Europe; and he devoted time and money, without stint, to this his favourite study. His collection is, perhaps, unrivalled in Europe. The gift is made on condition that the trustees set apart a room in the Museum for the exhibition of these specimens of Oriental caligraphy, and that Mr. Ayrton's Arabic scribe, Asaád Effendy, be engaged for three or four years, at a salary of 100*l.* per annum, to draw up a catalogue *raisonné* of the contents of each series.

"LES Richesses Naturelles du Globe à l'Exposition Universelle de Vienne," by M. Bernardin, is the title of a short pamphlet called forth by the Vienna Exhibition, the author's object being to show that most of the industrial materials obtained from the animal, vegetable, and mineral kingdoms within the last forty years have been lighted upon by chance, and that if competent men were to make a thorough investigation of the subject, Nature might be made to contribute to industry a vastly greater amount of material than she at present does.

WE learn from Trübner's *Literary Record* that M. Alphonse Pinart has just published a catalogue containing a description of the different collections made during his stay in what was formerly Russian America (Alaska), brought to Europe, and is now exhibiting in one of the galleries of the Museum of Natural History, Paris. The collection comprises objects of Natural History in general, Palæontology, Conchology, and especially a rich collection of objects of high ethnographical interest, as costumes, tools, arms, &c., used by the aborigines of Alaska.

WE are indebted to *Iron* for the following:—During the recent building of a bridge in Holland one of the traverses, 465 feet long, was misplaced on the supports. It was an inch out of line, and the problem was how to move it. Experiment proved that the ironwork expanded a small fraction of an inch for every degree of heat it received. It was noticed that the day and night temperature differed by about 25°, and it was thought this might be made to move the bridge. In the morning the end out of place was bolted down securely, and the other end left free. In the heat of the sun the iron expanded, and towards night the free end was bolted down, and the opposite end was loosened. The contraction then dragged the whole thing the other way. For two days this experiment was repeated, till the desired place was reached. We find no record that the heat of the sun has ever been employed in this way before.

THE following is from *Ocean Highways*:—During the last three years a naval party, commanded by Lieutenant Simpson, has been employed by the Chilian Government to explore the western side of Patagonia. In November and December 1871, Lieutenant Simpson, whose narrative has only just been published, ascended the river Aysen, which falls into the sea in latitude 45° 20′ S., opposite the Chinos Archipelago, to the south of Chiloe. He soon came to rapids and waterfalls which stopped his boats, but he pressed on through the forest in pouring rain on foot, and crossed the Cordillera at a point where it has never before been visited. The country had no inhabitants, but it is well wooded, and signs of coal were found.

No. 5 of the "Lecture Extras" of the *New York Tribune*, contains seven lectures with numerous woodcut illustrations. The principal lectures are, "Sound and Hearing," "Voice and Speech," and The Explanation of Musical Harmony," by Prof. Elsberg, of the University Medical College, New York, "Deep Placer Mining in California," by Prof. Benjamin Silliman, of Yale College, and "The Seven Senses," by Dr. R. W. Raymond, U.S. Mining Commissioner.

ADDITIONS to the Brighton Aquarium during the past week:—3 Green Turtle (*Chelonia viridis*), 4 Green Lizards (*Lacerta viridis*), 45 Mackerel (*Scomber scomber*), 3 Sea-trout (*Salmo trutta*), 4 Bass (*Labrax lupus*), 8 Black Bream (*Cantharus lineatus*), 3 Shad (*Clupea Alosa*) 1 Scad (*Trachurus trachurus*) 2 Octopus (*O. vulgaris*), 2 bunches of spawn of Squid (*Loligo vulgaris*), a brood of young Lobsters (*Homarus vulgaris*), hatched in tank No. 28.

THE additions to the Zoological Society's Gardens during the past week include a Mississippi Alligator (*Alligator mississippiensis*) from New Orleans, presented by Mr. John Hanley; four blossom-headed Parrakeets (*Palæornis cyanocephala*) and an Alexandrine Parrakeet (*P. alexandri*) from India, presented by Mr. Hugh Nevill; six Zenaida Doves (*Zenaida amabilis*) from the West Indies, presented by the Right. Rev. Dr. Stirling; a Tabuan Parrakeet (*Pyrrhulopsis tabuensis*) from the Feejee Islands, and a Wagler's Conure (*Conurus wagleri*) from Venezuela, both new to the collection; an Eland (*Oreas canna*) from South Africa, purchased; two Crested Porcupines (*Hystrix cristata*) born in the Gardens.

*ON THE GERM THEORY OF PUTREFACTION AND OTHER FERMENTATIVE CHANGES.**

II.

THE author next proceeded to describe and illustrate, by diagrams enlarged from camera lucida sketches, some of the variations he had observed in organisms found in the milk glasses when introduced into other media. Another unnamed species of Oidium closely allied to that before referred to, and like it operating as a putrefactive ferment upon urine, was seen to present strange varieties according to the fluid in which it grew and the length of time it remained in it; yet, when placed in boiled milk, it returned to exactly the same character which it had when in the flask of unboiled milk in which it was first observed. But still more remarkable modifications were seen among the Bacteria. One species of very large size, but of ordinary form and movements, as seen first in the milk, presented the following, among other varieties. In Pasteur's solution it grew as motionless algoid threads with nucleated segments. In urine and turnip infusion it did not grow at all, nor did it in the albuminous fluid till boiled and cooled solution of sugar of milk had been added, when it returned to its original Bacteric form at first, but afterwards assumed the characters of a toruloid organism. In boiled milk it resumed the original Bacteric character, but, after seven weeks, the Bacteria had changed from very large to excessively minute ones.

Another species, seen in the first instance in milk, as about the most minute form of Bacterium the author had ever observed, grew in Pasteur's solution as an ordinary full-sized Bacterium; but in urine it assumed the unjointed and cork-screw shape, and the spiral movements of a Spirillum. In turnip infusion it grew with extreme rapidity as an ordinary double-rod-like moving Bacterium, but after remaining some weeks in that medium it assumed a remarkable fungoid character with greatly increased diameter, which on introduction into urine reproduced the moving Spirillum, now of very large size, and sometimes remarkably branched, but as time passed gradually growing a smaller and smaller progeny as the liquid became vitiated, till at length it lost in the urine its spiral shape, and returned to the appearance of the minute ordinary Bacterium first seen in the milk. These may serve as samples of this class of observations, which proved on the one hand how utterly fallacious are any descriptions hitherto given of Bacteria according to form, size or movement, yet, on the other hand, showing that the different Bacteria, like the different Oidia, retained amid all their variations their distinct specific characters.

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* Continued from p. 214.

The fermentative changes induced in the media by the introduction of the various organisms were next alluded to. The test-tubes of the experiment with unboiled milk were shown, and it was pointed out that each different organism was accompanied by a different appearance of the milk, implying that each was associated with a special chemical change in the fluid in which it grew. An enlarged sketch was also exhibited of the boiled milk glasses as they were seen some weeks after they had been inoculated with the various Bacteria, showing that no two of those glasses were alike. In that containing the Bacteria derived from a drop of tap-water introduced into urine the milk had changed to a beautiful green colour; that with the kind which formed the Spirillum in urine was a pure white curdy mass, sharply acid to test-paper, while a third, inoculated with a curious irregular form of Bacterium from another of the milk-flasks, was of umber brown colour. This glass was brought to the meeting because it was of especial interest, not only on account of its peculiar tint, but because it was an instance of a primary alkaline fermentation of milk. Another milk glass had been inoculated with the same organism, and had undergone the same change, assuming in a few days the same umber brown colour, accompanied by powerful alkaline reaction. This particular Bacterium was in some forms undistinguishable from pairs of granules of a form of "Granuligera," which occurred in one of the milk glasses associated with the large Bacteria above mentioned; but the Granuligera having been obtained unmixed by introducing it successively into liquids which permitted its growth, but not that of the Bacterium, it proved to be a feeble acid ferment of milk, not producing any effect upon its colour. One of the glasses sketched was of peculiar interest, because it contained a large motionless Bacterium, which had been the sole product of exposure of a glass of the boiled milk for an hour in a sitting room, the fungus spores that in all probability entered with it having been prevented from developing by the growth of the Bacterium. It happened that the Bacterium thus derived from the air refused to grow in Pasteur's solution, urine, or turnip infusion, so that if the experiment had been performed with either of those fluids, it would have afforded negative results as regards the Bacterium, though fungi would probably have appeared; and this might have been quoted as a good illustration of absence of Bacteric development after atmospheric exposure.

The Oidium, which, as before mentioned, was a powerful putrefactive ferment of urine, produced scarcely any effect on milk, which had remained unchanged in flavour for seven weeks, although converted into a thick mass, not by coagulation of the casein, but simply by the dense jungle of the fungus filaments, while test paper indicated merely a very faint increase of alkaline reaction. The fluid remaining thus unimpaired in quality, explained the luxuriant growth and healthy appearance of the fungus in it, contrasting strikingly with its characters in urine, in which it rapidly occasioned putrefaction, and then formed merely a scum of toruloid rounded cells.

In describing these facts, the author did not affect the circumlocution that would be necessary in order to avoid using the language of the germ theory. As stated at the outset, his original object in the investigation had not been to prove that theory, but to throw light upon the nature and habits of the fermenting organisms. Nevertheless, for the sake of any who might still entertain doubt upon the question, it might be well to point out that the facts which had been adduced were irreconcilable with any other view. It was plain that they utterly disproved the oxygen theory, while they indicated with sufficient distinctness that all instances of so-called spontaneous generation had been due simply to imperfect experimentation. It remained to consider shortly the only other rival theory, the somewhat specious one of chemical ferments. After pointing out some of the inconsistencies of that theory with the facts observed, and how its difficulties became increased with the discovery of every new organism with its corresponding chemical change, requiring the assumption of a new and purely hypothetical chemical ferment, the author reminded the Society that in truth there was not a fact in chemistry to favour the belief that any substance destitute of vitality possessed the one faculty which distinguished all true fermentation, viz. the property of self-propagation of the ferment. Perhaps the most remarkable instance of a chemical ferment was the resolution of the amygdalin of the bitter almond into the essential oil of bitter almonds, hydrocyanic acid, formic acid and glucose under the influence of emulsin. The amygdalin neither gained nor lost a single atom, but was simply broken up into new compounds under the influence of the peculiar albuminous principle emulsin. But did the emulsin undergo multiplication as in the true fermentations? On the contrary, it had been shown by Liebig and Wohler in their original paper[*] that a certain weight of emulsin would only break up a limited quantity of amygdalin, and that the emulsin when afterwards separated no longer affected amygdalin. So far from having the property of self-propagation, it lost its catalytic power in the act of catalysis. Thus the chemical ferment theory was in truth utterly destitute of scientific basis as explaining true fermentation.

Such being the case it was contended that the germ theory must now be regarded as demonstrated; viz. that putrefaction and other true fermentations characterised by indefinite multiplication of the ferment are caused by the growth of living organisms, which, while capable of great variations according to the circumstances in which they are placed, retain their specific characters like larger plants, and like them spring only from pre-existing similar organisms.

Nevertheless the so-called chemical ferments had a high degree of interest in this question, as very likely playing an important part in bringing about the chemical changes. For just as it was proved that a peculiar albuminous principle, emulsin, existing in the sweet as well as in the bitter almond, but absent from the pea, or bean, or other leguminous plants examined by Liebig and Wohle, could break up as much as ten times its weight of a stable crystallisable substance like amygdalin, so it seemed probable that other peculiar albuminous principles might exist in other plants, such as the fungi, and in like manner break up larger or smaller quantities of other stable organic compounds. In this sense, then, as intervening between the growth of the organisms and the resulting decompositions, the theory of chemical ferments might be welcomed as a valuable hypothesis.

Lastly, the author showed some blood obtained from a horse between three and four weeks previously, in the hope that by exposing the carotid artery antiseptically, and receiving the blood from it into a "heated" vessel, and protecting it from dust, he might, after the clot had contracted, decant off the clear serum, and inoculating or exposing the uncontaminated fluid, observe organisms and fermentations corresponding to those which occur in the practice of surgery.

But to his great surprise day after day passed without the clot showing any sign of shrinking, and it remained still uncontracted. In the flask shown, the buffy coat was seen to be present on the upper part of the still tremulous jelly-like coagulum, but instead of being powerfully pinched together into a comparatively small bulk bathed with serum, that part like the rest of the clot was everywhere in contact with the sides of the glass, and not a drop of serum was to be seen. At the same time there was no smell whatever about the cotton that covered the neck of the flask, showing that putrefaction had been avoided. Somehow or other the exclusion of living organisms, while it had not interfered with coagulation, had prevented the fibrine from acquiring a tendency to shrink. This fact, while entirely new, and opening up a wide field of inquiry, was seen to tally with phenomena met with in surgical practice, such as the absence of shrinking of the plug of clot near a ligature placed upon an artery. It was an illustration of how little we are often able to predict what may arise when even the most familiar objects are placed in new circumstances.

SCIENTIFIC SERIALS

THE *Journal of Mental Science*, July.—We have heard or read of a rather impressionable gentleman who, as he perused Dr. Buchan's "Domestic Medicine," fancied himself afflicted with every disorder therein described, not even excepting the pains of pregnancy. Bearing this in mind, we would recommend that none save those well assured of their own sanity should read the *Journal of Mental Science*. There is so much about morbid psychology, madness, and idiocy, that weak readers are in some real danger of being taken possession of by an uncomfortable suspicion that they may be a little touched themselves. The place of honour is given to an address on idiocy by Dr. J. C. Bucknill. This is a piece of special pleading (justified, perhaps, by its occasion) for the education of idiots. Now, as these miserable abortions must be kept in life because of the *indirect* evil effects of any system of extinguishing them, we certainly desire that they should be kept in asylums and made comfortable. But we cannot even grant that they are "more worthy of our efforts than those races of animals which men strive to bring to perfection." Except in so far as Science may be advanced by such work, it seems very much of a waste of time for such a man as Séguin to labour for four months to fix the eye of an idiot as the first step in the education of sight. We cannot go into ecstacy on hearing that idiots are actually taught to use knives and forks, when so many rational beings around us have neither knives nor forks to use, nor any use for them. By all means let the charitable support asylums for idiots; but at the same time it should not be forgotten that these poor creatures can never be educated into anything useful or lovely, and that a point is soon reached beyond which further education is misspent labour.—A valuable paper on "The Use of Digitalis in Maniacal Excitement" is contributed by Dr. W. J. Mickle. Next follows, under the title of "Consciousness and Unconscious Cerebration," a rather muddled attempt, on the part of W. G. Davies, B.D., to upset Dr. Carpenter's doctrine of "unconscious cerebration." From this article one might suppose that the views combated were peculiar to Dr. Carpenter and his so-called disciples Dr. Bastian and Miss Cobbe, whereas in truth the writer has against him not these only, but also the most distinguished of living psychologists. His writing is a good deal in the bad old style, the language serving at times, as it seems to us, to obscure rather than express thought. Dr. Carpenter is accused of imagining a nervous anatomy to suit his theory. But Mr. Davies does not himself seem to be up with the latest scientific surmises. For example, in laying the groundwork of one of his own arguments, he says: "The very same cells in the visual sense-centre cannot, at one and the same moment, see brown and yellow." He does not seem to be aware that it is highly probable that the cells that see one colour never do see another. There are over a dozen other papers, all of more or less, some of them of considerable interest.

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[*] *See* "Annales de Chimie et de Physique," 1837, p. 185.

THE *Monthly Microscopical Journal* for this month commences with an article by Mr. J. W. Stephenson on the optical appearances presented by the inner and outer layers of Coscinodiscus when examined in bisulphide of carbon and in air, in which the importance of considering the refractive index of the medium in which calcareous and silicious structures are examined, is fully discussed. This is followed by a paper on some new diatoms from the harbours of Peru and Bolivia, by Mr. F. Kitton, in which *Aulacodiscus formosus* and *Omphalopelta versicolor* are the most important.—Mr. F. Wenham, in a very temperate manner, rebuffs the unjustifiable statements of the American microscopists, who, not realising the high scientific position he holds in this country, accuse him of acting unfairly to Mr. Tolles, and insinuate that he has acted from mercenary motives. He ends by saying, "I trust that Colonel Woodward, having affirmed that 'the position taken by me is certainly true for objectives, as ordinarily constructed,' will allow that this additional lens embodies a deviation from the ordinary question, which was to the effect that there would be no loss of angle aperture of ordinary objectives by the immersion of the front surface in fluids."—Dr. Braithwaite continues his observations on the bog-mosses.—Dr. Royston-Piggott considers the high-power definition of minute organic particles, in which he divides his subject into five parts, including the nature of the least circle of confusion, the nature of mixed shadows, and the nature of perfect definition.—The preparation of the brain and spinal cord for microscopic examination, forms the subject of a paper by Mr. H. S. Atkinson, in which he explains in detail the methods employed by Professor Rutherford, and the means of staining sections adopted by himself.

Petermann's Geographische Mittheilungen, No. VI.—An account of Dr. Nachtigal's travels in Northern Africa, which appears in this number, we have already noticed in the advanced sheets. One of the longest and most valuable papers is by Dr. C. E. Meinicke on Dr. Bernstein's explorations in the Northern Moluccas, accompanied by a map. An important article is the second part of an account by Freiherr F. von Richthofen, of some of the results of his journey from Pekin southwestwards through China, embracing valuable details on the geology, topography, and natural history of the little known interior of that country. Another important article is on the Aurora Borealis, by M. E. Pechuel-Loesche, who for the purpose of ascertaining the real nature of the phenomenon, brings together the results of the observations of those who have carefully observed it in the Polar regions. This is to be followed by another paper in the same direction.—Dr. H. Wagner contributes an article on the Development of the German Railway System, accompanied by a well-constructed map.

A VERY interesting number of the *Bulletin Mensuel de la Société d'Acclimatation de Paris* has been published for May. One of the principal papers is a long article by the Abbé Desgodins, missionary at Yer-ka-lo, on the zoology of Thibet. The varied temperatures of its different levels are such that the country contains a great variety of animals, the fauna of both tropical and cold climates being found there. A description is given by M. Robert of his patent artificial incubators for hatching eggs, which seem to be more perfect in all their details than any of those appliances we have seen. As a proof of the usefulness of such a Society, the secretary calls attention to the increased price of certain animal and vegetable products of foreign countries, which, if the principle of acclimatisation were more fully developed, could be produced much cheaper in France. Experiments on sericulture have shown that silk of varied colour can be produced by feeding the silkworm on different leaves. Worms fed on vine leaves produce a silk of a magnificent red colour. Lettuce has been found to produce an emerald-green coloured silk. During April, 51 animals and 886 birds were received at the Gardens of the Society, while 51 animals, and 1,333 birds were distributed. Among interesting items of intelligence we may mention that the ostriches have begun to lay, and it is hoped that kangaroos may be so freely bred in France as to justify their being turned loose in suitable parts of the country. Three Trumpeter Swans were received from America.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 19.—"On a tendency observed in Sun-spots to change alternately from the one Solar Hemisphere to the other." By Warren De La Rue, D.C.L., F.R.S., Balfour Stewart, LL.D., F.R.S., and Benjamin Loewy, F.R.A.S.

1. Hitherto in our reductions we have summed up the spotted areas of the various groups occurring on the sun's surface on any day, and have regarded their sum as a representation of the spot-activity for that day. It has occurred to us to see what result we should obtain by taking instead for each day the excess of the spotted area in the one solar hemisphere above that in the other.

2. On adopting this method, it soon became evident that during periods of great disturbance there is a tendency in spots to change alternately from the north or positive to the south or negative hemisphere, and *vice versâ*, the period of such change being about 25 days. When, on the other hand, the solar disturbance is inconsiderable, the spots do not present any such systematic oscillation.

3. We have graphically represented on a diagram the results derived from this method during three of the most considerable periods of solar disturbance.

In this diagram the observed values of hemispherical excess are marked with an asterisk, and a curve is drawn so as to equalise their smaller irregularities. The northern hemisphere is reckoned positive, and the southern negative. The unit of area is, as before, the one millionth of the sun's visible hemisphere.

4. The first of these three periods extends from the beginning of August to the end of December, 1859. We derive from our diagram the following Table, exhibiting the maximum amounts of hemispherical excess, with their respective dates:—

	Hemispherical excess	
Date.	North.	South.
1859, July 31	+4180	
Aug. 18		(+ 40)
Aug. 27	+2580	
Sept. 11		−2920
Sept. 17	+ 920	
Oct. 3		−1420
Oct. 16	+1000	
Nov. 3		−2480
Nov. 15	+ 120	
Nov. 20		−1320
Dec. 7	+1050	
Dec. 22		−1400

From these we derive the following values of a period of oscillation by taking the differences in dates between the positive extremes:—

27 days, 21 days, 29 days, 30 days, 22 days—mean, 25·8 days;

while doing the same with the negative extremes, we obtain:—

24 days, 22 days, 31 days, 17 days, 32 days—mean, 25·2 days.

5. The second of the three periods extends from the end of June to the beginning of November 1860. Treating this in the same manner, we obtain:—

	Hemispherical excess.	
Date	North.	South.
1860, July 1	+4900	
July 22		− 600
July 30	+2040	
Aug. 9		−2400
Aug. 21	+ 400	
Sept. 5		−1400
Sept. 16	+ 400	
Oct. 1		−1180
Oct. 9	+ 800	
Oct. 19		−2560
Oct. 31	(− 380)	

From these we derive, by taking the differences in dates of the positive extremes,

29 days, 22 days, 26 days, 23 days, 22 days—mean, 24·4 days;

while doing the same with negative extremes, we obtain:—

18 days, 27 days, 26 days, 18 days—mean, 22·25 days.

6 The third of these three periods extends from the beginning of May to the end of August 1862. Treating this in the same manner, we obtain:—

	Hemispherical excess	
Date	North	South
1862, May 9	+ 600	
May 22		−1160
June 3	+2960	
June 15		−2600
June 29	+1880	
July 16		−800
July 26	+2400	
Aug. 14		− 200
Aug. 23	+ 460	

Taking, as before, the distances between the positive extremes, we obtain:—

25 days, 26 days, 27 days, 28 days—mean, 26·5 days;

while from the negative extremes we obtain:—

24 days, 31 days, 29 days—mean, 28·0 days.

From the whole three periods we obtain, as the most probable mean value, 25·2 days.

7. We do not profess to have discovered the cause of these oscillations, but we would nevertheless suggest that the observational facts here brought to light may perhaps be connected with two other observational facts, the one of which was first brought to light by Carrington, and the other by ourselves.

The first of these is the fact that, generally speaking, spots in the north hemisphere have much about the same latitude as those occurring at the same or nearly the same period in the south, both sets widening or contracting together. We may perhaps, therefore, suppose, by applying this law, that the latitude of the spots which cause the positive extremes in the above series is not greatly different from that of those which cause the corresponding negative extremes.

The second observational law is that which tells us that spots about the same period have a tendency to attain their maximum at or near the same ecliptical longitude. Now, if we suppose that in the foregoing three series the greatest positive extremes were caused by the positive spots attaining their greatest size, and the greatest negative extremes by the negative spots, attaining their greatest size, it would follow that the two sets, positive and negative, must have taken their rise at places on the sun's surface 180° of longitude different from each other inasmuch as the one set about twelve or thirteen days before or after passed (let us say) the same ecliptical longitude as the other.

But if the positive set have the same latitude as the negative, and if the one is 180° of solar longitude different from the other, it would mean that *the two outbreaks are at opposite ends of the same solar diameter*.

This conclusion is an interesting one, but, of course, it requires to be verified by further observation before it be finally received. Meanwhile, we are engaged in mapping out systematically the positions of the various outbreaks of the sun's surface, and we shall soon, therefore, be able to find whether or not there be any truth in this conjecture.

Geologists' Association, July 4.—Mr. Henry Woodward, F.R.S., president, in the chair.—1. "A sketch of the Geology of Northamptonshire," by Samuel Sharp, F.S.A. A general section of the county of Northampton shows the lias as a basal formation with the inferior oolite beds of the "Northampton sands" above. Fossils are abundant, and some species are not found in other localities. The upper division consists of a nearly white siliceous sand with bands of clay and a plant bed, the whole of these deposits being evidently of estuarine and littoral origin. Above these, but unconformably, lies the bed classed as Great Oolite, and which consists of, firstly, a series of clay beds with a ferruginous base and containing a plant bed; then, secondly, a limestone series abounding with fossils and affording an ornamental stone called "Alwalton marble." The bed of clay reposing on these great oolite strata may be considered the equivalent of the "Bradford clay," and still higher in a general section will be found the Forest marble, the Cornbrash, and, highest of the secondaries, the Oxford clay. The high lands of the county are frequently capped by boulder clay and glacial gravels containing fragments from nearly the whole series of the primary and secondary rocks. A peaty fluviatile bed above the gravels contains at its base numerous remains of mammalia. The lias extends throughout the county though appearing only in the valleys, the iron sands occupy the middle and the Lincolnshire limestone the northern portion of the county, while the other formations are patchy in extension. A high table-land about Naseby gives rise to the Avon, the Welland, and the Nene, which occupy the principal valleys of the county. In past times efforts were made at considerable cost to find coal, and recently the question of whether coal can be obtained in the county has been discussed, but judging from what we know of the rocks of the nearest coal-field of Warwickshire, and of the intervening district, as much as 4,500 ft. of strata may lie above coal-seams of sufficient thickness to be worked. Moreover, Prof. Hull, F.R.S., concludes that "Carboniferous" coal will not be found at any depth in Northamptonshire.—2. "On some new Crag Fossil," by Alfred Bell. The author's observations since his former paper on the crags was read, confirm the views he then expressed as to the divisibility of the English crags into four divisions founded on palæontological evidence. He had determined 145 species (some new, some new to the crag, and some new to particular divisions) in addition to those given in his published lists.—3. "An account of the Eruption of Mount Vesuvius of April 1872," by J. M. Black. In this paper the brief but violent and destructive eruption of last year was described by the author, who has carefully noted the various phenomena that occurred during its continuance. An ascent of the volcano was made by Mr. Black, a few days after the eruption, and the form and condition of the crater observed. The author had succeeded in photographing various parts of the mountain after the eruption, and the views so taken were exhibited.

PHILADELPHIA

Academy of Natural Sciences, April 1.—Dr. Ruschenberger, president, in the chair. The following paper was presented for publication:—"On the Affinities of the Sirenians," by Theo. Gill. Prof. Leidy remarked that the rat presented this evening by Mr. L. Fussel was a specimen of the Black Rat, or *Mus rat-*

tus, which had been caught on board a ship in the vicinity of the city. This rat is exceedingly rare, but is said to have once been common enough, and is also said to have been nearly exterminated by the common brown or Norway Rat.

April 8.—Dr. LeConté announced the death, at Davidsburg, York Co., Pa., on March 10, of Freidrich Ernest Melsheimer, M.D., a correspondent of the Academy, aged nearly ninety-one years. He inherited great taste for entomology from his father, E. F. Melsheimer, a clergyman, who cultivated natural science with much success, and not only was a highly esteemed correspondent of Knoch and other European entomologists of the end of the past and beginning of the present century, but an active collaborator with Say, the founder of descriptive entomology in the United States. Entomology also owes to Dr. Melsheimer the catalogue of the described Coleoptera of the United States, which was published by the Smithsonian Institution in 1853. It was the first work of bibliographical importance in the modern history of that branch of science, and gave a powerful impetus to its development in the United States, and has greatly diminished the labour of those who have continued the study of that department.

April 15.—"Observations on a Change of Structure of a Larva of *Dryocampa imperialis*," by Thos. G. Gentry.—"Remarks on Extinct Mammals from California. Prof. Leidy directed attention to some fossils, which he had borrowed, through Prof. E. O. Hovey, from the cabinet of Wabash College, Crawfordsville, Indiana. The most interesting specimens consist of an upper molar and a complete lower molar series of a lama as large as the existing camel. Remains of a still larger species from California have been previously indicated under the name of *Auchenia californica*. The present specimens were referred to a species with the name *Auchenia hesterna*. Prof. Owen has described some remains of an equally large lama from Mexico, which he refers to an allied genus with the name *Palauchenia magna*, and which perhaps may be the same as the *Auchenia hesterna*. An inspection of Prof. Owen's figures of a series of molar teeth leads to the suspicion that he has inadvertently mistaken the upper series for the lower ones, and has thus been led to refer them to a genus different from *Auchenia*.

April 22.—"Influence of Nutrition upon Sex among the Lepidoptera," by Thos. G. Gentry.—"Fungus Parasite on a Mouse." Prof. Leidy exhibited a mouse with several whitish masses adherent to the ears, side of the face, and nose. The mouse had been caught in the children's department of Blockley Hospital. The white matter examined beneath the microscope proved to be composed of sporular bodies, single, double, or in short chains of a dozen or more. They measure about the [illegible] of a line in a diameter. The fungus is a Torula or Oidium, and resembles that found in Aptha. Perhaps the disease in the mouse is the result of feeding upon articles imbued with adherent portions of apthous matter from the mouths of children.

BERLIN

German Chemical Society, June 24.—C. Rammelsberg, vice-president, in the chair.—F. Römer has investigated the following derivatives of normal propylic alcohol:—The mercaptan and its mercury-compound, propyl-xanthogenic acid and its sodium salt, and the monamine. By heating cyanate of potash with propyl-sulphate of potash no cyanurate of propyl was formed, but a well-crystallised biuret in which three atoms of hydrogen are replaced by three molecules of propyl.—R. Otto sent a well-crystallised specimen of phosphate of ammonium and magnesium from the cesspool of an old house in Brunswick, analogous to the crystals of "Struvite" found in Hamburg in 1842.—C. Scheibler showed a specimen of glass ground by a new method, which has come to us from America, and is now practised in the glassworks of M. Hasenclever at Hollberg, near Aix-la-Chapelle. By means of Giffard's injector a current of fine hard sand is thrown with great force on the glass, which is thus ground; but any pattern cut in paper and pasted on the glass remains unaltered. Even hard minerals, such as corundum, can be ground by this process.—C. Bottinger has studied the action of baryta on pyruvic acid. According to Finck two acids are thus produced, one crystalline, which he called uritinic, and one syrupy body, to which he gave the name of uritonic acid. Mr. Bottinger's researches throw doubts on the existence of the latter body, which seems to be a mixture of uritinic, acetic, and oxalic acids.—C. Rammelsberg communicated new researches on the composition of vesuvians of different origin.—W. H. Pike, of London, has treated sulfo-urea with chloride of benzoyle, obtaining well crystallised benzoyle-sulfo-urea $(C_7H_5ONH)CS(NH)_2$ of the melting-point 170°.

PARIS

Academy of Sciences, July 7.—M. de Quatrefages, president, in the chair. The following papers were read:—New clinical researches on the localisation, in the anterior lobes of the brain, of the action by which the brain contributes to the psycho-physiological faculty of speech, by M. Bouillaud. At the conclusion of this somewhat long paper, M. E. Chevreul made some remarks on Dr. Bouilland's conclusions.—On the exponential function, by H. Hermite.—On the heat of combination referred to the solid state, a new expression for thermo-chemical reactions, by M. Berthelot.—The election of Dr. Carpenter, Mr. Steenstrup, and Mr. Dana, as recorded in our notes, then took place.—On a system of optical telegraphy, invented during the siege of Paris, by a commission appointed by the Governor, by M. Laussedat.—On the nutritive and milk-producing properties of *Galega officinalis*, by M. Gillet-Damitte.—On the constitution of the sun and the theory of the spots, by M. E. Vicaire. The author vigorously supported the scoriæ theory of spots, which he regards as formed by the fall of heated products of combustion into a boiling liquid; he considers that the prominences are formed at the same time and by the same agency.—Solar cyclones compared to those of our own atmosphere, by M. H. Tarry.—On a new isomer of valeric acid, by MM. Friedel and Silva.—On the transformation of succinic into malic acid, by M. E. Bourgoin. The author has succeeded in effecting this by heating fine and dry argentic malate, mixed with fine sand, to 180°.—On the mode of decomposition of explosive bodies as compared with the phenomena of supersaturation, by MM. Champion and Pellet.—On the action of benzyl chloride on napthalamine, by MM. Ch. Froté and D. Tommasi.—Experimental researches on the action of nitrous oxide, by MM. F. Jolyet and T. Blanche. The authors believe that this gas is not a true anæsthetic, but acts by producing asphyxia.—Researches on the floral organogenesis of the hazels, by M. H. Baillon.—Discovery of the makis and the horse in the fossil state in the phosphorites of Lot, by M. F. Delfortrie.—On the crystalline forms of Scotch Lanarkite, by M. A. Schrauf.—Details of the earthquake of the 29th of June, by M. W. De Fonvielle.

BOOKS RECEIVED

ENGLISH.—Geological Evidences of the Antiquity of Man. 4th ed.: Sir Charles Lyell (J. Murray).—Human Longevity, its Facts and Fictions: Wm. J. Thoms (J. Murray).—The Human Mind; a System of Mental Philosophy: Jas. G. Murphy (W. Mullan, Belfast).—Mrs Taylor's A B C of Chemistry. Edited by W. M. Williams (Simpkin, Marshall & Co.).—Six Lectures on Light, delivered in America: John Tyndall, F.R.S. (Longman & Co.)—Journal of Anatomy and Physiology, 2nd Series, No. 12 (Macmillan & Co.).—Relations of the Air to the Clothes we wear, &c.: Dr. Max von Pettenkofer: translated by Dr. Hess (Trübner & Co.).—The Royal Readers, No. 6 (T. Nelson & Sons).—Essay on the Mathematical Principles of Physics: Rev. Jas. Challis, M.A. (Deighton, Bell & Co., Cambridge).

PAMPHLETS RECEIVED

FOREIGN.—Sitzungsberichte der Konigl Böhmischen Gesel. in Prag, Jan. to June and July to Dec., 1871, Jan. to June 1872.—Eleven copies of Proceedings of Ditto: K. W. Zenger, A. von Waltenhofen, O. Feistmantel, J. Schöbl, J. Dienger, J. M. Solin, E. Weyr, W. Matzka, K. Domalip, and C. Kupper. Die Bewegungen der Thiere und ihr psychischer Horizont. von Dr. Karl Möbius.

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THURSDAY, JULY 24, 1873

THE ENDOWMENT OF RESEARCH

III.

It is probable that though the main proposition here advocated, that original workers in the Sciences deserve, on public grounds, a recognised position and pecuniary support, will not meet with much opposition from any quarter, the means by which this desirable end is chiefly proposed to be attained will not be acquiesced in with equal readiness. Englishmen have been so long accustomed to regard their Universities as merely high schools of liberal education, and the independent growth of modern Science in this country has been so rapid and vigorous, that to many worthy persons it will seem nothing better than a Utopian dream to attempt to re-establish the genuine pursuit of scientific knowledge as an end in itself at our ancient seats of learning. Those, however, who know something about the system of a German University, and are acquainted with the former history of Oxford and Cambridge, will not consider the attempt to be of such a hopeless character. The present time also affords an admirable opportunity of urging upon public attention a fundamental reform in the direction above indicated. The Universities have of late years been losing many of the peculiarities which they once so warmly cherished, and at the same time their revenues have been increasing to an enormous extent. The same Government which passed a Bill to pronounce them national and not ecclesiastical establishments, has also issued a Royal Commission to inquire into the extent and distribution of their endowments. Now that the nation has established its claim to remodel the Universities solely with a view to the public interest, and is taking stock, as it were, of the property which has fallen under its disposal, the very occasion has come when scientific men should formulate their demands on behalf of those public interests which the practical politician is likely to neglect. It must, moreover, be borne in mind that the impulse in this direction must come from without, for although it will not be difficult to prove that no less benefit would accrue to the Universities themselves than to the cause of Science from the scheme herein advocated, yet the most advanced academical reformers do not seem to have got beyond the notion of extending and perfecting the professorial functions.

We propose then to show at some length that the Endowment of Research should naturally take a leading place in the reconstruction of the University system which appears to be close at hand, and to indicate in what manner such endowment may most readily be carried into effect. For this purpose it will not be necessary to reveal the many minor abuses which the reforms of twenty years ago failed to remove, but it will be necessary to adopt the more difficult task of sketching out the true conception of what a University should be, and of considering the comparative claims to endowment of teaching and of study.

Without any attempt to prejudge the matter, or to awake the dormant controversy as to the original meaning of the word, it may be safely laid down that a University is an institution composed of the most competent teachers and the most promising students, on which the State, in consideration of its diligently promoting the higher education, confers a lofty position and important privileges. That such an institution should enjoy large endowments is evidently not of the essence of its nature, for the Universities of old were uniformly most famous when they were least rich: it is, however, absolutely necessary for the healthy activity of its functions that it should not be so encumbered with wealth as to be disposed to lavish sinecures upon its favourite members. It is evident, also, that it will forfeit its trust as the home of Culture and of Science, and will degenerate into a lyceum for the adult sons of the well-to-do classes, unless it continually maintains itself on a level with the ever-advancing boundaries of human knowledge, and that just so far as it lags behind it will exercise a mischievous influence on the simple public, who continue to rely upon its treacherous authority. Further, it is of great importance that the original institution, on which alone the rank was bestowed, and which alone deserves the high privileges, should not be absorbed by the growth of a number of parasitic institutions, whose interests and aims may be not identical with or even analogous to its own. But above all other symptoms of decay that a University can show, is to be placed its rejection of the highest branches of knowledge which the progressive activity of human thought is ever comprehending within the domain of Science. To this danger the most ancient and the most wealthy Universities are naturally the most exposed. Their antiquity leads them to regard the erudition which they have inherited through many centuries as synonymous with real knowledge, and their wealth is used (where it is not misused) to afford encouragement only to those kinds of learning which their traditions have sanctified. In brief, a false University would be an institution which is content merely to satisfy the demand for teaching which custom approves, and which neglects as a hindrance to its tuitional duties the higher knowledge which it was originally founded to promote.

To recall such a University to the true conception of its duties no mere mechanical changes with reference to its internal organisation will be sufficient. It has lost the spirit of disinterested study which first gave it life, and the atmosphere of intellectual activity under which alone it can flourish. It requires that new vigour should be poured into it, and a new order of workers established within its limits. It requires to be relieved of the burden of part of its wealth, in order that it may receive back again greater advantages than it can give. By endowing research in all those departments of knowledge to which the scientific method has been already extended, and by reserving the power of similar endowment for those other departments of knowledge which will, no doubt, before long be similarly reduced to order and law, Oxford and Cambridge may yet regain the proud position which was once theirs, as "bodies of learned men devoting their lives to the cultivation of Science, and the direction of academical teaching."

To point out more particularly the source from which the endowments of research should be drawn, it will be necessary to revive the original distinction between the Universities and the Colleges of which they may be said to be now composed. To raise the University proper at the expense of the individual Colleges, has long been a favourite project with academical reformers, yet no one yet appears to propose any more radical scheme than an augmentation in the number of University Professors, and a diminution in the influence of College tutors. Against any such scheme, however carefully elaborated, there arise the old objections that an improvement in the mechanism of teaching is not the main reform of which the Universities stand in need, and that the endowment of more teachers will not remedy the crying evil which has so lamentably hindered the advance of purely scientific investigation in this country. The circumstance that the Universities are comparatively poor, while many of the Colleges are very rich, and an awakening conviction that the Colleges exist for the Universities, and not the Universities for the Colleges, would seem to have suggested the above proposal: whereas the smallest historical knowledge of the objects with which the Colleges were originally founded, would reveal the curious circumstance that the first benefactors had a truer conception of the manner in which knowledge ought to be endowed, than have the modern recipients of their benefits. Nothing can be more certain, though nothing is more frequently denied by those whose duty it is to be better informed, than that the majority of the great Colleges were not founded to be boarding schools for teachers and students, subordinate to the University curriculum, but to be homes at the central seats of learning, where life-long students might be supported while acquiring all the knowledge of the age, and augmenting the store of learning which they had there inherited. According to the old Oxford tradition, she could boast in the fifteenth century before there was ever a wealthy College that she had thousands of students living in hundreds of private halls. Many of the early Colleges did not include at all in their arrangements those whom we should now call Undergraduates, some of those which did do so allowed for a teaching staff independent of the body of Fellows, and it is within modern memory that many Colleges have had more Fellows than Undergraduates on their books. All these facts, and there are many similar ones, go to prove decisively that, in the language of Mr. Mark Pattison, "the Colleges were in their origin endowments not for the elements of a general liberal education, but for the prolonged study of special and professional faculties by men of riper age: and that so far from it being the intention of a Fellowship to support its holder as a teacher, it was rather its purpose to relieve him from the drudgery of teaching for a maintenance, and to set him free to give his whole time to the studies and exercises of his faculty." The wish of the Founders, that is to say, when harmonised with the wants of the present age, and interpreted into the language of modern science, was to afford the means of living and the instruments of work to those who pledge their lives to the unremunerative task of scientific investigation, and original research.

Surely then, if the influential and wealthy members of our Universities have at heart the real interest of their Institutions, or retain any veneration for the express intentions of their benefactors, they should not be the last to join in the patriotic object of raising the scientific reputation of this country, and increasing in manifold unseen ways the elements of our national greatness. C.

ALEXANDER VON HUMBOLDT

Life of Alexander von Humboldt, compiled by F. Löwenberg, Robert Ave-Lallemant, and Alfred Dove. Edited by Professor Karl Bruhns. Translated by J. and C. Lassell. 2 vols. (London: Longmans, 1873.)

WE cordially welcome this admirable translation of the only biography of A. v. Humboldt that has yet appeared possessing any authentic or scientific value. Humboldt's own definitely expressed aversion to biographical notices, whether in regard to himself or his friends, the fact of his having outlived nearly all his blood-relations and the greater number of the contemporaries of his earlier working years, together with other causes, combined, for a time, to retard the appearance of a trustworthy life of this remarkable man.

The want of such a work was, however, strongly felt, and at the Congress of Astronomers convened at Vienna on Sept. 14, 1869, in honour of the centenary of A. v. Humboldt's birth, Dr. Karl Bruhns, Director of the Observatory at Leipzig, laid before the meeting the prospectus of a Scientific Biography of their great countryman, for which he demanded their active co-operation. The result of this appeal and of his own editorial labours, was the appearance last year, in Germany, of the work of which the present excellent translation gives us two volumes. The third volume of the original, which consists of critical *résumés* by various writers of the state of different branches of the physical and natural sciences, with notices of Humboldt's contributions to each, has been omitted by the translators, on the ground that the facts were treated of with sufficient minuteness in the general biography. On less good grounds, as it appears to us, they have also omitted from the last section of the second volume, the comprehensive catalogue of his published writings, of which upwards of 600 are enumerated in this list.

Humboldt's life, like the work devoted to its exposition, resolves itself into two distinct parts or periods. The first of these is characterised by intense and incessant activity in the acquisition of knowledge, the second by the quiet mature elaboration of the results of earlier study and observation ending in a thirty years' term of comparative stagnation under the depressing influences of honorary court servitude.

Alexander v. Humboldt was born at Berlin, in 1769, and together with his elder brother Wilhelm, was prepared under excellent private tutors for his university career at Frankfort, A. O., where he matriculated in 1787. He had already then shown that craving for the accumulation of facts which he retained to his latest years, and from his boyhood had been distinguished for his love of observing and collecting natural history objects, and his inaptitude for acquiring the exact classical scholarship for which his brother evinced such marked ability. Botany was Alexander's first love, and the earliest of his voluminous literary productions was a treatise in French which appeared anonymously at Berlin, in 1789, in the *Gazette Literaire*, entitled, "Sur le Bohon-Upas, par un jeune Gentilhomme de Berlin." This composition was, however, rapidly followed by papers on the flora and geology of the Rhine lands, and other districts which he visited in the course of the few short intervals of cessation from study which mark his university career, and by numerous essays on mathematical, physical, medical, physiological, and even *classical* subjects; for by dint of hard work he had, during his attendance on Heyne's Greek lectures at Göttingen, so thoroughly mastered his earlier deficiencies that he won from that learned professor the distinction of being commended as "a better philologer than any who had left the class for many years." The University of Gottingen to which the brothers had migrated in 1789, and which had already begun to attract students from all parts, as the best school of pure and practical science, afforded the advantages that Frankfort had failed to give them; and here, under Lichtenberg, Gmelin, Osiander and Blumenbach, Alexander laid the solid foundations of those varied acquisitions in the departments of physical and natural science, which justly entitle him to rank as the greatest pioneer in the cause of modern research. Others may have very far surpassed him in one or more domains of inquiry, but no one man in his time has done more than A. v. Humboldt in accumulating materials, testing evidence, repeating experiments and carrying on observations in almost every section of knowledge by which the labours of subsequent inquirers have been lightened. To his latest years, Humboldt did justice to the benefit which he had derived from Göttingen, which he had entered with "the *unusual* advantages," as we are told by his former tutor, the mathematician, Fischer, "of having received an excellent education, and of possessing a proficiency in mathematics which might have secured him distinction had he been able to devote his attention exclusively, or even partially, to that science." Political economy had, however, already become the principal object of his studies, in consequence of his having made choice of the public bureaucratic service of the State as his future career. In 1790 his experiences of foreign travel were begun during a visit to England, made in company with George Forster, the friend whose adventurous voyages and various books of travel had given Humboldt from his earliest boyhood the keenest desire to visit tropical lands, and see with his own eyes the exotic floras and faunas which he described in such glowing colours. The journal which records the experiences of this tour gives evidence of the astonishing range of information possessed at this time by Humboldt, who, true to his destined vocation, set himself steadily to work to observe everything bearing upon the politico-economical aspects of English life, although his scientific tastes are perpetually cropping out in remarks upon the geological features of the country. To this first experience of English life and to the influence exerted on his future pursuits by intercourse with George Forster and his friends, Humboldt long looked back with grateful pleasure. Soon after his return to Germany he went to Hamburg for the sake of attending lectures on currency, book-keeping, and other practical branches of commercial knowledge at the Academy of Commerce, which, under the management of its chief professors, the jurists Busch and Ebeling, was attracting the attendance of young men preparing for a political career.

From Hamburg A. von Humboldt passed to the Freiberg School of Mines, where, under Werner, he prepared himself for the special duties of the post of Assessor and Superintendent of Mines to which he had for some time aspired, and which for a time after its attainment seemed to him the realisation of all his wishes. No *employé* had ever been more zealous, and all his reports were expansive geognostic treatises on the districts he was called upon to survey. The charm of novelty soon, however, wore off, and then the complete stagnation, the systematised red-tapeism, and the absolute dearth of intellectual or rational interests belonging to Prussian Public Service in those times, proved as unbearable to Alexander as they had already become to his elder brother, and both ceased their official connection with the State at the first moment they could do so. Society in Berlin was equally distasteful to them on account of the prejudice and etiquette by which it was regulated, and after a prolonged and happy sojourn at Jena and Weimar, the then active centres of the true intellectual, æsthetical, and literary life of Germany, Alexander proceeded, on the death of his mother in 1796, to carry out his long-cherished dream of visiting far distant tropical regions. To prepare himself thoroughly for this purpose had been for years the object of his studies, and few men were ever better fitted than himself for the end he had in view. To his other qualifications for becoming an efficient scientific traveller, he added the possession of an almost unparalleled range of knowledge, including an intimate acquaintance with the character, history, and resources of his own country, unbounded love of nature, unflinching perseverance, nearly inexhaustible capacity for work, wide sympathies with his fellow-men, a ready gift of pleasing and being pleased, and an ardent, almost ideal enthusiasm, which found expression in his own favourite motto, "Der Mensch muss das Grosse und Gute wollen" (Man must strive after the Great and the Good).

After oft repeated disappointments and many shattered plans, A. v. Humboldt, in spite of the numerous obstacles arising from the disturbed political condition of Europe at the time, achieved his long-cherished project of visiting the New World, and in the summer of 1799 he landed in South America. In the following year he and his companion and friend, Bonpland, plunged into the steaming forests of the Orinoco, and bidding farewell to civilisation, threw themselves into the work before them. An enormous mass of specimens collected from every kingdom of nature preceded A. v. H.'s return to Europe in 1804, and gave the scientific world at home a faint foreshadowing of the gigantic dimensions of the labours accomplished by that indefatigable explorer. Paris was at that time the only spot where a work such as he meditated could be produced, and accordingly thither he repaired, and after securing the co-operation of Cuvier, Latreille, and many of the other leaders of science, proceeded to elaborate his materials. The result of these combined labours was the appearance, in 1807, of the magnificent work known as "Voyage aux Régions equinoxiales du Nouveau Continent fait dans les années 1799 à 1804, par A. de Humboldt et A. Boupland." The cost of bringing out this colossal *résumé* of his American observations involved Humboldt in pecuniary embarrassments, from which he can scarcely be said ever to have freed himself, and which had moreover the disastrous results of forcing him to accept help at a subsequent period from the King of Prussia; and thus incur an obligation which he found could only be redeemed by devoting himself to the perpetual restraints of a court-life. The times were inauspicious to great literary or scientific undertakings, and hence we cannot wonder that the "Voyages aux Rég. Equinox." should have proved pecuniarily a failure. At that period of political inquietude and financial depression in every part of the Continent, 290*l.* was a very large sum to pay for any work, although, perhaps, not in this case commensurate with the outlay, when we bear in mind that the printing and paper alone had cost 840,000 francs, and that it contained more than 1,400 beautifully coloured illustrations, and consisted of twenty folio and ten quarto volumes, which were, moreover, divided into five distinct parts, complete in themselves, and to be purchased separately. Humboldt had started on his travels with property realising about 500*l.* a year, but the cost of his expedition and of publishing, added to the war requisitions by which the value of his private property had been materially injured, left him for a time on the brink of absolute poverty. These temporary anxieties had, however, little effect on his mental energies; and after the completion of his American voyage, he continued for twenty years to reside at Paris, where his life was passed in one incessant whirl of intellectual labour, scientific discussions and social intercourse. Thus at one time he would spend months together working with Guy Lussac in the laboratory of the Ecole Polytechnique, at another keeping watch day and night at the Observatory, while he was always preparing fresh papers to read before the Institute and other scientific associations, and carrying one or more works contemporaneously through the press. Besides these labours he had early entered upon the study of the Oriental languages with the view of undertaking a scientific expedition into Asia for the purpose of collecting materials for a comparison between the eastern and the western continents. This scheme after many abortive attempts was finally carried out in 1829, when by the munificent aid of the Prussian King and the truly imperial liberality of the Emperor Nicholas, Humboldt found himself able to penetrate at the head of a carefully equipped scientific staff into the Steppes and the remotest parts of Asiatic Russia. The cost of his journey from Berlin to St. Petersburg and back was defrayed by the Prussian Government, whilst a sum of 20,000 roubles was placed at his disposal for his personal expenses by the Emperor, on his arrival in Russia. The results of this great expedition are of very inferior value to those yielded by the American voyages of earlier years.

This comparative failure may be in part referred to the short time—only nine months—devoted to the purpose, during which the veteran traveller passed over nearly 12,000 miles of the Russian territory. The journey was moreover a princely procession rather than a scientific expedition. Wherever he went crowds of local dignitaries, soldiers and police officers surrounded him. Governors of provinces, commandants of fortresses, superintendents of mines welcomed him with speeches and reports whenever he appeared within the limits of their jurisdiction. Generals supplied him with minutes of the strength of the various brigades under their command, while officers and men in dress uniforms saluted him in military fashion as he passed their posts. At Miask these military marks of respect culminated in the presentation, by the directors of the mines, of a grand cavalry sabre, in honour of his sixtieth birthday. The learned bodies were equally on the alert to show him respect. At Kasan, after incessant feasting and speechifying, the Professors escorted him to his lodgings at 1 A.M. in gala costume, and reappeared in the same attire at 4.30 A.M. to speed his departure to the next station. After enduring a host of similarly oppressive social distinctions, which included at Jekatharinenburg the obligation of leading off a ball in a stately quadrille, and on the Steppes at Orenburg the necessity of presiding over a Kirghis festival at which the men ran races and the Tartar Sultanas warbled sweet songs in his praise, Humboldt had to encounter at Moscow one of the most absurd ordeals to which the fame of his greatness exposed him. On his arrival he was invited to attend a special meeting of the Physical Society, and duly made his appearance at the University, holding in his hand the paper he had prepared to read to the learned members "On the deviation of the Magnet in the Ural." The court, passages, stairs, and halls were crowded with great people, gorgeous with stars and orders, amongst whom stood conspicuous the Professors, wearing long swords girded to their sides, and three-cornered hats tucked under their arms. Speeches of welcome in German, French, and Latin from the Governor-General, the chief clergy, and the deans of the various faculties had to be heard and replied to, and instead of engaging in scientific discussion on magnetic aberration, Humboldt had to listen to a Russian poem in which he was hailed as Prometheus, and to examine a plait of Peter the Great's hair, which was solemnly presented for inspection by the Rector of the University. The "Asie Centrale" and a few very fragmentary works were the immediate results of this most oppressively-honoured expedition, from which, satiated with ceremonials and respect, Humboldt had, in the winter of the same year, 1829, returned to Berlin, which thenceforth to the end of his long life in 1859 became his home.

To fully understand the sacrifices to expediency and to the obligations of gratitude made by Humboldt in accepting the position of what may best be termed an honorary *attaché* to his own Court and Sovereign, one requires to read with attention the pictures drawn in these volumes of society in the Prussian capital during the earlier half of this century. But it would scarcely, perhaps, be possible in the present changed position of Prussia to realise the deadness and stagnation that then hovered over every phase of social life. Humboldt, who from the year 1809, when he accompanied the Prince of Prussia to Paris in the capacity of friendly and official adviser, had repeatedly been entrusted with diplomatic and other honourable missions by the Sovereign, entertained a warm regard for the different members of the Royal family, while his relations to the late King Frederick William IV. were those of a long-tried, affectionate friendship. These feelings undoubtedly softened the hardships of the courtly bondage in which he spent his last thirty years, but though they may have gilded the bitter pill, they scarcely made it palatable; and Humboldt's voluminous correspondence at Berlin bears ample testimony to the struggle which was going on within himself to keep in check his contempt for Courts, his natural proclivity to sarcasm, and his impatience of routine constraints. With the view of trying to leaven the dead mass around him, and to awaken some interests apart from everyday life, he gave popular lectures to the upper classes, which ultimately resolved themselves into that very attractive—if slightly prolix—*résumé* of his knowledge, observations, and speculations, which we know under the title of "The Cosmos." And while he laboured assiduously to exercise his influence for the endowment of scientific institutions of all kinds, and the encouragement of learning and learned men, not only in Germany, but in every country where his reputation made his recommendations authoritative, he set his scientific brethren a striking example of patient, persevering industry in trying to keep pace with the rapid progress of inquiry, and of humble readiness in renouncing old opinions whenever he found that they had been superseded by more correct views.

To the English reader interested in tracing the progress of scientific and social development in Germany and other parts of the Continent during the close of the last and the first half of the present century, the "Life of A. von Humboldt, by Bruhns and Lassell," cannot fail to prove at once instructive and suggestive.

STIRLING'S "PHILOSOPHY OF LAW"

Lectures on the Philosophy of Law. Together with Whewell and Hegel, and Hegel and Mr. W. R. Smith, a Vindication in a Physico-Mathematical regard. By James Hutchinson Stirling, F.R.C.S. and LL.D. Edin. (London: Longmans, 1873.)

THIS volume contains certain lectures on the Philosophy of Law, delivered to the Juridical Society of Edinburgh in November 1871, together with a discussion of Hegel's opinions concerning gravitation and the differential calculus. Of the lectures we may say, that if the members of the Juridical Society understood them, they must be much more clever than we profess to be. The first lecture is an introduction to philosophy in general, that is, the philosophy of Hegel. It expounds the doctrine of the *notion*, and discloses in the briefest possible space the "secret of Hegel." Mr. Stirling has already written a work of two substantial octavo volumes, entitled "The Secret of Hegel." A friend of the author being found reading it, and being asked what he thought of the "Secret," answered, "Why, I think the author has kept it." If then the secret cannot be disclosed in two volumes, how did Mr. Stirling hope to make it plain in a lecture occupying only fifteen printed pages? In reading this lecture we did not enjoy for a single moment the feeling of solid ground. We had an impression that we understood what logic was until we met with the following passage:—

> "Hegel's system, as is now pretty well known, is contained in three great spheres—the Science of Logic, the Philosohpy of Nature, and the Philosophy of Spirit. Here we see at once that what we have before us is the Notion. Logic is the universal; Nature is the particular; and Spirit is the singular. Logic, having developed into full *Idea*, passes into the particular as the particular, into externalisation as externalisation, in Nature; and Nature, rising and collecting itself, through sphere after sphere, from externality itself in the form of space, up to *natural* internality in the form of organic life, passes into the Soul, which is the first form of Spirit. The instrument of the evolution all along, we are to understand, is the *Notion*, in its three *Moments*" (p. 15).

So long as Hegel and his satellite Stirling kept to the *notion* and its three moments in the abstract, they are impregnable and unapproachable, like those fishes which are said to make the water muddy all around when an enemy is near. It was when Hegel ventured out of his own mists that he showed his extreme fallibility. Having applied his "notion" to the theory of gravitation, he discovered that Newton was wrong in asserting the curve of motion of a gravitating particle to be any conic section.

> "Hegel's idea certainly is that the ellipse is a necessary outcome of *the notion* on this the stage of free motion according to the relations of time and space as moments. If planets do move in circles, or even if planets might move in circles, Hegel would here have to confess a failure. It would be his metaphysic that in that event would suffer, however, rather than his knowledge of physics. In the meantime, the fact is that the curve of movement still remains an ellipse, and Hegel so far is not in error" (p. 99).

Now, inasmuch as the circle is only the extreme case of an ellipse possessing no eccentricity, it is just as likely that a planet would move in a circle as in any one definite ellipse; but astronomers could never discriminate with certainty between a circle and an ellipse of very slight eccentricity; and so far Hegel escapes absolute conflict with fact. Unfortunately, however, it is known that certain comets move in hyperbolic paths (see Chambers' "Handbook of Descriptive and Practical Astronomy," p. 203, 1861), and as the ellipse is the *necessary outcome* of Hegel's notion, we think he must suffer both in his metaphysics and his physics.

In Mr. Stirling's controversy with Mr. W. R. Smith concerning Hegel's notion of the differential calculus, we also think that Hegel suffers. The critical statement of the necessary outcome of Hegel's philosophy is as follows (p. 113):—

> "The limit of a qualitative relation is that in which it both is and is not, or, more accurately, that in which the quantum has disappeared, and there remains the relation only as qualitative relation of quantity."

Now the very essence of the differential calculus consists in the fact that quantities, although indefinitely decreasing, or vanishing, as the expression is, preserve all their quantitative relations. Mr. Stirling says (p. 114):—

> "What is called infinitely *little* is only qualitative, and is neither little nor great, nor quantitative at all."

On the contrary, the very principle of the calculus is that infinitely little magnitudes are still comparatively little or great, and preserve all their quantitative relations, so that differential co-efficients, or the ratios of such infinitesimals, are definite numbers.

As Hegel's "notion" here again comes into conflict with all that is best established in abstract mathematical science, we must decline to follow Mr. Stirling through his generally incomprehensible vindications of Hegel. When Hegel's philosophy breaks down so sadly at the slightest touch of fact, can we waste our own time, or that of our readers, with endeavouring to attach a meaning to pages of this kind of *philosophy*?—

"The outside *Aarshauung* being viewed as the *con-*

tinuum, the *regula* may be regarded as the *discretum*; but it were a false conception, that of the continuum as made up of an infinite number of *discreta* (regulæ) infinitely small. Such continuum is but the *exemplification*, *proexumbration*, *externalisation* of the regula," &c. (p. 116.)

W. S. J.

OUR BOOK SHELF

Junior Course of Practical Chemistry. By H. E. Roscoe, B.A., F.R.S., &c., and Francis Jones. (London: Macmillan and Co.)

THE work now before us represents the course of practical chemistry carried out by students entering the Owens College Laboratory. It commences with the preparation of the ordinary gases, which are, if anything, too shortly described; and then proceeds to the subject of blowpipe analysis and the preliminary examination of simple substances, and afterwards to the reactions of metals, &c., and qualitative analysis itself. The book does not deal in any way with theoretical chemistry, but the student is referred to Prof. Roscoe's "Lessons in Elementary Chemistry" for any explanation of this kind. This, of course, necessitates a considerable amount of extra reading, more particularly in the earlier portions of the book. The course of qualitative analysis, and so forth, through which the student has to pass, seems to be very similar to that which is now in use in most of our laboratories.

The various experiments, reactions, &c., are as a rule clearly described, but we notice one or two which would undoubtedly be better for some slight alteration and addition; thus, on p. 59, we find the following given as a method of testing for Baric sulphate:—"Barium sulphate fused with Na_2Co_3 and HCl added, yields $BaCl_2$ (flame coloration green), precipitated by $SrSo_4$ solution." Now we think that there is a strong probability that a student proceeding as directed in the book would again form the original Baric sulphate, and he would certainly not obtain any precipitate with Strontic sulphate solution, and probably would not obtain the green colouration. The same method is also given for the detection of Strontic sulphate. Another instance in which we think that clearness has perhaps been sacrificed to brevity is in Table A, but with a teacher at hand there need be little fear but that the student will easily overcome such minor difficulties. In fact the book is written with the desire to aid the teacher in his work, and not to dispense with his services altogether; in the former we think the book is very successful, but we do not believe that a student could well work through the book without such aid.

A number of well-selected questions is appended at the end of the book. They seem well adapted to test the student's knowledge of his work, and will in this way considerably lighten the teacher's labours.

We must also not forget to mention in terms of high praise the three short rules for the guidance of students, which are appended by Prof. Roscoe at the end of the preface, and we hope that every student who works by this volume will lay them to heart, and practise them with all sincerity.

The title of this book, "Junior Course," &c. scarcely conveyed to our minds exactly what we have found the book to be. It is more advanced than we anticipated, and yet, perhaps, it is not a thoroughly complete manual of qualitative analysis, although nearly so; but we must still thank the authors for a clear and succinct little manual, which will no doubt prove very useful to both teachers and students.

The Philosophy of Evolution. An Actonian Prize Essay. By B. T. Lowne. (Van Voorst.)

THE author of this short sketch of the theory of evolution is already favourably known by his treatise on the anatomy of the Blow-fly, a strictly anatomical work, abounding in detail, and not going beyond the region of fact. We can scarcely congratulate him, however, on the success of his theoretical attempts, as many of them are but weakly based, and others lead to very unreasonable deductions.

In the discussion of the variations which, according to the Darwinian hypothesis, give rise to the development of new forms, Mr. Lowne terms the greater tendency possessed, as he states, by some animals, to vary, plasticity, and the less tendency among others, rigidity; and he considers that these characters, plasticity and rigidity, are capable of being transmitted from generation to generation like other hereditary characters. At first sight this may appear highly probable, but to any one who considers the subject, it will be evident that it is based on an erroneous conception of the nature of that so frequently employed, but still ill-understood expression, variation. For the assumption of the existence of a struggle, together with the concomitant "Survival of the Fittest," means that the possible variation in a particular advantageous direction is tending to a limit, or in other words, that the continuation of the struggle is correlated with a tendency to the reduction to a minimum of the power to vary, for directly any advantageous tendency is developed, it is immediately run upon and exhausted.

The chapter on nutrition contains more than one proposition open to criticism; the function is incorrectly defined, and the ultimate destination of foods which is said to be in three directions, namely of nutrition, energy, and excretion, is very misleading. But it is in the explanation of the formation of the antlers of the Deer that a theory is given, which is not exceeded in rashness and lack of foundation by any lately put before the scientific world; the following is a sketch of the argument:—Herbivorous animals, specially ruminants, take into their system a superabundance of salines, the excess of which the kidney is not sufficiently developed to eliminate; consequently, on an axiom laid down by Sir J. Paget (who would be one of the first to object to this abuse of his words) that every part of the body may be looked upon as an excretion to every other part in highly complex organisms, this excess is got rid of by the development of the antlers, which contain a large amount of calcium salt, and are shed every year: the females have no horns, because in them the excess of salts is employed in the formation of the bones of their progeny. Such being the case, we do not know how Mr. Lowne explains the elimination of the salts in the Cavicorn ruminants, and their non-development in the males of all other herbivorous animals.

We cannot agree with our author in his attempt to derive all the higher forms of animal life from aquatic ancestors. Upon this supposition he attempts to prove that the Penguins and Auks belong to the early type of birds, and that they show marked reptilian affinities, but as they do nothing of the kind, his endeavour is worse than feeble. We are quite unable to see how the view "that the aquatic penguins belong to an early type of birds has been materially strengthened of late by Professor Marsh's remarkable discovery of an Ichthyornid type of birds in the Cretaceous shales of Kansas."

The elaborate markings of the flint shields of the Radiolaria and Diatomaceæ being somewhat like the curves which are produced on the surface of a vibrating metal plate, on which sand has been scattered, we are told that "nothing appears more probable than that similar points of vibration and rest exist upon the surface of these shield-forming organisms, and that the excreted silica which forms their shields comes to rest at the nodal points." This explanation is bold, to say the least, considering the very different circumstances under which the results are produced. Mr. Lowne should try to produce the curves or the vibrating metal plate under water.

Natural Theology being the subject for which this essay obtained a prize, some of its dogmas are shortly discussed. In answer to the statement that the hypothesis of a soul is objectionable "on the ground that it is not known to exist in nature, and cannot, therefore, be known to be capable of producing the effects ascribed to it," it is shown "that when the effects are such that they cannot be produced by any known cause, they must result from an unknown cause or causes capable of producing the effects ascribed to them." However, in an earlier part of the work it is remarked that Mr. Darwin has done injustice to his theory by comparing it to the undulatory theory of light, because the latter assumes the existence of an ether, which is an unknown agent. It is therefore to be inferred that the Darwinian hypothesis is on a better basis than that of the existence of a soul, from the perusal of an Actonian Prize essay!

Light Science for Leisure Hours. Second Series. Familiar Essays on Scientific Subjects, Natural Phenomena, &c., with a Sketch of the Life of Mary Somerville. By Richard A. Proctor, B.A. Camb., Honorary Secretary of the Royal Astronomical Society, author of "The Sun," "Other Worlds," "Saturn," "Essays on Astronomy," "The Orbs around Us," &c. (Longmans, 1873.)

THE essays in this volume have already appeared in various journals. Besides the life of Mrs. Somerville, the volume contains the following:—"The coming Transit of Venus, and British Preparations for observing it;" "The Ever-widening World of the Stars;" "Movements in the Star-depths;" "The great Nebula in Orion;" "The Sun's True Atmosphere;" "Something Wrong with the Sun;" an article occasioned by the intense heat of July last year; "News from Herschel's Planet;" "The two Comets of the Year 1868;" "Comets of Short Period;" "The Gulf Stream," "Oceanic Circulation," "Addendum in Reply to Dr. Carpenter;" "Climate of Great Britain;" "Low Barometer of the Antarctic Temperate Zone."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Pay of Scientific Men

IT is unfortunately too true, as stated in your last week's leading article, that whether the claims of men of Science in serving their country are generally acknowledged in the future must to a large extent depend upon the men of Science themselves. I say unfortunately because, as a general rule, such claims, at least as far as pecuniary rewards go, could not be left in worse hands. I know so well how utterly repugnant it is to the feelings of all true and earnest workers in Science even to speak of such matters, however much they may be compelled to feel them sometimes, that they will be the last to force public attention to the question. Though this may be a natural and honourable feeling as far as each individual is concerned, I cannot help thinking that it is one which for the sake of the Science they love, it is a duty to place, for the time at least, in abeyance.

Very much has been said and written of late about the "Endowment of Scientific Research." I, for one, hold what you would probably consider rather heretical views on the subject, believing that the "protesters" against the report of the Committee on the Organisation of Academical Study, as well as the writer of your recent articles on the subject, are rather running the risk of losing a very substantial and comparatively easily attainable method of reaching the end we all have in view, whilst so keenly pursuing a very shadowy ideal. I think that Scientific Research can be endowed *indirectly*, so effectually and at comparatively so little trouble in overcoming old prejudices, and all the various obstacles to radical changes of organisation which I need not specify, that this should be the first object of all who have its promotion at heart. The far more difficult question of *direct* endowment will follow more appropriately and be carried out more efficiently when the body of educated scientific men in the country is larger than it is now, and the public generally, especially those in high places, have more appreciation of the claims of Science for its own sake.

The educated men of Science in this country are still but a handful; we want more, and there is but one way of obtaining them. Pay them better for their work, that it may be worth while for parents to allow their sons of promise to take up a scientific calling. What our Universities and to a certain extent our Government are now beginning to do to encourage scientific education, viz. offering prizes, scholarships, and even fellowships is a delusion and a snare, unless followed up by something more substantial.

There will never be wanting young minds ardent enough to commence the pursuit of Science for its own attractions, but it is positive cruelty to lure them on by bribes further in a path which will only lead them to the edge of a precipice or into a morass of hopeless difficulties. To be supported in a scientific pursuit when young, is of very doubtful advantage, if you are to find yourself landed in middle or old age, encompassed by all the stern realities of life and all the needs engendered by our complicated social system, with only the miserable and precarious pittance now accorded even to some of the most able veterans of Science. It is this which naturally and rightly discourages scientific research in this country; and it is this which could to a large extent be so easily remedied.

The urgent want is better paid appointments which can be held by men of high scientific attainments, more especially professorships at the Universities. I must confess that I am not one of those who think that a moderate amount of teaching work or even official duties of a scientific nature is any hindrance to a life of healthy and genuine advancement of Science by original research. On the contrary, they may be (if not overdone, as usually is the case in this country) rather an assistance; but that is a long question which I need not discuss on the present occasion.

As such appointments would probably only be given to those who had already shown evidence of their ability by their contributions to knowledge (and this will be more and more the case as the number of available candidates increases, and public opinion forms itself in such matters) the prospect of attaining one would be the greatest possible stimulus to scientific research in young men. Scholarships and Fellowships are valuable adjuncts to the training of such men, but nothing more. What I contend for is that if Science, as a profession, is to compete in its attraction with other callings, as law, medicine, the civil services, to say nothing of trade, we must provide far more liberally than at present for the endowment of the latter half of the lives of those that follow it. That a man should be able to grow wealthy by Science is not asked for, probably not to be desired. The advantages and pleasures of a life devoted to scientific pursuits are such that for myself (and probably most others would say the same) I would prefer them with a simple competency—by which I mean sufficient to join freely in intellectual society and to give one's children a good education—to the wealth of a millionaire acquired in any other way.

But in the present condition of things Science does not even do this, at least for the branches with which I am best acquainted. Some pursuits, such as chemistry, which bear more directly on the arts and commerce, stand on a different footing; but in biological Science I do not know of a position in the kingdom to which a man, however distinguished he may be in his subject, can aspire, in which he can live as I have described, unless aided by independent means.

To remedy this we want no new organisations; nothing, in fact, but the simplest and most intelligible change in the present state of things. In the first place the Government ought at once to increase the pay of all its scientific officers, such as the Astronomer Royal, the Director of Kew Gardens, and especially the Curators of the British Museum referred to in your article.

Secondly, the Universities, as bodies specially interested in the advancement of learning, and having (at least in the revenues of the Colleges) immense resources at their disposal which could legitimately be devoted to such purposes, ought to lose no time in largely increasing the number and the emoluments of their scientific professors, as has been so long and ably urged by the Rector of Lincoln College.

Lastly, certain still more strictly scientific bodies, who have in their own hands the appointment and pay of their fellow-workers, are especially concerned in showing their appreciation of their services, as it may fairly be taken as a standard by which the other cases may be judged. It is gratifying to find that in some of these bodies a liberal spirit is spontaneously showing itself, as in the case of the one with which I have the advantage of being associated. The Council of the Zoological Society is another example, although even here it takes time to shake off the narrow spirit of illiberality or economy which has so long prevailed in such matters. We think nothing (and very properly) of paying a judge or a bishop 5,000*l.* a year, but a fifth part of that sum for a first-class scientific man still seems to many a preposterous extravagance. There are many societies which, being mainly supported by scientific men themselves, are unfortunately without the means of doing justice to their officers, however much it might be their wish; but I cannot conclude without referring to one body which I think really might be expected to set a better example—a body composed solely of scientific men of the highest character, who have the nearly uncontrolled use of a large sum of public money to spend in carrying out a great scientific object; I mean the Meteorological Committee of the Royal Society. Whatever the committee may do personally in the way of suggestion and guidance, the real efficiency of the operations carried out under their care must depend upon the chief executive scientific officers. The committee, in fixing the proportion of the 10,000*l.* annually placed at their disposal by Parliament, which is devoted to the remuneration of these officers, afford, I am afraid, an illustration of what I stated in the beginning of this letter, that scientific men are not the best fitted to take care of their interests or those of their class. Eight hundred and four hundred a year respectively for the Land and Marine Superintendents of the departments, are considered by the committee as sufficient remuneration for such responsible posts. If a body of the first scientific men in the land think it is so, who can wonder that very unscientific Lords of the Treasury should be of the same mind. Doubtless it was with some fear of the same Lords in their eyes, that the committee fixed the lowest possible standard at which they thought they could get the work done. Happily for themselves and the country, they found competent *amateurs* willing to undertake it; but from such a body a different line of action might be expected; they should lead, not follow, the instincts of Chancellors of the Exchequer in such matters. If scientific men are reluctant to speak on such topics for themselves, the lovers of Science among men of influence, wealth, and position, are the more bound to speak for them.

July 21 W. H. FLOWER

Habits of Ants

SOME months ago (vol. vii. p. 443) I sent you an extract from a letter from Mr. Hague, a geologist residing in California, who gave me a very curious account of the terrifying effect on the other ants of the sight of a few which he had killed on one of their paths. Mr. Traherne Moggridge saw this account in NATURE, and wrote to me that he had heard from a gentleman who had lived in Australia that merely drawing a finger across the path deters ants from crossing the line.

Mr. Moggridge tried this experiment with some ants at Mentone with similar effects. I therefore sent the letter to Mr. Hague, and asked him to observe whether his ants were alarmed by the smell left by the finger, or were really terrified by the sight of their dead and dying comrades. The case appears curious, as I believe no one has ever observed an invertebrate animal realising danger by seeing the corpses of a fellow species. It is indeed very doubtful whether the higher animals can draw any such inferences from the sight; but I believe that everyone who has had experience in trapping animals is convinced that individuals who have never been caught learn that a trap is dangerous by seeing others caught.

Here follows Mr. Hague's letter, fully confirming his former statement. CHARLES DARWIN

"By a somewhat singular coincidence the first reappearance, since last winter, of any ants in the room where I then observed them occurred on the day when your last note arrived,—that is, after an interval of several months. Then a few were observed about the tumbler at the middle of the shelf and the vase at the other end from that whence they were first driven, although they all came from a hole near the base of the mantel, directly beneath the vase which they avoided.

"Acting on Mr. M's. suggestion, I first tried making simple finger marks on their path (the mantel is of marble) and found just the results which he describes in his note, as observed by himself at Mentone, that is, no marked symptoms of fear, but a dislike to the spot and an effort to avoid it by going around it, or by turning back and only crossing it again after an interval of time.

"I then killed several ants on the path, using a smooth stone or a piece of ivory, instead of my finger, to crush them. In this case the ants approaching all turned back as before and with much greater exhibition of fear than when the simple finger-marks was made. This I did repeatedly. The final result was the same as obtained last winter. They persisted in coming for a week or two, during which I continued to kill them, and then they disappeared and we have seen none since. It would appear from this that while the taint of the hand is sufficient to turn them back, the killing of their fellows, with a stone or other material, produces the effects described in my first note. This was made clear to me at that time from the behaviour of the ants the first day that I killed any, for on that occasion some of them approaching the vase from below, on reaching the upper edge of the mantel, peeped over and drew back on seeing what had happened about the vase, then turned away a little and after a moment tried again at another and another point along the edge with the same result in the end. Moreover, those that found themselves among the dead and dying, went from one writhing ant to another in great haste and excitement, exhibiting the signs of fright which I described.

"I hardly hope that any will return again, but if they do, and give me an opportunity, I shall endeavour to act further on Mr. M's. suggestion. "JAMES D. HAGUE"

San Francisco, June 26

Fertilisation of Viola tricolor and V. cornuta

ALLOW me to thank Mr. Kitchener for his correction of my spelling. What I object to in the word "be-pollen" is the harsh combination of syllables, which I should have thought would be offensive to any ears, whether scientific or not. The word "pollen," used as a verb, would be free from this fault, and would be objectionable chiefly from the possibility of confusion arising from the novelty of its use in this sense. Neither of these objections could apply to Mr. Kitchener's term "be-dust," but why coin a new word when a simpler one exists ready-made? Does not the ordinary English verb "to dust" equally give the exact meaning of *bestauben*? I cannot, however, agree with Mr. Kitchener that it would be more expressive than "pollinate," as, unlike the Germans, we do not habitually use the word "dust" as a synonym for "pollen." I have no wish to dispute Mr. Bennett's conclusion that *Viola tricolor* is very commonly fertilised by "very minute insects of the Thrips kind," but only to point out that in its whole structure the flower seems rather adapted for cross-impregnation by larger insects, and that at least some varieties are attractive to humble-bees. On this view, the opening between the two lower anthers, described by Mr. Kitchener, is necessary for the escape of the pollen, which falls, according to Hildebrand, without the help of insects, into the groove beneath, where it is held by the lining hairs until removed by insects. Besides humble-bees, I have seen the small cabbage butterfly (*Pieris rapæ*) sucking the flowers of a cultivated pansy.

With regard to *V. cornuta*, besides the absence of the black mark on the style, mentioned by Mr. Kitchener, which is not universally present in *V. tricolor*, it differs from the latter in the uniform size of the unvariegated, pale blue, or white flowers, the somewhat looser disposition of the petals, the great length of the spur, and the sweetness of the flowers at night, all characters leading to the belief that it is, in fact, a pansy (if I may use the word in a sub-generic sense), adapted to uniform conditions of life, and to fertilisation by *Noctuidæ*. A comparison of the present condition of two beds of this species in our garden, in connection with their surroundings, helps to strengthen this belief, of the practical truth of which I have been able to satisfy myself by the capture of *Cucullia umbratica* in the act of sucking the flowers. One of these beds, in an exposed part of the garden little frequented by moths (as I can testify from long experience), still displays a profusion of blossoms in all their virgin beauty, with only a few small capsules among them; in the other, in a sheltered nook, an old favourite "mothing-ground," the flowers are mostly past their prime, and a great number of well-filled capsules are already formed. By day I have seen the flowers visited by a few humble-bees, which seemed to have difficulty in reaching the nectar, and by the meadow-butterfly (*Hipparchia Janira*). Hosts of small flies run over the petals in bright sunshine, but rarely attempt to enter the nectary, and I have never seen such an attempt succeed. A remarkably long-beaked fly which I watched feeding on the pollen, as it repeatedly inserted and withdrew its proboscis, must probably have left some of the flower's own pollen on the stigma. W. E. HART

Kilderry, Co. Donegal, June 22

Spots on the Cherry-laurel

CAN any of your readers tell me of what use to the plants are the small spots—glands I suppose—on the back of the laurel-leaf near the bottom of the rib? Sometimes there are two pairs, sometimes one; but no leaves seem to be without them. They are most apparent in the young leaves. They evidently contain something delectable to the bees, which frequent the laurels very much this year, and always fly to these spots upon the leaves; and the microscope shows a drop of liquid. J. M. H.

Sidmouth

YOUR correspondent means, I suppose, the cherry-laurel. His observation is quite correct; such glands are to be found in similar situations on other leaves. I know of no explanation of their purpose or origin. W. T. THISELTON DYER

Turnham Green, July 10

Halomitra

THERE is a singular morphological coincidence between the specimen of *Orbitolites tenuissimus* Carpenter, figured on p. 91 of "The Depths of the Sea," and several specimens which I have seen of the corallum of a species of the *Fungia* group, genus *Halomitra* Dana. The *Orbitolites* has the appearance of having been developed on a nucleus formed by a frustum of a former specimen. The outer rings are altogether unconformable with those of the truncated segment composing the nucleus; and it is somewhat interesting to notice, as illustrated by the figure in Prof. Wyville Thomson's work, how the growth of the Foraminifer, oppressed at the corners and advancing *per saltum* at the excavated sides, has shaped itself towards the completion of its normal disc-like form.

An appearance precisely similar has come under my notice in the corallum of *Halomitra*. Two specimens in the Free Public Museum, Liverpool, from the Solomon Islands, exhibit this peculiarity, and of about eight or ten other specimens seen by myself, I cannot recollect more than one in which the large frustum of a former corallum, constituting an unconformable nucleus, did not distinctly appear.

In a single case the presumption would be altogether in favour of attributing the peculiarity to an accidental fracture of a former corallum; but its frequent occurrence suggests that it may be worth while to inquire into the possibility of spontaneous fission taking place in the adult *Halomitra*. Some of the *Fungidæ* are said to possess powers of limited locomotion. It is quite conceivable that a great extension of size in the coral might interfere with its mobility and render division advantageous. That the Zoantharian Actinozoa are able to re-absorb solid portions of their coralla is variously illustrated, no example being more familiar than that of the young of several species of *Fungia*, which are attached to the under side of the parent polype by a strong neck of cœnenchyme, which is subsequently absorbed and the young are liberated.

Rainhill HENRY H. HIGGINS

Periodicity of Rainfall

I HAVE observed in recent numbers of NATURE a discussion upon the subject of the Periodicity of Rainfall, and its connection with sun-spots, and I hoped by an examination of the Rainfall Returns of this island (Barbados), which I have collected for 30 years, 1843 to 1872, to have been able to confirm the theory broached by Mr. Meldrum and Mr. N. Lockyer, which is so interesting in itself, and might lead to such important results. But assuming that sun-spots affect all parts of the globe equally, and that periodicity prevails in all alike, the experience of Barbados is opposed to the theory, and I am led to the conclusion that it was "chance alone" that led to the coincidences noticed by Mr. Symons in his letter published in vol. viii. p. 143.

In the following calculation I state the years separately in order to show that not only the triennial and quinquennial averages, but the individual years, contradict the theory. I am able to furnish six periods—three of maximum and three of minimum sun-spots. Of the triennial averages two of each show an absolute equality; in the third the rainfall is in an *opposite* proportion to the sun-spots. The quinquennial averages do not materially disturb those results. As regards individual years, the rainfall was much above the average in two of the minimum sun-spot years; and was above it only in one of the maximum sun-spot years; in the second it was an average; in the third it was excessively below it. The average of the island for 25 years, from 1847 to 1871, is 57 74 inches, based upon the mean of 3 stations in 1843, and increasing to 141 in 1871.

		Yearly Rainfall.	Average of 3 years.	Average of 5 years.
	43	45 31		
Minimum	1844	74·45	54·56	
	45	43 91		
	46	65 82		
	47	48·10		
Maximum	1848	63·77	54·86	59·67
	49	52·77		
	50	67·88		
	54	50·88		
	55	77·31		
Minimum	1856	48·49	62·23	56·56
	57	60 90		
	58	45·22		
	58	45·22		
	59	56·22		
Maximum	1860	57·91	61·98	58·09
	61	73·82		
	62	59·27		
	65	68·64		
	66	59·68		
Minimum	1867	69·93	58·07	58·27
	68	44·60		
	69	48·52		
	69	48·52		
	70	60·17		
Maximum	1871	41·46	50 00	52·71
	72	48 39		
	73	65·00		

I have ventured to estimate the rainfall of the present year with much confidence upon the data given in the accompanying notice, with which I need not trouble your readers.

Barbados RAWSON W. RAWSON

NOTES FROM THE "CHALLENGER"

IV.

ON Saturday, the 15th of March, before going into the harbour of St. Thomas, a sounding was taken in 450 fathoms off the island of Sombrero. The bottom brought up by the sounding machine was globigerina mud largely mixed with broken shells, chiefly those of pteropods. The dredge was put over early, and veered to 1000 fathoms. At noon it was hauled up half filled with calcareous ooze. It was again sent down, and brought up early in the afternoon with a like freight. These dredgings, which we did not regard as entering into the regular work of the sections, but which were only undertaken to give us a general idea of the deep-water fauna of the West Indian province, may be taken in connection with one or two hauls taken with the same object and under the same circumstances, in waters of nearly equal depth on the 25th of March, after leaving St Thomas. The careful examination of this zone, between 300 and 1,200 fathoms among the West Indian Islands, will undoubtedly add enormously to zoological knowledge. The objects of the present expedition do not, of course, include a detailed investigation of this kind, which must be done quietly in a small steamer, by some one on the spot, and will require the patient work of several years. Even the few hauls of the dredge which we had it in our power to make, brought to light a number of new and highly interesting forms, representing nearly all the invertebrate groups. A thorough investigation of the belt must yield a wonderful harvest.

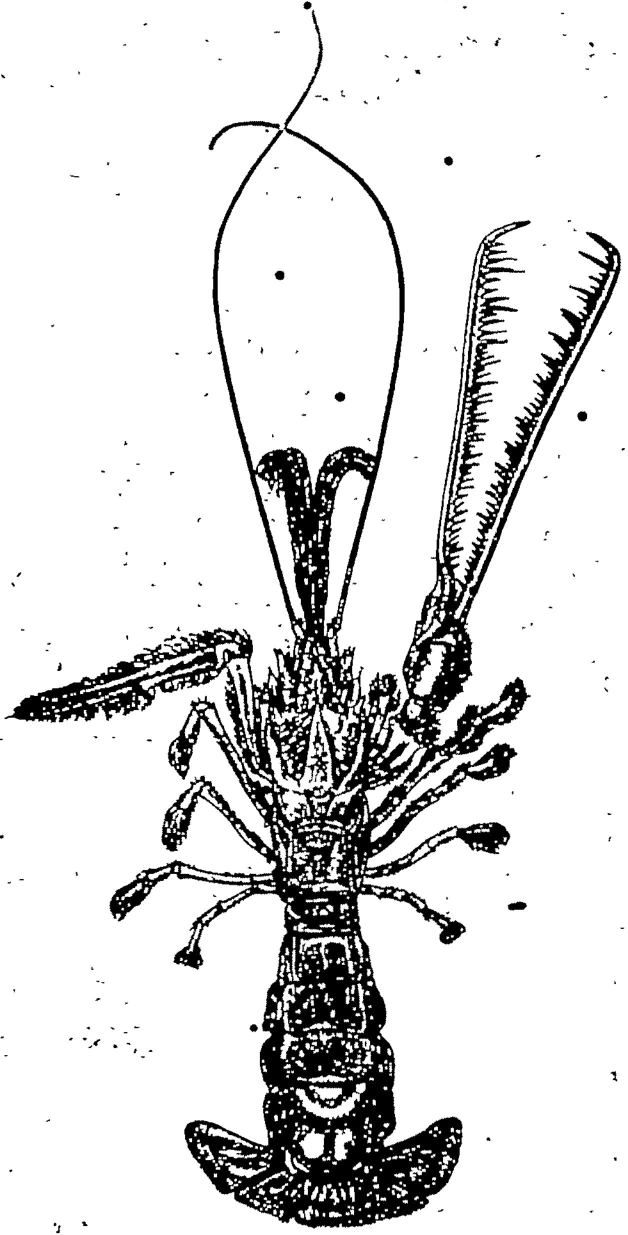

FIG. 1.—Astacus Zaleucus, v W.-S.

In those dredgings on the 15th we got several sponges belonging to the Hexactinellidæ, very closely allied to those which we had previously met with in moderately deep water off the coast of Portugal, showing that the distribution of this remarkable order in deep water is very wide. Several stony corals occurred, but of all these, with the exception of a species of *Stylaster*, which was very abundant at this station, we got better examples on a subsequent occasion. The *Stylaster* agrees very closely with the description and figure given by Pourtalès of *S. complanatus*. The only marked difference is that the primary and secondary septa do not unite to the same extent as shown in the figure.

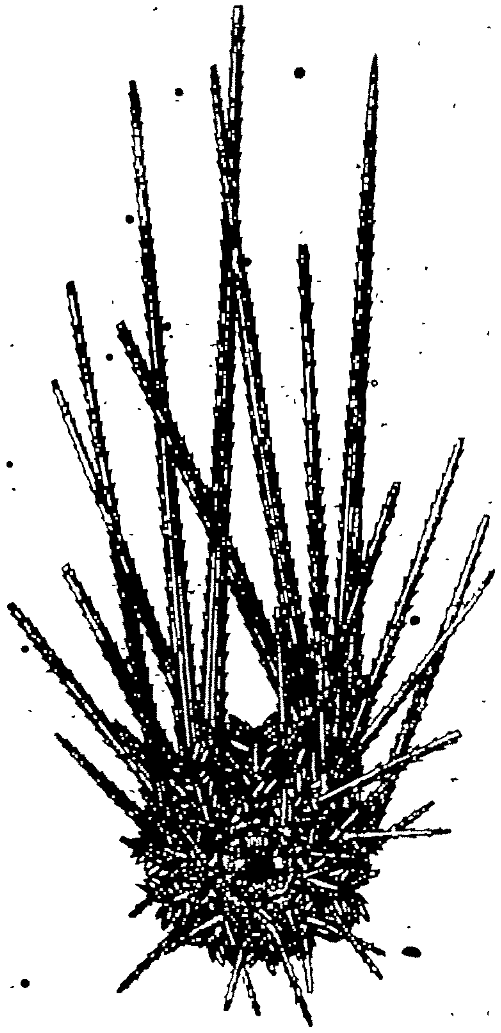

FIG. 2.—Salenica Varispina, A. Ag.

In this dredging two very interesting crustaceans occurred, both belonging to the decapod family Astacidæ, and both participating in a singular deficiency, the total absence of eyes. One of these has been referred by Dr. v. Willemoes-Suhm to his genus Deidamia. It agrees with the species described in my former report in all its leading characters, although certain marked differences must lead to a slight modification of the characters of the genus as formerly defined. In *Deidamia leptodactyla* all the five pairs of ambulatory legs bear chelæ, while it is a character of the typical Astacidæ that chelæ are present on three pairs only. In the new species there are chelæ on four pairs of the ambulatory legs, the fifth pair ending in simple curved claws. The two species agree with one another, and with *Astacus*, in possessing a lamellar appendage at the base of the outer antennæ, and with this they have the flattened carapace of *Palinurus*. These characters have not been hitherto observed in combination, and their so occurring seems to be a more valuable generic character than the variable one of the form of the limbs. The character of this genus will now stand thus:—

Deidamia.—Cephalothorax flattened, with a compressed free lateral margin. A lamellar appendage at base of each of the outer antennæ. Swimmerets, consisting of three joints with two palpi. No trace of eyes or of eyestalks.

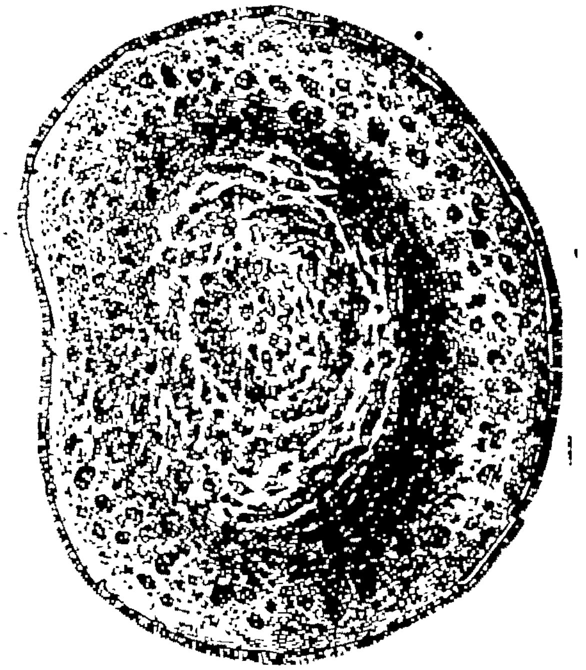

Fig. 3.—Hyalonema Toxeres, Wyville Thomson (Upper surface of sponge body).

D. leptodactyla v. W.-S.—All the ambulatory feet bearing chelæ.

D. crucifer v. W.-S.—Four pairs of the ambulatory feet bearing chelæ.

As in *D. leptodactyla*, not only are the eyes and eyestalks absent, but there is no indication of a space for their accommodation in the position in which eyes are normally developed.

Deidamia crucifer certainly differs widely in general appearance from the recent Astacidæ, at the end of which family we should, however, be inclined to place it for the present. It has a very close resemblance to some fossil forms, particularly the varying species of the genus *Eryon*. It has been already remarked that *Deidamia*, in its flattened cephalothorax, approaches the Palinuridæ; in all the living members of that family, however, the first pair of legs are monodactylous, while in *Deidamia* they are didactylous. The fossil genus *Eryon* forms an exception in this particular among Palinurids, with which it has hitherto been arranged, and has the first pair of limbs didactylous, as in *Deidamia*. It has not yet been ascertained whether *Eryon* has a lamellar appendage at the base of the outer antennæ. If this appendage be absent, there is probably scarcely sufficient ground for separating *Deidamia* generically from *Eryon*. It is very likely that when the recent deep-sea forms near the Astacidæ and Palinuridæ come to be carefully correlated with the cretaceous and Jurassic species, it may be necessary to establish an additional family.

The second crustacean, although having little of the facies of the typical *Astaci*, presents apparently no characters of sufficient value to warrant its separation from that genus.

Astacus zaleucus, v. W.-S. (Fig. 2), with its long compressed cephalothorax, flattened abdomen and unequal chelæ, has at first sight somewhat the appearance of a *Calianassa*.

The total length of the animal is 120 mm.; the cephalothorax, 50, and the abdomen, 60 mm. The carapace is hard, and firm, though only slightly calcified. It is greatly compressed laterally, rising into a high arch. It terminates in front in a slender spiny rostrum, 8 mm. in length. The rostrum is covered with a thick felting of hair, which extends backwards, forming two hairy triangles on the anterior part of the cephalothorax. In front of the carapace, between its anterior and upper edge and the insertions of the antennæ, in the position of the eyes in such forms as *Astacus fluviatilis*, there are two round vacant spaces which look as if the eyestalks and eyes had been carefully extirpated and the space they occupied closed with a chitinous membrane. The lamellar appendage of the outer antennæ has teeth along its inner border. It extends to the middle of the second basal segment of the antenna, which is remarkably long. The flagella of the outer antennæ are 130 mm. in length. The inner antennæ originate in a line with the outer. The funiculus is shorter, and the flagella, which are equal in length, are much shorter than those of the outer antennæ.

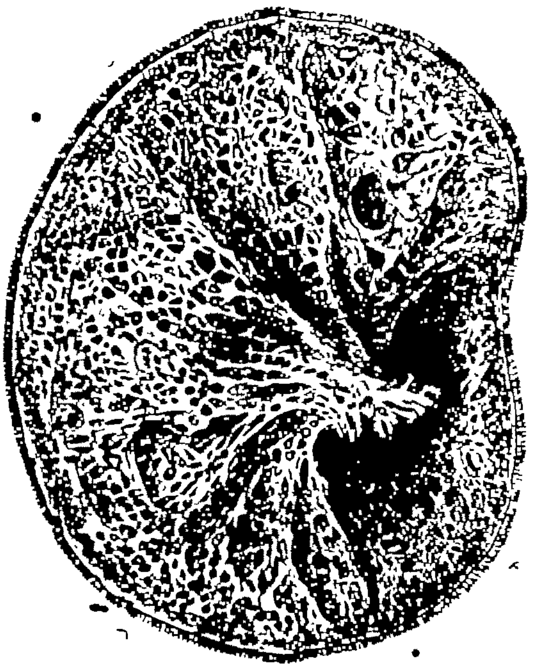

Fig. 4.—Hyalonema (Lower surface).

The parts of the mouth are normal. The three first pairs of ambulatory legs are terminated by chelæ, the fourth pair bear recurved claws, and the fifth abortive stump-like claws. The chelæ of the first pair of legs are strangely developed, particularly the right chela, which is double the length of the left, and with its formidable ranges of long spines along the inner border of each claw reproduces on a small scale the jaws of the Gangetic gavial. The last segment of the pereion is not covered by the carapace but is in moveable connection with it. The first segment of the abdomen is very small, and the segments gradually increase up to the fourth, which the fifth and sixth equal in size. The abdominal segments are flattened from above downwards. The telson is quadrate, and combines with the two pairs of caudal appendages, which are widely expanded laterally to form the caudal fin. The dorsal surfaces of the second, third, and fourth abdominal segments, and the margin of the tail, are thickly covered with woolly hair. The individual being a male, the first pair of swimmerets consist of long slender appendages, and the four succeeding pairs have one strong, round, basal joint, to which are attached two palpi fringed with hair. There is some resemblance between this form and *Calianassa*, but in this genus the lamellar appendage to the outer antenna is absent. There are four pairs of limbs with chelæ instead of three, and the carapace is soft.

To the genus *Astacus*, therefore, with which it has all characters in common except the great development of the right chela and the total absence of eyes—neither characters of generic value—the present species must be referred.

A. Zaleucus, n. sp. (Fig. 1).

Rostrum spiny, elongated. Lamellar appendage of the outer antennæ reaching to the middle of the second joint of the funiculus. Chelæ on three pairs of ambulatory feet, those on the first pair strongly but unequally developed. Cephalothorax very much compressed laterally, eyestalks and eyes entirely wanting.

On Sunday, March 16, we anchored in the Gregaria Channel, at the entrance of the harbour of Charlotte Amalia. We spent a few very pleasant days at St. Thomas, some of the civilians of our party enjoying greatly their first experience of life and scenery within the tropics. M. Garde, the Danish Governor, received us with the most friendly hospitality. He is a naval man, and was greatly interested in our investigations, and his Aide-de-Camp, Baron Eggers, had collected and worked out the plants of the Island with care, and was otherwise well acquainted with its natural history.

The natural history of the island of St. Thomas is tolerably well known, and large collections of its fauna and flora have been sent home from time to time by very competent naturalists to the Museum at Copenhagen. On the present occasion our time was much too limited to attempt to make collections, so the naturalists contented themselves with a little shallow water dredging, and such a general survey of the island and shores as might familiarise them with the more characteristic forms of animal and vegetable life; for while the Atlantic Islands Madeira, and the Canaries, although gradually assuming a more tropical character, maintain the most intimate relations in natural products with the south of Europe, in Tropical America everything is changed, and it takes a little time to become familiar with new acquaintances whom one has hitherto known, if he has known them at all, only from descriptions or figures, or at best mummied or pickled, or otherwise in inadequate effigy. Ophiurideans are particularly plentiful at St. Thomas, and we made large collections of these, particularly of the many large and characteristic West Indian species of the genus *Ophioderma*.

On the 24th of March we left the harbour of Charlotte Amalia and proceeded with a light north-easterly breeze towards the Culebra passage. The next morning we sounded in 625 fathoms. The ooze was closer and more free from shells and coral than in the former haul, but otherwise much of the same character. This time the dredge came up about half full, and on sifting its contents many interesting additions were made to our collections. Here we met for the first time with the curious little crinoid, *Rhizocrinus lofotensis*, for which we had been on the outlook since the beginning of the cruise, and *Salenia varispina*, which we now recognise as a very widely distributed inhabitant of the deeper water.

This elegant little urchin (Fig. 2) is about 10 mm. in diameter of the test. It resembles in general appearance young specimens of *Cidaris hystrix*. The ambulacral zones are narrow, the interambulacral correspondingly wide, and both are furnished with double rows of flat, paddle-shaped, secondary spines beautifully striated in purple and white, ranged along the middle line, from which they shed outwards on either side. The primary tubercles are large, imperforate, and distinctly crenated. Some of the larger of the primary spines are 50 mm. in length, ·8 mm. in diameter, and cylindrical, gradually tapering towards the point. They are fluted and serrated along the ridges with sharp prickles. The spines in all the specimens we have dredged are very uniform; some are slightly curved, but they scarcely agree with the description given by Prof. A. Agassiz, from a young specimen, of being "of all shapes." The spines round the mouth are short, some of them slightly flattened and sharply denticulated.

The corals which were abundant in individuals were all deep-water forms. They have been examined by Mr. Moseley, who refers the majority to species which have been described by M. de Pourtalès * from the Straits of Florida.

Two examples of the sponge-body of a very handsome Hyalonema were sifted out of the coral mud. Unfortunately in both cases the sponge had been torn from the central coil, and the absence of the coil might have thrown some little doubt upon the form and mode of finish of the complete animal, so that it was extremely fortunate that a young specimen of the same species about 40 mm. in length was caught in the tangles quite perfect.

Hyalonema toxeres, new species, resembles closely the other known species *H. lusitanicum* and *H. sieboldi* in general appearance and in the arrangement of its parts. A more or less funnel-shaped sponge presents two surfaces covered with a network of different patterns formed by varying arrangements of large fine rayed spicules. The upper concave surface shows a number of oscular openings irregularly arranged, and the lower surface a more uniform network of pores, some of which seem to be inhalent and others exhalent.

The central axis of the sponge is closely warped into the upper part of a coil of long and strong glassy spicules which, as in the other species, serve to anchor the sponge in the soft mud. Both of the species dredged have the sponge more flattened and expanded than it is in *H. lusitanicum*. In one of them it is nearly flat (Fig. 3), forming a reniform cake-like expansion 80 mm. in length by 70 mm. in width, and about 8 mm. in thickness. The upper or oscular surface is covered by an exceedingly close network with groups of large openings at nearly equal intervals. It is slightly raised in the centre. The central elevation is followed by a slight depression, and the upper wall then passes out nearly horizontally to a sharp peripheral edge fringed with long delicate spicules, each consisting of a slender central shaft with a cross of four short transverse processes in the centre. The outer half of the central axis is delicately feathered. The lower surface of the sponge (Fig. 4) is protected by a singularly elegant net-work of sarcode with wide oval and round meshes radiating irregularly from a central point. The membrane is traversed by irregularly radiating ridges of firmer substance, which unite in the centre in a projecting boss at the point where, in this specimen, the "glass-rope" has unfortunately been torn out.

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* Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College, No. 4—Deep-sea Corals. By L. F. de Pourtalès, Cambridge (Mass.), 1871.

WYVILLE THOMSON

(*To be continued.*)

THE ANCESTRY OF INSECTS

WITHIN a very few days after my last article on the "Origin and Metamorphoses of Insects" appeared in NATURE, I received from Mr. Packard a memoir,* under the above title, in which he developes his latest views on the same subject; and I am happy to find that his views do not differ so much from mine as I had supposed. He lays great stress, as is natural, on the larval forms. "If we compare," he says, "these early stages of mites and myriopods, and those of the true six-footed insects, as in the larval Meloë, Cicada, Thrips, and Dragon-fly, we shall see quite plainly that they all share a common form. What does this mean? To the systematist who concerns himself with the classification of the myriads of different insects now living, it is a relief to find that all can be reduced to the comparatively simple forms sketched above. It is to him a proof of the unity of organisation pervading the world of insects. He sees how Nature, seizing upon this archetypal form has, by simple modifications of parts here and there, by the addition of wings and other organs wanting in these simple creatures, rung numberless changes in this elemental form." And again (p. 151), "Going back to the larval period, and studying the insect in the egg, we find that nearly all the insects yet observed agree most strikingly in their mode of growth, so that, for instance, the earlier stages of the germ of a bee, fly, or beetle, bear a remarkable resemblance to each other, and suggest again, more forcibly than when we examine the larval condition, that a common design or pattern pervades all."

He distinguishes, as in his previous writings (p. 175), two principal types of larvæ:—

"There are two forms of insectean larvæ which are pretty constant. One we call leptiform, from its general resemblance to the larvæ of the mites (Leptus). The larvæ of all the Neuroptera, except those of the Phryganeidæ and Panorpidæ (which are cylindrical, and resemble caterpillars), are more or less leptiform, *i.e.*, have a flattened or oval body, with long thoracic legs. Such are the larvæ of the Orthoptera and Hemiptera, and the Coleoptera (except the Curculionidæ; possibly the Cerambycidæ and Buprestidæ, which approach the maggot-like form of the larvæ of weevils). On the other hand, taking the caterpillar or bee larvæ, with their cylindrical fleshy bodies, in most respects typical of larval forms of the Hymenoptera, Lepidoptera, and Diptera, as the type of the eruciform larva," &c.

At first sight it would appear that Mr. Packard's conclusions differ widely from those which I have advocated. He rejects, indeed, the suggestion made by Haeckel that the "common stem form of all Tracheata" may be found in "Zoeaform Crustacea." It is evident, he says (p. 159), that "the Leptus fundamentally differs from the Nauplius and begins life on a higher plane. We reject, therefore, the crustacean origin of the insects." And elsewhere "we find through the researches of Messrs. Hartt and Scudder that there were highly-developed insects, such as may-flies, grasshoppers, &c., in the Devonian rocks of New Brunswick, leading us to expect the discovery of low insects even in the Upper Silurian rocks. At any rate this discovery pushes back the origin of insects beyond a time when there were true Zoëæ, as the shrimps and other allies are not actually known to exist so far back as the Silurian, not having as yet been found below the coal-measures."

But then he observes that the "larvæ of the earliest insects were probably leptiform, and the eruciform condition is consequently an acquired one, as suggested by Fritz Müller." Again, "for reasons which we will not pause here to discuss, we have always regarded the eruciform type of larva as the highest. That it is the result of degradation from the Leptus or Campodea form, we should be unwilling to admit." And once more, "The Caterpillar is a later production than the young, wingless Cockroach."

Mr. Packard had already expressed these opinions elsewhere, and as I have on the contrary suggested that the grublike, or Lindia-forms were the first to come into existence, then the Tardigrade-form, and lastly, the Campodea-form, I had supposed that our views were in direct opposition to one another: but I am glad to find from other passages that after all there is not so much difference as these passages would seem to indicate. I cannot, indeed, agree with him in his classification of Insect larvæ; he ranks the Caterpillars with the grubs and maggots of Bees and Flies, as a class for which he proposes the term "eruciform" in opposition to the "leptiform" larvæ of Orthoptera, Hemiptera, and most Coleoptera. It seems to me, on the contrary, that the two great groups are the Hexapod or Campodea-form, and the apod, grublike type, which I have proposed to call the Lindia-form. At the first glance, no doubt, the heavy sluggish Caterpillar seems to have more in common with the grub of a Bee than with the active larvæ of Coleoptera. The difference, however, is one of habit, not of type.

As regards the ancestral forms of Insects, Mr. Packard considers that "while the Poduras (p. 154) may be said to form a specialised type, the Bristle-tails (*Lepisma*, *Machilis*, *Nicolitea*, and *Campodea*) are, as we have seen, much more highly organised, and form a generalised or comprehensive type. They resemble, in their general form, the larvæ of Ephemerids, and perhaps more closely the immature Perla, and also the wingless Cockroaches. Now such forms as these Thysanura, together with the mites and singular Pauropus, we cannot avoid suspecting to have been among the earliest to appear upon the earth; and putting together the facts, first, of their low organisation, secondly, of their comprehensive structure, resembling the larvæ of other insects, and thirdly, of their probable great antiquity, we naturally look to them as being related in form to what we may conceive to have been the ancestor of the class of insects. Not that the animals mentioned above were the actual ancestors, but that certain insects bearing a greater resemblance to them than any others with which we are acquainted, and belonging possibly to families and orders now extinct were the prototypes and progenitors of the insects now known."

As regards the probable origin of this Leptus form, Mr. Packard's views are expressed in the following passage (p. 169):—"While the Crustacea may have resulted from a series of prototypes leading up from the Rotifers, it is barely possible that one of these creatures may have given rise to a form resulting in two series of beings, one leading to the Leptus form, the other to the Nauplius. For the true Annelides (Chætopods) are too circumscribed and homogeneous a group to allow us to look to them for the ancestral forms of insects. But that the insects may have descended from some low worms is not improbable, when we reflect that the Syllis and allied genera of Annelides bear appendages consisting of numerous joints; indeed, the strange *Dujardinia rotifera*, figured by Quatrefages, in its general form is remarkably like the larva of Chloeon."

Moreover, though Mr. Packard says that "the caterpillar is a later production than the young wingless, cockroach," he elsewhere (p. 182) says, "it is evident that in the young grasshoppers the metamorphoses have been passed through, so to speak, in the egg, while the bee larva is almost embryonic in its build." Mr. Packard admits then that theoretically the Orthoptera do pass through transformations similar to those of metamorphic insects; though, while bees are hatched in an early larval, "almost embyonic" condition, Orthoptera pass through these early stages rapidly, and within the egg.

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* Being a chapter from "Our Common Insects," by A. S. Packard, Jun. (Printed in advance.)

Mr. Packard then derives the various groups of Insecta as I do from ancestors more or less resembling the hexapod larvæ of Neuroptera, &c.; these from a more acariform type; and these again from lower, more vermiform ancestors.

That the Lindia-type larvæ *of Diptera* are of more recent origin than the Campodea-form larvæ of Neuroptera of course I admit, because the palæontological evidence seems to show that the Neuroptera are a more ancient group than the Diptera; but I am not the less of opinion that the Lindia type itself is more ancient than the Neuropterous.

How far the form of any given existing larva is adaptive and how far it is hereditary, is a comparatively minor, though interesting question, and I am glad to find that there is less difference of opinion than I had supposed between Mr. Packard and myself as to the various stages through which in the long lapse of geological ages the existing types of insects have gradually been evolved.

JOHN LUBBOCK

NOTES ON THE HONEY-MAKING ANT OF TEXAS AND NEW MEXICO*

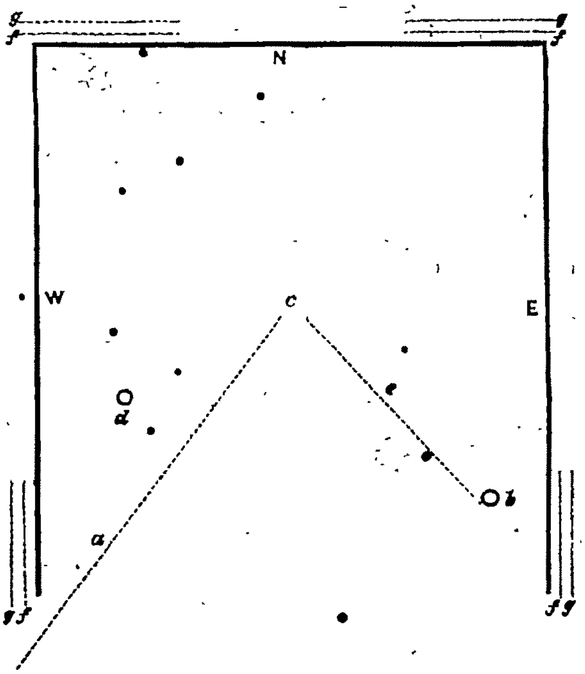
THE natural history of this very curious species (*Myrmecocystus mexicanus* Westwood) is so little known, that the preservation of every fact connected with its economy becomes a matter of considerable scientific importance, and the following observations, gleaned from Capt. W. B. Fleeson of this city, who has recently had an opportunity of studying the ants in their native haunts, may, it is hoped, be not without interest.

The community appears to consist of three distinct kinds of ants, probably of two separate genera, whose offices in the general order of the nest would seem to be entirely apart from each other, and who perform the labour allotted to them without the least encroachment upon the duties of their fellows. The larger number of individuals consist of yellow worker ants of two kinds, one of which, of a pale golden yellow colour, about one-third of an inch in length, acts as nurses and feeders of the honey-making kind, who do not quit the interior of the nest, "their sole purpose being, apparently, to elaborate a kind of honey, which they are said to discharge into prepared receptacles, and which constitutes the food of the entire population. In these honey-seeking workers the abdomen is distended into a large, globose, bladder-like form, about the size of a pea." The third variety of ant is much larger, black in colour, and with very formidable mandibles. For the purpose of better understanding the doings of this strange community, we will designate them as follows:—

No. 1.—Yellow workers; nurse and feeders.
No. 2.—Yellow workers; honey-makers.
No. 3.—Black workers; guards and purveyors.

The site chosen for the nest is usually some sandy soil in the neighbourhood of shrubs and flowers, and the space occupied is about from four to five feet square. Unlike the nests of most other ants, however, the surface of the soil is usually undisturbed, and but for the presence of the insects themselves, presents a very different appearance from the ordinary communities, the ground having been subjected to no disturbance, and not pulverised and rendered loose as is the case with the majority of species.

The black workers (No. 3) surround the nest as guard or sentinels, and are always in a state of great activity. They form two lines of defence, moving different ways, their march always being along three sides of a square, one column moving from the south-east to the south-west corners of the fortification, while the other proceeds in the opposite direction. In most of the nests examined by Captain Fleeson, the direction of the nest was usually towards the north; the east, west and northern sides being surrounded by the soldiers, while the southern portion was left open and undefended. In case of any enemy approaching the encampment, a number of the guards leave their station in the line and sally forth to face the intruder, raising themselves upon their hind tarsi, and moving their somewhat formidable mandibles to and fro as if in defiance of their foe. Spiders, wasps, beetles, and other insects are, if they come too near to the hive attacked by them in the most merciless manner, and the dead body of the vanquished is speedily removed from the neighbourhood of the nest, the conquerors marching back to resume their places in the line of defence, their object in the destruction of other insects being the protection of their encampment, and not the obtaining of food. While one section of the black workers is thus engaged as sentinels, another and still more numerous division will be found busily employed in entering the quadrangle by a diagonal line bearing north-east, and carrying in their mouths flowers and fragments of aromatic leaves which they deposit in the centre of the square. A reference to the accompanying sketch will give a more clear understanding of their course; the dotted line *a* representing the path of this latter section, while the mound of flowers and leaves is marked *c*. If the line *a* be followed in a south-west direction, it will be found to lead to the trees and shrubs upon which another division of the black workers is settled, engaged in biting off the petals and leaves to be collected and conveyed to the nest by their assistants below. On the west side of the encampment is a hole marked *d* leading down to the interior of the nest, which is probably chiefly intended for the introduction of air, as in case of any individuals carrying their loads into it, they imme-



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* By Henry Edwards, Californian Academy of Sciences. Communicated by Mr. Charles Darwin, F.R.S.

diately emerge and bear them to the common heap, as if conscious of having been guilty of an error. A smaller hole near to the south-east corner of the square, is the only other means by which the interior can be reached, and down this aperture, marked *b*, the flowers gathered by the black workers are carried along the line *e*, from the heap in the centre of the square, by a number of smaller yellow workers (No. 1), who, with their weaker frames and less developed mouth organs, seem adapted for the gentler office of nurses for the colony within. It is remarkable that no black ant is ever seen upon the line *e*, and no yellow one ever approaches the line *a*, each keeping his own separate station and following his given line of duty with a steadfastness which is as wonderful as it is admirable. By removing the soil to a depth of about three feet, and tracing the course of the galleries from the entrance (*b*) and (*d*), a small excavation is reached, across which is spread in the form of a spider's web, a net work of squares spun by the insects, the squares being about one-quarter inch across, and the ends of the web fastened firmly to the earth of the sides of the hollow space which forms the bottom of the excavation. In each one of the squares, supported by the web, sits one of the honey-making workers (No. 2), apparently in the condition of a prisoner, as it does not appear that these creatures ever quit the nest. Indeed it would be difficult for them to do so, as their abdomens are so swollen out by the honey which they contain, as to render locomotion a task of difficulty, if not to make it utterly impossible.

The workers (No. 1), provide them with a constant supply of flowers and pollen, which, by a process analogous to that of the bee, they convert into honey. The fact that the remainder of the inhabitants feed on the supply thus obtained, though it is surmised, has not been established by actual observation; indeed with reference to many of the habits of these creatures, we are at present left in total ignorance, it being a reasonable supposition that, in insects so remarkable in many of their habits, other interesting facts are yet to be brought to light respecting them. It would be of great value to learn the specific rank of the black workers (No. 3), and to know the sexes of the species forming the community, their season and manner of pairing, and whether the honey-makers are themselves used as food, or if they excrete their saccharine fluid for the benefit of the inhabitants in general, and then proceed to distil more. I regret that at this time I am only able to bring before the notice of the Academy, specimens of the honey-makers (No. 2), the other members of the community, except from Captain Fleeson's description, being quite unknown to me. It is, however, my hope that at a future meeting I may be enabled to exhibit the other varieties, and to give some more extended information upon this very interesting subject. The honey is much sought after by the Mexicans, who not only use it as a delicate article of food, but apply it to bruised and swollen limbs, ascribing to it great healing properties. The species is said to be very abundant in the neighbourhood of Santa Fé, New Mexico, in which district the observations of Capt. Fleeson were made.

NOTES

THE arrangements for the forty-third meeting of the British Association at Bradford, have been pretty nearly completed. The General Committee will meet on September 17, the opening day, at 1 P.M., for the election of sectional officers, and the despatch of business usually brought before that body. The concluding meeting takes place on September 24. We regret very much to hear that Mr. Joule, on account of ill-health, has been compelled to resign the presidency; Prof. W. A. Williamson, will, it is said, be appointed in his place.

THE forty-sixth meeting of the German Association of Naturalists and Physicians will be held this year at Wiesbaden from September 18 to 24. Communications are to be addressed to Drs. Fresenius and Haas Senior.

WE are glad to see so influential a paper as the *Times* give so prominent a place to a notice of Sir Charles Wheatstone's election to the French Academy, which we ourselves noted a fortnight ago. There is no doubt that if we take into consideration the amount and value of the services Sir Charles has rendered to Science, both in its theoretical and practical aspects, he must be ranked as among the most eminent men of the time. The following is the notice in the *Times*:—

"Sir Charles Wheatstone was elected, on the 30th ult., Foreign Associate of the French Academy of Sciences, to fill the vacancy occasioned by the death of Baron Liebig. He was for many years previously corresponding member of the Academy; but the honour recently conferred upon him is the highest which it is in the power of that body to confer upon a foreigner. The election was nearly unanimous, as he obtained 43 out of 45 votes. Sir Charles has also lately received from the French Society for the Encouragement of National Industry the great medal of Ampère, which is awarded every six years for what is considered the most important application of Science to Industry. The former recipients of this medal were Henri Sainte-Claire Deville, who introduced the manufacture of aluminum; De Lesseps, the Engineer of the Suez Canal; and Boussingault, distinguished for his researches in agriculture.

GUSTAV ROSE, the celebrated mineralogist, died after a few days' illness on the 15th inst., in the 75th year of his age and the 50th of his connection with the University of Berlin. His mind and power of work remained unimpaired almost to the last, and he was able on his sick bed to dictate to his son the results of his last researches.

MR. J. S. DAVENPORT was elected on Wednesday, the 16th inst., Assistant-Secretary to the Royal Horticultural Society, in the place of Mr. Richards, who has accepted a post under the Commissioners of the International Exhibition.

THE Chair of Physiology at Edinburgh is likely to be soon vacant, we believe, by the resignation of Prof. Hughes Bennet, from ill health. There are several likely candidates for the prospective vacancy, all of them good men:—Dr. McKendrick, who has for some time efficiently discharged the duties of the chair, and a paper by whom in conjunction with Mr. James Dewar, on the Physiological Action of Light, we published a fortnight ago; Prof. Rutherford, a former assistant of Prof. Bennet's, and Dr. J. Bell Pettigrew, F.R.S., who has distinguished himself as an investigator in comparative anatomy. There is a rumour that Prof. Burdon Sanderson is also a candidate.

WE regret very much to hear that Mr. Saville Kent has resigned his position in connection with the Brighton Aquarium. We do not desire to express any opinion upon the misunderstanding which has resulted in Mr. Kent's resignation, but we cannot help saying that we consider it a great loss to Science that the Aquarium is now without a resident naturalist. The Brighton Aquarium offers unequalled opportunities for studying the habits of fishes, and during Mr. Kent's short connection with the establishment he has considerably increased our knowledge of this department of Natural History, and we confidently looked to the Aquarium to add still more to scientific knowledge in this direction. It would be a grievous thing, indeed, if the Directors should allow their fine establishment to degenerate into a mere place of popular amusement.

MR. GEORGE SMITH returned on Saturday last from his successful labours in Assyria.

THE number of institutions in America devoted to education of all kinds and of all grades, endowed and supported both by the State and by the generosity of private individuals, theoretically and practically open to all-comers, is almost sufficient to fill a Briton with envy and chagrin, when he contrasts it with the comparative meagreness of the educational means of his own country, hampered with so many traditional restrictions. One of the most admirable, best organised, and most successful of these American institutions is the Sheffield Scientific School of Yale College, the Eighth Annual Report of which for 1872–73 we have just received. The School forms the Scientific Faculty of Yale College, on the same footing as the other faculties of Arts, Medicine, Law, and Divinity, and, to judge from the Report, must be one of the most successful and efficient scientific schools in the world. It owes its name to Mr. Joseph E. Sheffield, who, in 1860, presented it with a magnificent building and a liberal endowment, and has since frequently munificently increased his original gifts, his last one being an additional building of five stories, with ample accommodation, which was very much needed to meet the rapid increase in the number of students, which, during the last session, was 201. The education supplied is in all branches of Science, students being at liberty to choose a course of instruction to fit them for pure scientific research, or for some practical application of scientific principles, as engineering, agriculture, &c. The school is most liberally supplied with scientific apparatus in all departments, seems to have plenty of funds at its command, furnished both by the State and by private individuals, and, to judge from the prospectus, provides students with a thoroughly well-organised and complete course of scientific instruction in each of its numerous departments. "The benefit," the report says, "which the Scientific School has conferred upon the State in turning out young men who, on leaving the institution, are enabled to assume the position of leaders in their several callings, and of educators of the people to a higher grade of culture, increasing the productive brain capacity as well as the material wealth of the country, cannot be estimated in dollars and cents. From all parts of the country come back most favourable reports of the graduates who have been sent out, and their influence, already great, is constantly on the increase. The people of this state cannot do too much for an institution which has already done and is continuing to do so much for them, by developing the material resources of Connecticut, and by extending its reputation throughout the entire country."

IN this month's number of the *American Journal of Science and Art* Mr. Sellack gives a short but interesting account of his photographic work among the southern star-clusters at the Argentine National Observatory, Cordoba, where, for this purpose, he has been for some time at the expense of some gentlemen from Boston, U S.A. On his arrival at Cordoba Mr. Sellack found the lens of the photographic refractor he was to use, broken, but by dint of perseverance and ingenuity he managed to put the pieces together in such a manner as to enable him to obtain a well-defined, nearly circular, photographic image of stars of the first and second magnitude; and with exposures of eight minutes, even stars of the ninth magnitude, of white colour, give a photographic impression. We have received a lunar photograph obtained by Mr. Sellack, and although it will not bear comparison with the well-known photographs obtained by other astronomers who have devoted attention to the subject, nevertheless the impression submitted to us reflects great credit on Mr. Sellack, considering the difficulties he had to contend with in getting it taken. The picture has suffered somewhat by too long an exposure in the telescope and over development.

WE have received from Mr. Gerard Krefft, Curator of the Sydney Museum, what he calls "a splendid bit of mimicry," in the shape of a photograph of the chrysalis of *Papilio sarpedon*. The chrysalis seems to be attached to a leaf, and has itself contrived to assume the shape of a leaf, or rather of a part of the leaf to which it is attached. Its colour, Mr. Krefft says, is pale green, or sea-green.

THE last number of the Journal of the Linnean Society is entirely occupied by Mr. Bentham's important paper on the structure, classification, and history of development of the Compositæ, the largest and most natural order in the vegetable kingdom. In accordance with the system proposed in the "Genera Plantarum," he divides the order into 13 sub-orders, viz.: 1, Vernoniaceæ; 2, Eupatoriaceæ; 3, Asteroideæ; 4, Inuloideæ; 5, Helianthoideæ; 6, Helenioideæ; 7, Anthemideæ; 8, Senecionideæ; 9, Calendulaceæ; 10, Arctotideæ; 11, Cynaroideæ; 12, Mutisiaceæ; 13, Cichoriaceæ; the most important diagnostic characters depending on the structure of the pistil (in the hermaphrodite flowers), fruit, andrœcium, corolla, and calyx (pappus). A very exhaustive account is given of the geographical distribution of the sub-orders and principal families; and the first appearance of the order is traced with probability to Africa, Western America, and probably Australia; the difference between the forms now observed in the northern and southern hemispheres having become developed only after the tropical belt introduced an impassable barrier between them. It is one of the most important contributions to structural and systematic botany which has issued from this country for many years.

Dr. ROBERT SCHLESINGER publishes (from the house of Orell, Fussli & Co., Zurich) a small work on the microscopical examination of Textile Fabrics in the raw and coloured state, with a note on the mode of detecting "shoddy-wool." It contains a complete account of the fabrics made from the various vegetable fibres in more or less common use, also from hair and silk, with their distinguishing characteristics, as exhibited under the microscope, when raw, spun or woven, and dyed, illustrated with 27 woodcuts, and introduced by a preface by Dr. Emil Kopp.

THE current number of the *Zoologist* commences with a paper by Mr. F. H. Balkwill, having the pretentious title "A Difficulty for Darwinists," in which, like many others who do not fully understand the subject, he lays too much stress on the possibility of slight variations in an *infinite* number of directions. No doubt it is theoretically possible for an infinite number of variations to occur in living bodies, if they are within the influence of an infinite number of different forces, just as the result of a very large number of forces acting on a particle, may cause it to take one of almost an infinite number of directions. But the forces acting on the living body are comparatively limited, and when as in the cases of the Thylacine and the Dog, or of the Wombat and the Rodent, which are the author's stumblingblocks, the forces which have been called to act on the Marsupial and Placental types of organism have been practically identical, they having had to undergo the struggle for existence under similar circumstances, it is not to be wondered at, but only to be expected, that similar organisms should be the result, especially as the two types to start with are not separated by any great interval. It is just as probable, external circumstances being similar, that the isolated Marsupial ancestor should give rise to carnivorous, rodent, and herbivorous forms, as that they should be developed from a Placental type.

THE current part of Mr. Dresser's "Birds of Europe" commenced with the description of the Imperial Eagle (*Aquila mogilnik*), to which two plates are devoted, in one of which the young of that species is contrasted with that of the distinctly separated White-shouldered Imperial Eagle (*Ag. adalberti*), from Spain. This is followed by illustrated descriptions of the Algerian Black-headed Jay (*Garrulus cervicalis*), the Siberian Jay (*Perisoreus infaustus*), the White Stork (*Ciconia alba*) several Anserine birds, and the Isabelline Lark (*Galerita isabellina*), which, by the way, does not occur in Europe.

IT is locally stated that among the collections made by the Chilian exploring expeditions on the west coast of Patagonia in the *Chacabuco*, is a specimen of the huemul, an animal which had altogether been lost sight of. There are five well-prepared skins in the National Museum of Chile. Molina mentions it in his "Natural History of Chile," published in 1782. He describes a species of horse (*Equus bisulcus*), or rather an ass, with its hoofs divided like ruminants. He says it inhabits the most inaccessible parts of the Andes, and is difficult to be taken. Mr. E. C. Reed, of the National Museum of Chile, pronounces it to be a stag of the genus *Cervus*, and as not belonging to any new genus.

THE record of the "Astronomical and Meteorological Observations made during the year 1870 at the U.S. Naval Observatory" occupies a bulky quarto volume of about 1000 pages, and contains in its numerous carefully-constructed tables sufficient evidence of the amount and value of the work done at the Observatory. The U.S. Government contribute liberally to the support of the Observatory, the work of which is performed by an efficient staff. One of the most interesting parts of the record of work for 1870 is that describing the details of the Transit Circle.

WE would recommend to all interested in education a pamphlet by Mr. Henry Leedam, a practical teacher, entitled "Complete School Education." It is evidently the result of much thought and observation, and of advanced views of what constitutes a complete education even for boys intended for business. We are glad to see that in his system he gives great prominence to science, as one of the most efficient instruments in general training.

THE Liverpool papers report that a sharp shock of earthquake was felt at Southport on the evening of Wednesday, July 16, accompanied by a loud report. It was thought at first by many that a colliery explosion must have taken place in some of the collieries near Ormskirk, so loud and distinct was the first report. The other three—for there were four shocks—followed much quicker after each other than did the second after the first. There was no undulatory motion such as accompanied the severe shock which occurred about two years since.

ON May 10 a strong shock of earthquake, lasting two seconds, occurred at Opiape, in North Chile.

A COLLECTION of stone implements from Costa Rica, in Central America, has been sent to the American Museum of Natural History in New York.

DON CARLOS MOESTA, formerly Director of the Astronomical Observatory at Santiago, in Chile, has been appointed Chilian Consul-General in Saxony.

THE sixth annual report of the Provost of the Peabody Institute of Baltimore, to the trustees, dated June 5 of this year, is in all respects very satisfactory, and shows that the Institute forms an important means of education, literary and scientific, in the city to which it belongs. The library is a large one, upwards of 50,000 volumes, and the number of readers has increased considerably during the year, the proportion of scientific works sought for being on the whole, as things go, large. During the year 120 lectures were delivered, of which 20 were popular, and 90 special class lectures in particular departments. Though scientific lectures seem to be much less attractive than lectures in literature, still the Provost rightly thinks they should be persisted in, especially as this is one of the main objects of the institution, which is well supplied with scientific apparatus. We have no doubt that by judicious arrangement of subjects and hours, and by securing competent lecturers who know how to make their subjects attractive, scientific lectures will become increasingly popular.

WE give the following on the authority of the *American Artisan*:—The President of Rutgers College, New Jersey, Dr. Campbell, recently found beneath some of the trees in the campus, numerous carpenter bees, each minus its head. Having called the attention of Rev. Samuel Lockwood, the eminent naturalist, to the fact, careful observations were made with interesting results. It was first noted that these bees were all of the same species, and were all honey-gatherers. The case at first appeared to be one of wanton massacre; the merciless executioners being common Baltimore orioles. On making a more thorough examination of the headless trunks, it was discovered that every body was empty, the insect having been literally eviscerated at the annular opening made at the neck by the separation of the head. The interesting fact disclosed by these observations is that these birds had learned that the body of these particular bees—the stingless males—were filled, or contained honey sipped from the blossoms of the horse-chestnut; and so they watched the insects until they were fully gorged, then, darting upon them, snipped off their heads, and always at one place, the articulation, thus showing themselves acquainted with the anatomy of their victims as well as their habits, and taking advantage of both for the gratification of their love for sweets.

THE *Journal of the Franklin Institute* says that the splendid telescope designed for the National Observatory at Washington will, in all probability, soon be erected and in use. The work upon the new tower and dome, intended for its reception, is being rapidly brought to completion. The object-glass—the largest in the world, twenty-six and a half inches diameter, and thirty-two feet focal length—is now finished, and ready for the instrument. The cost of the new instrument, with the necessary machinery, will be about 30,000 dols., and that of the tower and dome, erected to receive it, about 15,000 dols. If to this we add the list of new apparatus already acquired or in process of construction for the Observatory, for the observation of the coming transit of Venus, the Institution will shortly be as well or, perhaps, better equipped than any other of its kind in the world.

THE additions to the Zoological Society's Gardens during the past week include two Argus Pheasants (*Argus giganteus*) from Malacca, presented by his Excellency, Sir H. Ord; a Jaguar (*Felis onca*) from America, presented by Mr. J. H. Murchison; a Himalayan Bear (*Ursus tibetanus*) presented by Mr. G. R. Taylor; two Mulita Armadillos (*Tatusia hybrida*) from Buenos Ayres, presented by Mrs. Mackinlay; two White-crested Guans (*Pipile jacutinga*) from British Honduras, presented by Mr. S. Carmichael; a Patas Monkey (*Cercopithecus ruber*) from West Africa, presented by Mr. E. Hoal; three Black Vultures (*Cathartes atratus*) from America, presented by Mr. C. C. Lovesy; three Fournier's Capromys (*Capromys pilorides*) from Cuba, presented by Mr. J. R. Watkins; a Rhesus Monkey (*Macacus erythraeus*) from India, presented by Miss E. D. Wishart; a Sable Antelope (*Hippotragus niger*), from South Africa, deposited.

RESEARCHES ON EMERALDS AND BERYLS*

PART I. ON THE COLOURING-MATTER OF THE EMERALD.

FROM the time of Vauquelin's analyses, the colour of the emerald was always regarded as due to the presence of oxide of chromium, until the publication of the memoir of Lewy, who ascertained that emeralds contained that element, and concluded that the colour was due to the presence of some organic substance. Lewy also affirmed that the deepest tinted emeralds contained the most carbon. Wohler and Rose, on the other hand, having exposed emeralds to a temperature equal to the fusing-point of copper for one hour, without their losing colour, and also having fused colourless glass with minute quantities of oxide of chromium and obtained a fine green glass, considered chromium and not organic matter to be the cause of the colour.

Boussingault, in the course of an investigation of the "morallons," arrived at the same conclusion as Wöhler and Rose; and although admitting them to contain carbon, denied that it was the cause of their colour, inasmuch as they endured heating to redness for one hour without loss of colour. This result has been confirmed by Hofmeister. I have carefully repeated and extended these experiments. The emeralds employed were canutillos from Santa Fe de Bogota. Their specific gravity was 2·69.

One of the above emeralds was exposed for three hours in a platinum crucible to a bright reddish-yellow heat. At the end of the operation it was rendered opaque on the edges, but the green colour was not destroyed. This experiment completely confirms those of Wohler and Rose and Hofmeister. The power of the colouring-matter to resist a red heat having made me inclined to disconnect the question of the colour from that of the presence of carbon, I made experiments to determine whether beryls contained that element, and, if so, to what amount. The experiments given further on, were made at this stage of the inquiry, and the result showed that the beryl analysed† contained the same amount of carbon as Lewy's emerald.

Although demonstration had been obtained of the presence of carbon in the beryl A, it was still possible that it might have been derived from the decomposition of a carbonate. To settle this question, an apparatus was so arranged that the beryl could be treated with sulphuric and chromic acids successively. It was found that no carbonic anhydride was liberated by sulphuric acid, but the addition of chromic acid caused it to appear immediately. The numerous precautions taken are fully described in the original paper.

Strictly comparative experiments were then made upon minute quantities of charcoal and graphite, the results indicating the carbon contained in the beryl A to be in a condition which is more slowly attacked than either charcoal or graphite, and it is probably in the form of diamond, as has been shown to occur with the carbon contained in artificially crystallised boron.

The presence of carbon in beryls does not appear to be invariable. After repeated experiments upon another large beryl from Haddam County, North America, I was unable to satisfy myself that it contained carbon.

The next point I wished to ascertain was the relation borne by the quantity of carbon in the beryl A to that in the emerald. For this purpose I employed a similar apparatus to that used by Dumas in his researches on the atomic weight of carbon previously alluded to. The following percentages were obtained:—

	Beryl A. I.	Beryl A. II.	Emerald.	Lewy. Emerald (mean).
Carbon anhydride . .	0·31	0·31	0·26	0·28
Water	1·35	1·73	1·20	1·89

II.—ON THE EFFECTS OF FUSION UPON EMERALDS AND BERYLS.

On the Effects of Fusion upon Opaque Beryls.—In order to study the effects of fusion upon beryls or emeralds, I found it necessary to use the oxyhydrogen blowpipe. My first experiments were made upon the beryl A; it weighed 62·54 grms., and its density was 2·65.

The phenomena observed on submitting a fragment of beryl to the action of the flame are very beautiful. Having so adjusted the flame that the beryl fuses tranquilly, and is yet at the exact point of maximum heat (if the substance is not too large for the apparatus), it no longer lies as a shapeless mass on the carbon support, but gathers together, rises up, and forms a perfect bead—round, clear, and brilliant. To obtain the adjustment of position necessary for this result, it is indispensable to wear very dark glasses, so dark, indeed, that objects can scarcely be discerned through them in broad daylight. Without this precaution, the minute details of the globule cannot be observed. The heat and glare would also seriously affect the sight. If all is working properly, the bead should be quite mobile; and advantage of this must be taken to keep it incessantly rolling, and yet not remove it from the point where it gives out the most brilliant light. By this means the whole globule is rendered transparent. If, on the other hand, it is allowed to remain without motion on the carbon (unless the globule be very minute), it will be found, when cold, to have a white opaque base, passing into the centre of the bead in a conical form, and entirely destroying its beauty.

The globules thus obtained from the beryl A were clear and colourless, but generally contained a few minute air-globules and striæ, which become obvious under the lens. Towards the end of this part of the investigation I succeeded in almost entirely avoiding these defects; but I have been compelled for a time to abandon experiments in this direction in consequence of the strain thrown upon the eyes.

When chromic oxide is added to the beads, and they are again carefully fused, they acquire a fine green colour; the tint is, however, inferior to that of the emerald. The green beads may, by an intense and prolonged heat, be rendered colourless. With cobalt oxide the beads afford beautiful blue glasses of any desired shade; and in all cases the results are the same as with the artificial mixture of beryl ingredients to be described further on.

The effect of fusion upon the beryl is to lessen the hardness and lower the specific gravity. The globules may be scratched by quartz. The specific gravity was found to be 2·41.

The beryl, therefore, lost nine per cent. of its density in passing from the crystalline to the vitreous state.

I was desirous of carefully comparing this loss of density undergone by beryls with that of rock crystal fused under the same circumstances. I have repeated with great care the determination of the specific gravity of rock-crystal, both before and after fusion. Before fusion it was 2·65, and afterwards, 2·19.

Rock-crystal loses, therefore, no less than seventeen per cent. of its specific gravity on passing from the crystalline to the amorphous state, or about half a per cent. less than is undergone by garnets, according to the observations of Magnus; whereas the beryl A only lost nine per cent., or little more than half as much.

On the Effects of Fusion upon Emeralds.—On heating alone before the oxyhydrogen blowpipe, emeralds bear a bright red heat without losing their colour; and at a heat which causes incipient fusion, the edges turn colourless and opaque, while the centre remains green. After fusion for a short time they yield an opalescent greenish glass, which, kept for a long time at the maximum temperature of the blowpipe, becomes quite transparent and almost colourless. The addition of chromic oxide causes the bead to become of a dull green colour, which is not improved by moderate heating. The fact that emeralds endure a temperature capable of fusing the edges, without the centre losing colour, appears conclusive against the idea of the colouring-matter being organic. The beads produced by the fusion of emeralds resemble those formed in the same manner from beryls; the phenomena during the fusion are also nearly alike; but it takes longer and a higher temperature to produce a colourless transparent bead with emeralds than with colourless beryls. The beads can be scratched by quartz, and the density is reduced to 2·40. The density of fused emeralds is therefore almost exactly the same as the globules obtained in a similar manner from the beryl A.

On the Effects of Fusion upon an Artificial Mixture of Beryl Ingredients.—Being desirous of trying the effects of fusion upon an artificial mixture of the same composition as that of a beryl, I made a series of careful analyses of the beryl A. Even my earlier analyses enabled me to obtain a sufficiently close approximation to the compositions of the beryl A. The following were the proportions used:—

Silica	67·5
Alumina	18·5
Glucina	14·0
	100·0

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[*] Abstract of paper read before the Royal Society, June 19. By Greville Williams, F.R.S.

[†] As this beryl will be repeatedly alluded to in this paper, and especially in the second part, I shall, for convenience of reference, call it "beryl A." It was found in Ireland.

I did not introduce any iron or magnesia, as I regard them as accidental impurities varying in amount.

When a mixture of the above composition is exposed to the flame of the oxyhydrogen blowpipe, it fuses with almost exactly the same phenomena as with the natural beryl. It is, however, as might be anticipated from the absence of iron and chromium, much easier to get a colourless transparent bead with the mixture than with either emeralds or beryls. The greatest difficulty in this respect is, of course found with emeralds. The specific gravity of the artificial fused globules was 2·42, or almost exactly the same as the density of native emeralds and beryls after fusion.

When a small portion of chromic oxide is added to the artificial mixture and the whole is subjected to fusion, the resulting bead is of a rich yellowish green, and in many experiments approached to the emerald tint; but, as a rule, the colour is more of a faded leaf-green: and, although I have never obtained a globule of the vivid tint of a fine emerald, the glasses, when well cut, are quite beautiful enough to serve as jewels. Prolonged heating gradually diminishes the colour, the bead gradually becoming of the palest bottle-green, and, finally, nearly colourless. This result is the same as with the emerald.

The metallic oxide which yields the finest tints when fused with opaque beryls, or the artificial mixture, is that of cobalt. The manner in which this oxide withstands the intense heat of the oxhydrogen flame is remarkable. All tints, from nearly black to that of the palest sapphire, can be obtained, and the resulting glasses, when cut, are extremely beautiful, and have almost the lustre of crystallised gems.

The globules obtained by fusing the artificial mixture of beryl ingredients with didymium oxide show the characteristic absorption-spectrum of that metal in a very perfect manner, the lines being intensely black. Even when the bead is quite opalescent from insufficient heating, the black lines are beautifully distinct in the spectroscope. With a large quantity of didymium oxide the beads are of a lively pink, becoming more intense by artificial light, and, when cut, form very pretty gems. The presence of didymium in sufficient quantity raises the specific gravity to 2·59, being nearly the same as that of the emerald before fusion.

Conclusions.—The evidence given in this paper, showing that colourless beryls may contain as much carbon as the richest tinted emerald, taken in conjunction with the ignition experiments, and the results of the fusion of chromic oxide with colourless beryls, and with an artificial mixture of the same composition, leave me no room to doubt the correctness of Vauquelin's conclusion, that the green colour of the emerald is due to the presence of chromic oxide.

The fact that emeralds and beryls lose density when fused cannot properly be cited as proving that they have been made in nature at a low temperature; for it is quite possible that they were crystallised out of a solution in a fused mass, originally formed at a temperature high enough to keep the constituents of the emerald in a state of fusion, and that the crystals developed themselves during a slow process of cooling or evaporation. The method employed by Ebelmen for the artificial production of chrysoberyl, namely, heating alumina, glucina, and carbonate of lime with boracic acid in a porcelain furnace until a portion of the menstruum had evaporated, yielded crystals of the true specific gravity, showing the density of minerals to be less dependent on the temperature at which they are produced than upon their crystalline or amorphous state.

One crystalline gem (the ruby) has undoubtedly been produced in nature at a high temperature. I have frequently repeated Gandin's experiment on the artificial formation of this stone, and can confirm most of his results. I did not, however, find the density to be quite the same as the native ruby or sapphire, which is, in different specimens, from 3·53 to 3·56. Artificial rubies of the finest colour made by me by Gandin's process had a specific gravity of 3·45, which is not 3 per cent. lower than that of the ruby. The reason for this close approximation will be found in the fact that fused alumina crystallises on cooling. The crystallisation is, however, confused and imperfect, which causes the resulting product to be only partially transparent, and to have a slightly lower specific gravity than the natural gem. It is consequently scarcely correct to call the fused stones made by Gandin's process "artificial rubies."

I have convinced myself that rubies have been formed in nature at a temperature equal, or nearly equal, to that of the fusing-point of alumina, from the circumstance that the reaction between chromic oxide and alumina, which results in the development of the red colour of the gem, is not effected at low or even moderately high temperatures, but requires a heat as high as that of the oxyhydrogen blow-pipe. It is not necessary that the chromium should be presented to the alumina in the form of chromic acid. It appears, therefore, that the red colour of the ruby is not caused by the presence of chromic *acid*. It is, in fact, a colour reaction *sui generis* between alumina and chromic oxide, which, as far as my experiments have gone, only takes place at very elevated temperatures.

SOCIETIES AND ACADEMIES

London

Royal Horticultural Society, June 18.—Scientific Committee.—Dr. Hooker, C.B., F.R.S., in the chair.—Dr. Capanema, from Rio Janeiro, described the destruction in Brazil of orange, peach, and cotton plants, more especially at Milagres, in the province of Ciara, from the attacks of a Coccus. An orange tree of historic interest more than 200 years old had been destroyed by this insect.—Dr. Masters, F.R.S., reported upon a double-flowered variety of *Lobelia erinus*. The calyx was normal, the corolla was affected by a *dédoublement*, the stamens were more or less petaloid, the ovary was represented by obscure carpellary leaves bearing ovules on the margins.—Mr. Lane, of Berkhampstead, sent a cutting of a yellow-leaved variety of Laburnum which had broken from an old stem of the ordinary kind previously budded some time before with the yellow one. The buds which were inserted died, but as in other cases the tendency to variegation in the foliage had been communicated to the stock.—The Rev. M. J. Berkeley stated that he had provisionally referred the thread blight which had attacked the tea plantations in India to *Corticium repens* Berk.

July 2.—A. Smee, F.R.S., in the chair.—Prof. C. Babington sent flowers of a potato in which the petals were replaced by stamens.—Dr. Denny sent a Pelargonium which showed an interesting reversion to one of the original wild forms (*P. inquinans*). It had been raised from Wellington as the seed parent, and Marathon as the pollen parent, both varieties of the nose-gay class.

Edinburgh

Scottish Meteorological Society, July 2.—Sir Thomas Buchan Hepburn in the chair.—The Council reported to the General Meeting, that there are 92 Stations in Scotland in connection with the Society, 5 in England, 4 on the Continent, 2 in Iceland, 1 in Faro, and 1 in South America; that there are 9 honorary, 16 corresponding, and 557 ordinary members; that the value of the Instruments at the Society's Stations amounts to 1,173*l*., of which 218*l*. belongs to the Society, and the rest to local parties; and that during the past ten years, 63 certified Barometers, 59 louvre-boarded boxes, on Stevenson's pattern, for holding thermometers, had been despatched to the Society's Stations, and about 800 thermometers compared in the office. In reply to an application from a Committee of the British Association, the Council have intimated that they will, as hitherto, be glad to make the unpublished meteorological observations in their possession available to scientific men, and free of charge, in so far as the limited means of copying at their disposal will enable them to do so.—Mr. Buchan gave in the report from the Committee which had been appointed to inquire into the subject of the Herring Fisheries in relation to Meteorology. The returns of the fishings at Wick, Buckie, Peterhead, and Eyemouth, for six seasons of thirteen weeks each (1867-1872) had been examined, and the catches of herring compared with the mean daily temperature of the sea and that of the air with the height of the barometer, the direction and force of the wind, storms of wind, thunderstorms, Auroras, and rain. The fishing season at these places, in common with the whole of the east coast of Great Britain, from Scotland to Flamborough Head, occurred during July, August, and September, ending somewhat earlier at the northern than at the southern stations. From the mean daily catch at Wick and Buckie, from which daily returns had been made, it is seen that during the six years the largest average catches were taken between the 13th and 22nd August, and the whole herring season began about the 19th July, and ended on the 2nd September. That period agreed exactly with the highest mean daily temperatures of the sea during the year, and the period of the heaviest catches coincided with that of the absolute maximum temperature of the sea. It is premature to affirm that there is any absolute connection between those two facts, seeing, for example, that the herring season at Stornoway occurred in May and June, but it is, to say the least, a striking coincidence. The relations of the temperature of the sea to the migrations of the herrings will receive further elucidation when, the returns from Stornoway and other places being discussed, it is exactly determined with what critical epochs of the annual march of the temperature of the sea, the herring seasons, and the periods of maximum catches in different districts correspond. In almost all cases the largest catches occurred with a high, steady barometer and light winds, indicating settled weather; and very light catches, in the height of the season, with thunder-storms, a low and unsteady barometer, northerly and easterly winds, and weather more or less stormy. It was recommended that, in the further prosecution of the inquiry, attention be given to investigate the causes which determine the time of the commencement of the fishing, the fluctuations of the catches in different districts or on different days, and the end of the fishing season. Self-registering thermometers, similar to those now in operation at Peterhead Harbour, established at different points on the coast, and observations on the temperature of the sea, by the more intelligent fishermen on their fishing excursions, could not fail to contribute very material assistance to this difficult inquiry. The Committee was re-appointed to continue their investigation of this important question, Mr. Thomas Stevenson, convener.—Mr. Robert Louis Stevenson then read a paper on "Local Conditions influencing Climate in Scotland," in which the effect of shelter from the East and West, and of relative proximity to the sea, were chiefly considered. The mean annual temperature of Unst and Monach, two of the Society's stations, which, being situated on outlying islands, are almost wholly removed from the influence of the land, was found to coincide with the mean sea temperature in their neighbourhood. A series of observations was proposed at three or four stations, provided with thermometers similarly placed and protected, one set being close to the shore, another a mile inland, and the others at intermediate distances, in order to decide in what manner the climatic influence of the sea extends inland. Mr. Milne Home stated his belief that the Society would be able to carry out the proposal.

BERLIN

German Chemical Society, July 14.—C. Bottinger has obtained a new acid from pyruvic acid, by heating it to 130° with a small quantity of baryta. It is well crystallised, and having two atoms of hydrogen more than uritric acid, has obtained the name of hydruritic acid. Its mode of formation is expressed by the formula

$$4C_3H_4O_3 = \underset{(new\ acid)}{C_9H_{10}O_4} + \underset{(oxalic\ acid)}{C_2H_2O_4} + CO_2 + 2H_2O.$$

A. Kekulé and A. Fleischer have treated camphor with iodine and thus transformed it into oxy-cymol, a phenotic body boiling at 231° of the formula $C_{10}H_{14}O$. Prof. Kekulé considers this reaction as a proof for a new graphic formula for camphor, which he intends to prove by further researches.—F. Landolph reported on the action of nitric acid on various cymols. Camphor-cymol yields mono-nitrocymol and mono-nitro-toluyic acid, which is volatile below its fusing point. Cymol from ptychotis-oil yields dinitro-cymol and a mononitro-toluyic acid different from the above and fusing at 184°.—F. Fittica has obtained identical products from the cymols of camphor, ptychotis-oil and thymol. All of them yield two different mono-nitro cymols, one solid, the other liquid.—A. Kekulé has found amongst the products of PCl_5 on phenol-parasulfuric acid a body of the formula $PO . \begin{matrix} OC_6H_4Cl \\ Cl_2 \end{matrix}$, yielding with water a corresponding acid and chlorophenol.—V. v. Richter has found that benzoate and formate of potassium fused together yield both terephthalic and isophthalic acids, a fact which renders untrue many conclusions on the constitution of aromatic bodies which have been founded on the production of either one or the other of the above acids with derivatives of benzol and formate of potash. Prof. Richter thinks that either one or the other of those isomeric acids are formed according to the temperature employed in fusing.—F. Baumstark has found in urine a new neutral crystallised substance of the formula $C_5H_8N_2O$, which with alkalis yields lactic acid and ethylamine.—K. Birnbaum reported on the attraction of water by superphosphate of calcium exposed to moist air.—G. Barbaglia reported on the impurities contained in commercial isobutylic aldehyde (chiefly acetone) derived from propylic alcohol, and on the conditions under which isobutylic alcohol yields acetone.—A. Oppenheim communicated the continuation of his researches on cymoles derived from various $C_{10}H_{16}$ isomers. Those from terpene and from citrene yielding both paratoluylic as well as terephthalic and acetic acids, can only differ in the position of the 2 atoms of hydrogen which they contain in addition to cymol. This renders improbable that all $C_{10}H_{16}$ yielding cymoles should be constituted according to the view lately expressed by Kekulé.

PARIS

Academy of Sciences, July 7.—M. Bertrand, president.—The proceedings commenced with the announcement, by the perpetual secretary of the award of the Albert Medal of the Society of Arts to M. Chevreul.—During the meeting the commission charged with the recommendation of a candidate for the place left vacant by the decease of M. de Verneuil, presented its report. It recommends, 1st, M. de Lesseps; 2nd, MM. Bréguet, du Moncel, Jacquin, and Sedillot.—The following papers were read:—Theory of the planet Saturn, by M. N. J. Leverrier.—On an isochronous regulator constructed by M. Bréguet for the Transit of Venus at Yokohama, by M. Yvon Villarceau.—On the method of action of the water during the reactions accompanying the mixing of neutral, acid, and alkaline solutions, by M. Becquerel.—On the definition attainable with small astronomical telescopes, by M. d'Abbadie.—A direct demonstration of the fundamental principles of thermo-dynamics; the laws of friction and concussion, by M. A. Ledieu.—Thermal researches on saline solutions by M. P. A. Favre.—On the fossils of the phosphatic chalk of Quercy, by M. P. Gervais.—On the development of the plague in the mountainous countries and plateaus of Europe, Africa, and Asia, by Dr. Tholozan.—On the iron ores of the department of Ille-et-Vilaine, by M. Delage.—Experiments on the action of ammonia and the prolonged action of water on the Phylloxera, by M. Guegrand.—On magnetism, by M. du Moncel.—On the variable period of the closing of a Voltaic circuit, by M. Cazin.—On an "absolute" barometer, by MM. Hans and Hermary.—On the dissociation of mercuric oxide, by M. H. Debray.—On a method of comparing different gunpowders, by M. de Tromenec.—On the oxalins, or ethers of glycerin and the polyatomic alcohols, by M. Lorin. Oxalin is produced by the action of oxalic acid on glycerin.—On the zoological position and rôle of the acarians known as *Hypopus, Homopus, Trichodactylus*, by M. Mégnin.—Experimental contributions to the history of digestion in birds, by M. Jobert.—Observations on certain of the organic liquids of fish, crustacea, and cephalopoda, by M. F. Papillon.—On the heat of combustion of explosive substances, by MM. Roux and Sarrau.—New experiments relating to the theory of the thrust of earthworks, by M. J. Curie.

DIARY

FRIDAY, JULY 25.

QUEKETT CLUB, at 8.—Anniversary.

SATURDAY, JULY 26.

BOTANIC SOCIETY, at 3 45.

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THURSDAY, JULY 31, 1873

THE ENDOWMENT OF RESEARCH

IV.

In accordance with the heading, deliberately adopted for this series of articles, the main object of them has been to insist upon the national importance of a direct endowment of research, and to indicate a way whereby scientific investigators, relieved from any incidental duties, may be placed upon a footing of security and competence. In justice, however, to the letter from Professor Flower, published in our last number, it is necessary to give some explanation why the indirect endowment of scientific men by means of the existing professoriate has been comparatively ignored.

Though it is very far from our intention to quarrel with the main drift of that letter, yet it were vain to attempt to disguise the real point of disagreement between Prof. Flower's proposals and those herein advocated. To augment the salaries of distinguished men of Science, whether government officials, executive members of the wealthy scientific bodies, or University professors, and to increase their numbers, is no doubt an object to which certain classes of the public require that their attention should be drawn, as it is also one of the means by which original scientific work would be encouraged. At this point, apparently, Professor Flower would for the present stop; yet we think that there are many and weighty reasons why those who are not content with such a scheme as final, should hold that a favourable time has now arrived for putting forward a more complete system sufficiently elastic to comprehend within its future development the liberal subsidy of all forms of unremunerative Scientific Research.

As to the funds at the disposal of scientific bodies, it is well known that they are so small as to form a scarcely appreciable element in the consideration of the present question, nor is it likely that they will receive much increase; but yet it would be desirable that the method of their distribution should form an example to guide the application of a more complete system. Again, with reference to the Government appointments, the prospect does not appear more encouraging. Our practical politicians are not unnaturally offended by the anomaly that the holders of these offices should confessedly receive pay, not for the work they do, but in honour of their general scientific attainments. The Mastership of the Mint has not been saved even by the illustrious character of its previous occupants, and along with it have gone several subordinate posts which also were honoured by the scientific men who held them. It only remains for some Chancellor of the Exchequer or Minister of Public Works to arise, wholly given over to the less noble doctrines of Political Economy, and Science will lose the remainder of those places which open competition could fill so much more cheaply, and then the public scientific work of which the popular voice demands the accomplishment, may be resigned to the enterprise of pushing newspaper proprietors. This sort of indirect endowment of research may be said to have had its day; it was of a piece with the public sinecures which used to be awarded indiscriminately for literary or other ill-recognised merit. It was extremely useful when no particular kind of work was required in return, and when the national benefits arising from the advancement were less thought of than they are now.

It is, without doubt, to the professoriate at the Universities that the advocates of indirect endowment must turn in the first place, both for the wealth and the organisation they require, and it is on this ground that issue with them must be joined. It is our purpose, therefore, to point out, first, that the Science professors at the Universities are already in a fair way to get both the position and the emoluments which they deserve, and secondly, that to subordinate original research to the paramount duty of teaching is a clumsy expedient which should not, on principle, be systematically adopted.

In the first place it hardly needs to be said that all the tendency of ancient and modern endowment has been in favour of the Professoriate, so that the interests of teaching are already in possession of the field. In the old days when all instruction was of necessity oral, to found a chair was the one means by which the highest forms of new learning could be promoted; and the force of this tradition, acting in harmony with the practical character of Englishmen, who always expect visible results from money spent, has been a guarantee that modern Science, while growing to its present dimensions, should not fail to receive this sort of attention at the Universities. At Oxford, for example, the present holders of the three leading chairs of Chemistry, Physiology, and Physics receive from various academical sources endowments of 800*l* each per annum, and if the other Science Professorships are inadequately endowed, the same may be said of many of those subjects which enter directly into the course for the Arts degree. According to a rough estimate of Mr. M. Pattison's, Science on the whole receives nearly 5,500*l*., whereas Philology, the next highly endowed faculty, gets but 4,000*l*.* It must also be borne in mind that the University Commission of twenty years ago gave a stimulus in this direction which has not died away. Both the University and College authorities are not unmindful of the duty of extending the Professoriate, and endowing it worthily: new chairs are even now in process of foundation, and at Oxford at least it is the rule rather than the exception to confer a full fellowship upon a hardworking professor in whatever department of knowledge, whose statutable endowment is comparatively small. From these statements it would be manifestly wrong to draw the inference that either the physical sciences or the other branches of scientific study are as yet fully represented or adequately endowed at Oxford and Cambridge: the purport of them rather is to show that teaching at the Universities in Science as in other matters has gained a position which can well take care of itself. If the plan were adopted which has worked so well at Glasgow, viz. to allow the professors an official house, and to leave to the fees of their pupils the further augmentation of their salaries, Prof. Flower's demand for a simple competency would be completely satisfied.

The real difficulty, however, will yet remain, for on the one hand we have not yet attained any assurance that we shall get from our endowments anything more than first-rate teaching, and on the other hand we have a large proportion of the University revenues yet to dispose of. It would, of course, be a possible alternative to endow so large a number of professors as to reduce their teaching duties to a vanishing point, and thus avoid the appearance of a radical change and escape the reproach which apparently attaches to the direct endowment of Research. It is not to be supposed that the advocates of indirect endowment intend deliberately to take up with such a subterfuge, yet on any other hypothesis it is as certain as anything can well be, that the original investigation which they put in the second place will come off second best. It were invidious to allude to particular instances, but it is past denial that the original discoveries in Science which once made England famous, and now more or less maintain that fame, neither were nor are achieved by the holders of teaching posts, and it is equally clear that many of the forms into which modern Science is developing are not of such a character as to be capable of being transmitted by oral instruction. The truth seems to be that the intimate connection sought to be established between original investigation and professorial teaching is borrowed from the artificial institutions of another country. It is the chief characteristic of a German University that the full professor, the extraordinary professor, and the *privat docent* make up the class which is there engaged in scientific study not less than in academical teaching, a peculiarity which may be partly attributed to the laborious character of the people, but yet more to the pecuniary poverty of the Institutions. It is in fact from the want of endowments that the emulous spirit of German patriotism has been compelled to exact double work from a single instrumentality. The renowned University of Berlin is indebted for the whole of its resources to the state, and that, a state which is the most frugally administered of any in Europe: and from this cause it has learnt to elaborate an organised system of student teachers and inchoate professors, from whom research is expected as a duty co-ordinate with instruction, while the natural docility and perseverance of the German character have caused these expectations to be abundantly realised. Yet one of the most celebrated of modern German professors is reported to have said, that "the life of a professor would be a very pleasant one, if it were not for the lecturing." No doubt there are many English professors who secretly to themselves would re-echo the sentiment; yet what could sound more absurd if regarded from the ordinary point of view which is popular in this country? Germany indeed has set an example of the novel forms of scientific industry which should flourish at a living University, but the attempt to transfer to Oxford and Cambridge the German system in its integrity would in some respects be a backward step, and would probably prove a failure. The history of our Universities is against it, and their wealth alone serves to vitiate any analogy borrowed from the parsimonious Teuton. They possess, however, a large number of appointments, unconnected for the most part with teaching duties, and originally destined to be held on the condition of study. It would be easy by means of amalgamation and modification of tenure to make these appointments worthy of the acceptance of those who devote their lives to scientific research; nor ought it to be styled "a visionary ideal," to recognise that natural division of labour, which is permitted to us by the magnificent wealth at our disposal, agreeable to English precedent, and in close accordance with the intentions of the founders of Colleges.

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* It should be noticed that the anomalous chairs of Divinity have been throughout excepted from these calculations.

C.

CARNÉ'S "TRAVELS IN INDO-CHINA"

Travels in Indo-China and the Chinese Empire. By Louis de Carné, Member of the Commission of Exploration of Mekong. With a Notice of the Author by the Count de Carné. Translated from the French. (London: Chapman and Hall, 1872.)

THE work, a translation of which is before us, is a history of the expedition despatched in 1865, under the auspices of the French Government, for the purpose of exploring the river Mekong, of which expedition Mons. Louis de Carné was a member. In consequence of his death the work has been carried through the press by his father, the Count de Carné. Mons. Louis de Carné, with every allowance being made for a father's very natural expressions of eulogy and admiration, seems to have been a young man of rare ability and promise, and his untimely death at the early age of twenty-seven, the result of the hardships he had to encounter during the expedition, marks a devotion to the cause of Science worthy of the emulation of all those who are desirous of helping forward scientific inquiry and research. The expedition, the history of which is here detailed, originated in a suggestion by the Governor of the French colony of Cochin-China to his Government, that the river Mekong, at the mouth of which Saigon, the capital of the colony, is situate, might be made the principal route for the commerce passing between Europe and China. There can be no doubt that, could this route be satisfactorily established, the advantage to Europe would be immense; for in addition to a saving of about 1,200 miles in point of distance, the perilous navigation of the China seas, so much dreaded on account of the terrible monsoons by which they are periodically ravaged, might be entirely avoided. Accordingly, in the year 1865 the Marquis de Chasseloup, the French Colonial Minister, sanctioned the scheme of an expedition which should serve the interests of Science, as well as those of the colony, and which, ascending the Mekong from its mouth, where it empties itself into the Indian Ocean, to its sources amid the mountains of Thibet, should report fully on the navigability of that great river, which was then almost unknown beyond the Lake of Augeor, through which the boundary line between the kingdoms of Siam and Cambodgia passes. M. de Carné thus sums up the objects of the expedition:—"It was desired, first, that the old maps should be rectified, and the navigability of the river tried, it being our hope that we might bind together French Cochin-China and the western provinces of China by means of it. Were the rapids, of whose existence we knew, an absolute barrier? Were the islands of Khon an impassable difficulty? Was there any truth in the opinion of geographers who, with Dumoulin, believed that there was a communication between the Meinam and the Mekong? To gather information respecting the sources of the latter, if it proved impossible to reach them; to solve the different geographical problems which would naturally offer, was the first part of the programme the Commission had to carry out. We were required, besides, to report any miscellaneous facts which might throw light on the history, the philology, the ethnography, or the religion of the peoples along the great river, which was to be as much as possible the guiding-thread of our expedition. We had instructions to seek for a passage from Indo-China to China; an enterprise in which the English have always failed as yet."

M. Drouyn de Lhuys, the Minister of Foreign Affairs, heartily approved of the scheme, and appointed young de Carné to represent his department on the expedition. The exploration party started from Saigon in June, 1866, but they were doomed to disappointment, so far as regarded their main object, for it was ascertained that the Mekong abounded in rapids, cataracts, and obstructions of various kinds, which precluded all possibility of a route being found to China in that direction, and after encountering severe sufferings and hardships to which some of their number succumbed, including M. de Lagrée, the chief of the expedition, they returned to Saigon after an absence of about two years and a half.

M. de Carné claims, as the actual results of the enterprise, so far as it was successful, to have "corrected the errors, and set at rest, by lifting the veil from the doubts which had hitherto led geographers to false and uncertain conclusions in describing the eastern zone of the Indo-Chinese peninsula. The capricious windings of the Mekong; the prolongation of its course to the west, at the 18th parallel of latitude; the importance of its affluents; the strength and volume of its waters, and, if I may venture to say so, the proof of its individuality, which, contrary to the received opinion (viz. of the union of the waters of the Mekong and Meinam), continues to the end of its course; the certainty of its entry into Yunan, where it receives the waters of Lake Tali, and into Thibet, where it has its source—all these points were cleared up. In a word, we brought back precise information respecting the whole course of an immense river, which rises amidst the snows, and completes its course under a burning sun. On the other hand, there are the exact observations and seemingly well-founded information respecting the other rivers of Indo-China; as to their position in different parts of their course, and the limits of their basins; and, in addition, many particulars respecting a part of China itself, which had been hitherto the least known."

We understand that an official report of the expedition is in course of preparation, and we have no doubt the present work will be found to form a very useful supplement to it. The volume would, however, be rendered more valuable and complete by the addition of a few maps, the only one it at present possesses being a somewhat rough sketch of the route followed by the exploring party. Whether France will be able, as M. de Carné suggests, to establish a communication between her colony and China by the river Songkoi, which flows along the north of the Annamite peninsula, is a problem which yet remains to be solved.

G.I.F.C.

"MOTHER EARTH'S BIOGRAPHY"

Chronos: Mother Earth's Biography. A Romance of the New School. By Wallace Wood, M.D. (Trubner and Co.)

THERE can be but few with active minds who have not occasionally found, after having grasped the essential points of any inclusive theory, that in moments of ease and quiet thought, it is far from unpleasant to attempt to apply it, by a running analogy, to some series of phenomena entirely different from those to which it was originally intended to relate, and by taking detail after detail, rebuild it on a fresh foundation. Few, however, have the confidence to put their results on paper, and fewer still to submit them to the criticism of a ruthless public.

The theory of evolution has an intrinsic fascination of this kind, especially to those with a cynical turn of mind; for though developed on a purely physical basis, nevertheless its entire applicability to the intricacies of society, puts the facts of every-day life in a manner so bold, and yet so evidently truthful, that, as it were, scales fall from the eyes of its disciples, and the panorama of moral philosophy flashes out in a manner so vivid and unmistakable as never to be effaced. The picture is a monochrome, and negativism is the colour!

As the title of this work indicates, the history of the world from the beginning of time has to be sketched, and the author commences with a vivid exposition of the nebular hypothesis, and the cooling down of the earth to the commencement of geologic time, under the headings of its Birth and Infancy. He then describes the commencement and development of vegetable and animal life. Just as in a tree all life is found in the terminal twigs, so "the species of animals we see on the earth are the twigs of the great animal tree, the body and branches of which have long since perished," and the struggle for existence by which the present forms have been arrived at, leads to the adoption of the fundamental maxim, "Be hungry and you will be great," which is proposed in place of the old adage—"Be virtuous and you will be happy." Further on, the same principle is illustrated in a very different manner: "only iron-clad and zinc-covered trunks are seen on the Western American railroads, all others being smashed up by the remorseless pitching of the baggage-men, employed, it would seem, for the purpose; this is the *Survival of the Fittest.*"

After the world had passed through the early ages of only protoplasmic and invertebrate forms, the vertebrate era commences with "the fishy period." From the amphibian type was developed the reptile, as we are told, thus: "The lizard differed from the frog, and the newt &c., chiefly by breathing entirely through lungs instead of gills, and thus dispensing with water, except as a beverage; forced to magnificent temperance by long ages of death; driven to it by the great propelling power to which we are all more or less victims—the force of circumstances. Thus a second nature is given, and a new type is created. The fish became a reptile. There was no more longing for the good old times; a more glorious prospect in life the world has never seen. The untrod earth was a garden of thick fleshy plants; whole oceans of appetising insects and delicious worms awaited only the eating. And the new-comers grew and throve as never has any immigrant race before or since." The tendency in animals, as we ascend in the scale of life, to assist one way or another in the further maintenance of their offspring, either by development of a nutritive yolk or by feeding them after they are hatched, is certain. "The explanation of this is very simple. As the population of the earth ever increases and competition grows sharper, it is those who have this assistance in their younger days that are enabled to succeed in the world, and to arrive at maturity. And these possess the inheriting tendency to do the same, or very likely a little more, for the new generation than their parents had done for them. 'If I could only give John a thousand dollars when he is twenty-one, I shall be satisfied,' says the sire; 'my father was only able to give me a hundred and a freedom suit.'"

The Reptilian Period is followed by "the Age of Brutes," wherein the maxim "might is right" was the ruling power. This is followed by "the Anthropological Age," that of the present time; a time of advance according to evolution, and not of decadence, for all we know tends to show that "the course of history is one of progress, and that consequently man is an elevated and not a fallen being; that he is a perfected creature and not a degraded divinity; that his course is Excelsior, onward and upward, and not downward." And if we consider the age of Man, in contradistinction to that of brute and reptile, to have been that in which man first appeared on earth, what may the present be considered—but the age of Woman. "Historically considered, her case is very strong. If the position of woman continues to become exalted in the future at anything like the rate it has advanced in the past—granted that she began as the slave of a brute—that future will show not an equality, but woman the ruler, the subordinate man; and these are advantages in her favour which none but the naturalist dreams of."

"A complete equilibrium—when for every desire there shall be a gratification," is the author's deduction as to the future, things being as they are; but "it would seem that life on earth is doomed to die a violent and not a natural death. Man proposes, but the attraction of gravitation disposes," and so "we must be resigned, remembering that after all we are but a mere speck in the *great* celestial economy, which will lose nothing by our death."

The above short account of this eccentric and amusing work, which excels more by the quaint way in which well-known facts are put, than by anything original in itself, will be best supplemented by a perusal of the original.

OUR BOOK SHELF

The Elements of Chemistry. Theoretical and Practical. By William Allen Miller, M.D. D.C.L. LL.D., late Professor of Chemistry in King's College, London. Revised by Herbert M'Leod, F.C.S., Professor of Experimental Science, Indian Civil Engineering College, Coopers Hill. Part I. Chemical Physics. Fifth Edition, with additions. (London: Longmans, 1872.)

ALTHOUGH Parts II. and III. of this well-known manual have needed frequent alteration and revision as the science advanced, Part I. has, until quite recently, experienced but little change from its well-known form. The recent great advances which have been made in what is now so well known, or at least so often heard of, as solar chemistry, have necessitated considerable additions to the edition of 1867, the last that left the hands of the lamented author.

The name of Mr. M'Leod is a guarantee that the work has fallen into good hands. At page 196, a most complete and well-condensed statement of the present aspect of the subject will be found. The early Indian observations of Captain Herschel and others are referred to, and an account of the discovery of the method of observing the chromosphere without an eclipse is given, and also a sketch of the nature of the phenomena thus observed. A very good statement of the present state of our knowledge with regard to the thickening of the F line, and of Frankland and Lockyer's researches on that subject, is also given, and reference is made to their remarkable observation of the different lengths of the metallic lines above the pole, an observation which has since lead to such important results in connection not only with solar and stellar, but with terrestrial spectroscopy. The additions conclude with a very clear and succinct account of our knowledge of the movements of the gaseous masses on the surface of the sun, and the means of measuring their rapidity and direction. The nature of the spectroscopic phenomena of sun-spots is also described, but somewhat briefly. The added portion is illustrated with twelve woodcuts.

Mr. M'Leod's hand is again visible in the chapter relating to atomicity, where he has added in notes several important points in modern chemical theory, which had not been sufficiently explained in the original work of Dr. Miller; and we also notice in the body of the book a short explanation of the graphic and symbolical formulæ now so much used in explaining chemical facts to the student. We most cordially welcome this new and improved edition of an old friend, and congratulate the present editor on the share he has had in producing it.

R. J. F.

The A B C of Chemistry. By Mrs. R. B. Taylor. Edited by W. Mattieu Williams, F.R.A.S., F.C.S. (London: Simpkin, Marshall, and Co., 1873.)

THIS little book is intended apparently for the use of very young children. The attempt to explain the nature of the elements by analogy with the letters of the alphabet is somewhat obscure, though it would perhaps be difficult to find a different method. The book is divided into lessons, and each lesson followed by questions which are, on the whole, well selected. The same cannot, however, be said of the experiments at the end of the book, which all smack strongly of the "conjuring trick." We cannot coincide with the editor in recommending the book to artisans and business men, who, we think, might attempt something a little more advanced, even as a first book. For those, however, who wish to teach children chemistry, it will no doubt be useful.

Third Annual Report of the Wellington College Natural History Society, December 1870 *to December* 1872. (Wellington College: George Bishop, 1873).

IT is disappointing that the first words of this report, as in the case of the Rugby Society which we noticed recently, should be a confession of partial failure: "Natural History," the Preface begins by telling us, "does not flourish at Wellington College . . . The chief reason undoubtedly is, that during the past two years the older Fellows—and in particular the Sixth Form—have ignored the existence of the Society altogether." Judging from what is said at p. 36, the apathy of the older members of the school is owing to some antagonism which exists between the Natural History Society and the Debating Society attached to the school. But, with Mr. Penny, we cannot see that there is any reason why the two societies should be in the slightest degree antagonistic. On the contrary, they might be mutually helpful, both having ultimately the same end in view—to teach the boys to examine, think, and act for themselves. Of course it ought to be remembered what a great innovation a society like that of Wellington College is on the traditional methods of instruction belonging to a school. The work is entirely voluntary, not clearly defined, as in the regular task-work of the school; and the only rewards held out, rewards which it is difficult to get the traditional schoolboy to understand and appreciate, are, besides the direct acquisition of knowledge and the pleasure attending it, development of the power of observation, keenness of insight, and general intellectual vigour. A debating society, with all its undoubted advantages, is apt to become a nursery of boyish vanity; the reward of successful speaking is immediate and very sweet to a tyro, and can be obtained without much labour. The work of a Natural History Society involves much plodding patience, with very little glory to follow; the rewards are intangible, invisible, especially to the boys themselves, and it will take the training of a few generations to teach boyish human nature to love knowledge for its own sake. One of the most valuable means to accomplish this purpose in a school is a society like that of Wellington College, and therefore we would counsel those who are anxious for its prosperity not to be discouraged, but to work on so long as they can get any boys to work with them, using all possible means to insure success. We hope the merely local obstacles will be overcome, and that the next report will have a more lightsome beginning; also that it will contain many papers by the boys themselves, nearly the whole of the papers in the present report being by Mr. Penny and Mr. Lambert, and not one by a boy, though we are glad to see that some papers by boys were read at the meetings. The Rev. C. W. Penny, president of the Society, deserves the greatest credit for the interest he displays in the Society, and the amount of work he does to help on the objects for which it is established. A large number of the papers, full of instruction and interest even to boys, are by him; his predecessor in the presidentship, Mr. Lambert, has also contributed much to make the meetings of the Society attractive and instructive. Appended to the report are pretty full botanical, zoological, and entomological lists.

Familiar History of British Fishes. By Frank Buckland, Inspector of Salmon Fisheries of England and Wales, Corresponding Member of the "Deutscher Fischerei Verein," &c. &c. (London Society for Promoting Christian Knowledge)

THIS is a new edition of the above work, Mr. Buckland having found it necessary, he says, almost to re-write the book. It may be described as a free-and-easy gossip about fishes, the book being largely made up of extracts from all quarters, *Land and Water* especially being very fruitful in material. As might be expected, Chapter xv., treating of *Salmonidæ*, and occupying upwards of 100 pages, a fourth part of the volume, is the most original and valuable. The chapter will be found useful to all who take an interest in the rearing and preservation of salmon. The numerous illustrations are very fairly executed, and the general reader will find the book entertaining and informing.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Endowment of Research

Direct and Indirect Endowment

I SHOULD like to make one or two remarks on Prof. Flower's letter in your last number.

He modestly suggests that his views respecting the endowment of research unencumbered with teaching, or as he felicitously calls it, the *direct endowment of research*, may be considered by members of the Association for the Organisation of Academical Study as "heretical." I venture to think that he is orthodox on the main theoretical position that, *in the long run*, research must be endowed directly as well as indirectly (by the subsidy of teaching professors) and with an *equally* liberal hand. He is at issue with us only, if I take him rightly, as to the *time* when it will be desirable or possible to make a claim for such direct endowment. We contend that *now is the only time* for making such a claim, and for a reason which I will give presently. Mr. Flower, on the contrary, says that while *indirect* endowment of research, by raising the salaries of teachers, may be carried out at once with less opposition from old prejudices, "the far more difficult question will follow more appropriately and [the endowment] be carried out more efficiently when the body of educated scientific men in the country is larger than it is now, and the public generally, especially those in high places, have more appreciation of the claims of Science for its own sake," *i.e.* in the more or less indefinite future.

In answer to this I would say:—

(1) The "public in high places," by which I suppose is meant Mr. Lowe, who make a conscience of Political Economy, appear to appreciate the fact that the support of an useful and necessary but essentially unremunerative employment like research, out of public money is economically a sound investment; whilst the subsidy of a remunerative employment like teaching, out of public funds, though perhaps unavoidable, is nevertheless, economically speaking, an unsound one. We have no fear of Mr. Lowe's opposition.

(2) If by "the opposition of old prejudices" is intended the attitude of the Conservative party towards the claims of knowledge, I would call Mr. Flower's attention to the fact that some of the warmest supporters of "direct" endowment are political Conservatives. It is, indeed, one of the soundest elements in the Conservative consciousness, the distrust of immature generalisations resting upon insufficient inquiry; and the suspicion that, if we insist too much upon exposition, and throw the weight of our endowments into that, and if we make it every man's duty to be continually expounding, instead of insisting upon research and throwing the weight of our endowments into study, the heads of the rising generation run the risk of being inflated with immature and windy generalisations. Depend upon it, the Conservatives are prepared for keeping the endowments of our colleges for the support of that lifelong and uninterrupted study for which the founders originally intended them.

(3) Thirdly, Mr. Flower desires to wait till the demand for these supports of knowledge is much increased, and the body of scientific men wanting them is larger than it is now. But has he ever asked himself whether it is likely, that when this millennium of expectancy arrives, there will still be any university or college endowments undistributed, out of which this increased demand is to be satisfied? If Reformers of our old Institutions content themselves with sketching merely a teaching organisation on the German model, and with asking to have that amply endowed, and take no thought for the morrow when this larger body of trained investigators shall have come miraculously into existence—and I think this would be a real miracle, the emergence of a set of phenomena for which the conditions do not previously exist—if, I say, they are afraid of asking *now* to have a large fund gradually put in reserve, to be gradually drawn upon as the occasion arises, for the support of study and of those engaged in it—does Mr Flower imagine that the remainder of the College endowments which are not taken up by the teaching establishment upon the German model, will be allowed to lie dead? That no claim will be put in for them by the county towns for the erection of more teaching establish-

ments, or for the support of the lectures to ladies, or as Mr. Walter Morrison desires, for the improvement of the incomes of village schoolmasters?

Assuredly all these claims, and more, will be put in for the residue of the funds—and I think it will be more than half—which will remain unemployed when we have pulled down our old Universities and set up our German teaching establishments in their stead. And shall we be able to offer any resistance to such demands, unless we can come forward *now* with the courage of our opinions, and present the whole of our scheme for a scientific as well as a teaching organisation, the former on a no less complete scale than the latter, instead of keeping half of our scheme, and the more important half of it, in our pockets? Mr. Flower will remember the old lines:—

> "When land is gone and money spent
> Then learning is most excellent."

In conclusion, I would refer for a moment to Mr. Flower's fifth paragraph, in which he seems to say that the interruption of research and study by teaching work or by official duties, is rather an assistance to them. As this statement is very often made, but always without the addition of any reasons for the opinion, I would respectfully ask Mr. Flower to let us know why an interrupted employment is more likely to prosper than a continuous one? what is the precise advantage of distracting intellectual force from the work it has to accomplish? and why the members of the Government, or, say, the jury in the Tichborne case, should not also be compelled to deliver at least one course of lectures during the London season?

July 25 C. E. APPLETON

Method of Endowment

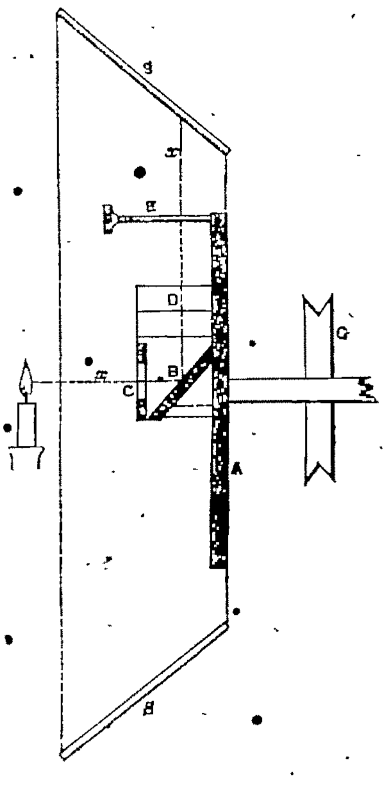
I HAVE read with much interest the three articles which have appeared in NATURE under the above title. The author of these articles has not as yet indicated the manner in which the object which he proposes is, in relation to the Universities, to be attained. He may intend to do this hereafter; but as the absence of any really practical scheme has been mentioned in the public journals as an objection in the way of such endowment as that proposed, I may perhaps be permitted to offer one or two suggestions on the matter. First, it appears certainly desirable that the Fellowships at the Universities should not be abolished, but that the conditions of their tenure should be changed. Scholarships of considerable value, and tenable for a limited number of years, might still be awarded after strict examination; but the Fellowships should be reserved exclusively for the recognition of a capacity for original research, proved by the publication of memoirs, or otherwise. Under such a system there would be little need for an Order of Intellectual Merit. The title of "University Fellow" might well suffice. I have used the expression "University Fellow," for though it would still be desirable that a certain proportion of the Fellows should be required to reside at the several colleges, yet it would probably be considered preferable that the power of election should be transferred from the colleges to a University Council. Such a Council would have to discharge a function similar to that annually performed by the Council of the Royal Society. To prevent favouritism and nepotism, it would be requisite that the names of all candidates should be published, together with the grounds on which each bases his candidature. Similarly the names of the selected candidates should be published, together with the reasons by which the Council have been influenced in their selection. But, it will probably be said, supposing that the Council have in their selection exercised a wise and unbiassed judgment, what is there to prevent the Fellowships from degenerating into mere sinecures? How is the continuance of original research to be secured? Probably there would be, in this respect, little danger in the case of those who have already proved their capacity for original work. But if it be contended that the danger is real, it would not be difficult to provide against it by granting Fellowships, not for life, but for ten or fifteen years, and by renewing them, on the expiry of the original term, only to those who have given strict proof of the continuance of their researches, making exception, of course, in the case of persons disqualified from work either by age or disease.

Such a scheme as that I have suggested would, I venture to think, be both practical and useful, though many matters of detail would still remain to be considered.

July 24 M. A.

Mechanical Combination of Colours

As you have kindly requested me to give a short account in NATURE of the instrument I designed to illustrate the "combination of colours," I have much pleasure in complying with your request. The instrument was designed to show the colours that resulted from the mixture of all or any of the colours of the spectrum given by any light. The construction is as follows:—



To the centre of a disc, A, which can be caused to revolve by the wheel G, a plain mirror, B, is fixed at an angle of 45° to the surface of the disc. In front of the mirror is placed a prism, D. At the edge of the disc there are placed different slides, E, for cutting off any particular rays; also, above the mirror, is a small slit cut in a piece of brass, C, to admit the ray under examination.

x x is a ray of light, which passing through the slit C, is deflected at right angles by the mirror B through the prism D, and is then received in the form of a spectrum upon the screen S S. As soon as the wheel G is set in motion the spectrum also moves round the conical screen S S, and when a certain velocity is arrived at, the colours combine and form the original coloured light which is entering at the slit C. In the same way, by using the slides, any two or more colours may be combined to form the resultant colour.

FREDERICK J. SMITH

On seeing the Red Flames on the Sun's Limb with a Common Telescope

ON observing the partial eclipse of the sun on Dec. 22, 1870, it occurred to me whether it might not be possible to see the red flames on the sun's limb without waiting for a total solar eclipse, or whether it was possible to make an artificial eclipse sufficiently perfect to admit of the red flames being seen. Accordingly I cut out several circular discs of thin brass (blackened on both sides), leaving three arms projecting from the periphery of each of such length that when the ends were bent they should slide into the tube of the eye-piece. I placed one such disc in the eye-tube as near to the field lens as possible to avoid its getting hot; but here a difficulty presented itself which I had not foreseen,—the disc was a trifle too large, and it shut out the sun altogether. I put in a smaller one which admitted too much of the sun's light. I afterwards tried several, and it required a considerable amount of filing and scraping to produce one just the right size to cover the sun's disc and no more; especially as the least jarring or vibration of the telescope would cause the edge of the sun to be seen first on one side and then on the other. After several trials at different times I succeeded on January 16, 1872, in seeing on the south-western limb a red flame. It appeared rather wider at the top than the bottom with a smaller one growing out from the bottom or root close to the sun's limb. There was another tongue of flame a little to the right, which appeared to be detached from the larger flame and also from the sun's limb.

On September 20, 1872, I saw a red flame which went up a little distance from the sun's limb and then divided in three. Close to this, on the edge of the sun's disc, was a group of nine small spots, and a large space was covered with faculæ. The flame—which was of a deep red colour—did not appear to be projected against the sky, but upon a very delicate purple background.

No coloured glass was used in either of these observations, but a sheet of letter paper was held between the eye and the telescope which was removed the instant the sun was brought into the centre of the field of view.

R. LANGDON

The Huemul

IN the number of NATURE for July 24, p. 253, I see it is stated that "the Chilian Exploring Expedition has discovered a specimen of the Huemul, an animal that has been altogether lost sight of."

The late Earl of Derby received a female specimen of this animal from Port Famine, in the Straits of Magellan, described and figured by me in the Proc. Zool. Soc. 1849, p. 64, t. xii., as *Cervus leucotis*, which is now in the Derby Museum at Liverpool. Mr. Bates has sent to the British Museum a male and female of the Huemul, which were obtained by Don Enrique Simpson in a valley of the Cordilleras, lat. 46 S. These specimens have been described, the horns of the male figured, and the history of the animal given in detail by me, under the name of *Huamela leucotis*, in the Ann. and Mag. Nat. Hist. 1872, x. p. 445; 1873, xi. p. 214, and p. 308.

The animal, like all the American deer, differs from the stags of the Old World in having no tarsal gland.

British Museum, July 24

J. E. GRAY

Colour of the Emerald, &c.

IN the valuable and important paper given on this subject in NATURE (July 24), the writer has not made it quite clear what kind of emerald was experimented on.

Taken in conjunction with the beryl, it may be assumed that reference is intended to the green beryl, a silicate of alumina and glucina, commonly called emerald, from its colour; but the name of emerald is also applied to green varieties of corundum, which is crystalline alumina.

It would be interesting to understand fully the distinction of colour constituents.

July 25

A. H.

Parasites of the House Fly

SOME of your readers may not be aware that the common house fly is at this time frequently found with from one to twenty parasites on its body. To such I recommend the observation of them as an interesting microscopical study. They are usually on the under part of the fly and can be seen with an ordinary lens of high power.

A. R.

Regent Street, July 23

Bees and Aphides

IN his interesting communication respecting the relations supposed to exist between *Trigona* and *Membracis*, Dr. H. Muller appears to have overlooked the Abbé Boisier's observation (Kirby and Spence, "Introduction to Entomology," 7th edition, p. 384) that hive-bees will collect the honey-dew excreted by Aphides. I have also observed the same habit in humble-bees.

Kilderry, Co. Donegal

W. E. HART

Flycatcher's Nest

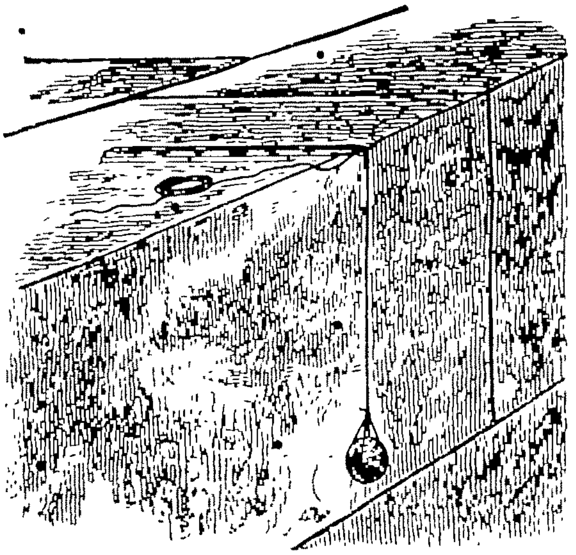
SOME flycatchers have built their nest *inside* a temporary shed erected for the masons at present employed upon the rebuilding of Llanfrechfa Church. The nest is now full of young ones, and the old birds fly in and out of the shed with perfect confidence, carrying food to them, and quite regardless of the carving and sawing going on close to them.

July 16

ELIZABETH H. MITCHELL

Relics of the Pyramids

GLANCING over a number of your periodical I find depicted (vol. vii. p. 147) a grey granite ball, recently discovered in the Great Pyramid, and surmised to be an ancient Egyptian weight It does not seem to have struck the author of the article that this ball could be anything else than a standard weight, but the description he gives leads me to assign to it quite a different use



I believe it to be a naturally formed granite pebble, selected on account of its nearly spherical form, for a mason's "plumb-bob" The small white spots of lime found on the ball were probably the result of its impact against the narrow cement joints whilst the masonry was in progress and the mortar not yet set.

The bronze hook and cedar rod may have formed part of the same tool, which possibly resembled the accompanying sketch.

Mangalore, June 20

E. H. PRINGLE

FISH DISTINGUISHED BY THEIR ACTION

AS the trained eye of a constant resident in the country enables him to recognise the various species of birds that cross his path by their flight, irrespective of their form and colour, so the observer of fish as they wander at will in the tanks of a large aquarium soon learns to invest them with an additional marked individuality imparted by their mode of action. In some instances these distinctive characters are instructive, as illustrating the varied mechanical principles on which locomotion is effected, while in others they are highly valuable as affording accessory means of discriminating the zoological affinities of the different races and species.

Commencing with the Plagiostomous order, we find in the two primary sub-groups, including respectively the Sharks and Rays, that progression is effected on very distinct principles. With the *Selachoidea*, or shark tribe, the fish move by the even, powerful swaying from side to side of the largely developed and unsymmetrical caudal fin and whole posterior part of the body, the other fins remaining quiescent and being merely subservient as balancers. Descending to the species we find again that each form exhibits a peculiarity of action distinct from its congeners, and one which readily enables us to discriminate between them. Thus in the Smooth Hound, *Mustelus*, the pectoral fins are so largely developed that their balancing powers are highly augmented; comparatively slow motion of the caudal extremity suffices to propel the fish through the water, and the whole body being flexible, it progresses with a measured grace of action surpassed by no other species of its tribe. In the Picked Dogfish, *Acanthias*, the general contour of the body is very similar to that of the last species, but the pectorals being much smaller, more rapid action of the caudal extremity is requisite for supporting it in the water, and to this has to be added a great rigidity of the anterior half of the vertebral column, causing the fish to swerve from side to side with each stroke of the tail, the same cause preventing it also from turning corners with ease and rapidity, and altogether imparting to it a want of grace of action compared with that of other members of its tribe. For the foregoing reason this species requires a tank of larger size for its preservation in good health than other Dogfish, as if confined within the boundaries of a small one, it beats its head against the sides and rockwork to such an extent, that the cartilage of the skull is frequently exposed to view. In the Spotted Dogfish, *Scyllium*, the whole body is more elastic even than in *Mustelus*, a character admirably fitting it for its ground-loving habits, and enabling it to explore, and adapt itself to every sinuosity of the ground while hunting for its prey. When swimming in open water, it is distinguished by a more rapid action and swifter progress than *Mustelus*, though at the same time the greater amount of force expended in its movements deprives it of the peculiar grace associated with that species.

One anomalous form standing as it were between the Sharks and Rays, the Monk, or Angel fish, *Rhina squatina*, affords in its locomotive characters an interesting link further indicating its close affinity rather with the former than the latter group. The habits of this fish are essentially nocturnal, and throughout the daytime it usually reclines sluggishly at the bottom of its tank. Its depressed body and broadly expanded pectoral fins, resemble those of a Ray more than a Shark, and like the former fish it seeks concealment by burying itself beneath the sand or shingle, excavating a hole with the shovel-like action of these broad fins, and thus waits in ambush for passing prey. Immediately the Monk fish rises above the surface of the ground, its true affinities become apparent, progression being effected entirely by the lateral action of the caudal extremity, as in the Sharks, though in a more slow and clumsy manner. The lateral position of the gill openings in this fish forms its chief shark-like anatomical character, and to this has to be added its viviparous habits.

In the Batoidea, or Ray tribe, onward motion is accomplished by a singular, even, and wing-like action of the broad pectoral fins, the attenuated caudal extremity remaining perfectly quiescent, and serving only to preserve the fishes' equilibrium. Swimming towards the surface of the water, these fish present a most remarkable bird-like aspect, their large flapping fins reminding the observer of the flight of the heron or some other unwieldy representative of the Grallian order, while the slender tail dependent in the rear suggests the characteristic mode in which those birds hold their long legs, while pursuing their course through the more subtle medium which they inhabit.

Proceeding to the Teleostean group, we find the means by which the same organs are made subservient to the faculty of locomotion, still more highly diversified; space, however, will only admit of a few selections.

In the Gurnards, *Trigla*, during rapid movement, all the fins are pressed closely against the body, the broad wing-like pectorals being shut up like a fan, while the fish is propelled swiftly through the water by the vigorous undulations of the tail; when the fish moves leisurely the pectorals are opened to their full extent, acting as balances. In many species, such as the Striated Gurnard, *T. lineata*, these fins are brilliantly coloured, reminding the observer, especially when regarding them from above, of gorgeous tropical butterflies, gliding along with the smooth action characteristic of the Vanessa tribe. Yet a third property of motion is possessed by these remarkable fish. Settling on the ground at the bottom of the water, they are capable of literally walking over it by means of the three free rays of the pectoral fins, which are situated a little in advance of the others, and are curved and especially thickened, to adapt them for their anomalous office.

The Gemmeous Dragonet, *Callionymus lyra*, a small and beautiful fish somewhat resembling the Gurnards in outward appearance, is distinguished by an essentially different mode of progression. The habits of this species are rather sluggish; it spends much time reclining on the ground, occasionally moving for short distances just above its surface, by the flitting action of the delicate pectoral fins. On ascending towards the top of the water, its swimming capacities are shown to be very limited, being restricted to the weak vibrations of the pair of fins above mentioned, and which impart to it a peculiar jerky action. The male in this species is recognised by the extraordinary length of the first ray of the anterior dorsal fin, which is raised and depressed at pleasure like the latteen sail of a Mediterranean fishing yawl. This singular appendage appears, from my own observations of the species in confinement, to be subservient to the same end as the wattles, crests, and other abnormal adjuncts of the male in the Gallinaceous birds—for the purpose of fascinating their mates; to this is added a similar heightening of the colour, which is carried to such an extent in this fish, that the two sexes were long regarded and described as separate species, under the respective titles of *Callionymus lyra* and *dracunculus*.

In the Pipe-fish and Sea-Horses, *Syngnathus* and *Hippocampus*, representatives of the Lophobranchii, the organs of locomotion are reduced to their minimum, being often restricted, in the former genus, to a single median dorsal fin, and being at the most supplemented by a pair of diminutive pectorals and a rudimentary caudal. In all cases this dorsal fin is the chief propelling instrument, and in motion, rapidly undulating from end to end, illustrates the action of the Archimedian screw, driving the fish through the water on the same principle. Dr. J. E. Gray was the first to point out this remarkable peculiarity, in the case of *Syngnathus*, from observing these fish in the Aquarium at the Zoological Gardens. In both *Syngnathus* and *Hippocampus* the animal usually assumes a vertical position while progressing through the water.

The John Dorée, *Zeus faber*, affords us an example of the same principle noticed in the Syngnathidæ, applied to the purposes of locomotion, though to a still more remarkable and extensive degree.

One of these singular looking fish added to the Brighton tanks about two months since, has continued in perfect health up to the present time; and although of shy and retiring habits, has already yielded many points of interest in connection with its life history. The ordinary position assumed by this fish is the neighbourhood of some projecting rock near the bottom of its tank, and against which it sometimes inclines in a leaning posture, remaining motionless for hours together. Its ordinary progress from place to place is remarkably slow, and it is only when on rare occasions it rises high in the water, that the beautiful mechanism that guides its movements can be appreciated. It may then be seen that the only organs called into action are the narrow and delicate membranes of the posterior dorsal and anal fins, each of which vibrates in a similar manner to the single dorsal of the pipefish; the long filamentous first dorsal, pectorals, ventrals, and caudal fins meanwhile remaining perfectly motionless. Thus this wary fish, with an almost imperceptible action, silently and stealthily advances upon its intended prey, engulphing it in its cavernous mouth almost before the hapless victim is aware of its enemy's approach.

W. SAVILLE KENT

THE ORIGIN OF NERVE FORCE

TO any one taking a general view of the present position of physiology, there are few things more striking than the deficiency of our knowledge respecting the source of the current which traverses the nervous system, and is brought into play through the instrumentality of its various parts. That the current itself is electricity in some form or another, is almost universally acknowledged, but in what part of the body it originates, or from what store of energy it is derived, is more than most have attempted to answer. The question is made more difficult than it would otherwise be, from the fact that in all those animals which exhibit external electrical phenomena to any extent, such as the Torpedo and Gymnotus, there are large and elaborate special organs for the development of the shocks they produce, but no similar mechanicism, and nothing approaching to it, can be detected in man or other animals, whereby an electrical current or charge might originate. The brain and the various ganglia are often compared to the batteries of a system of electric telegraph, but how they would act if they were such, it is almost impossible to explain.

Direct evidence, therefore, failing to give a satisfactory solution of the problem as to whence nerve force originates, it is necessary to appeal to the indirect in endeavouring to obtain an answer. The hypothesis of "the survival of the fittest" evidently presupposes that after the struggle for existence has lasted a certain time, the individuals which remain, economise to the utmost all the forces at their disposal, because the more perfect use that a living being can make of the limited forces at its command, the easier will it be for it to continue to live. The Rev. Samuel Haughton from the resulting very strongly marked economy of the animal mechanicism, has deduced the principle termed by him that of "least action in nature." The generalness of this principle makes it necessary, if there is evidence of the existence of any store of energy in the living body apparently unemployed, to endeavour to find whether its effects have not been overlooked, or included with those of some other force; and if, at the same time, a force is at work whose origin is unknown, to try and prove whether the two are in any way related to one another. As shown above, there is a force which is in continuous action, with an unexplained origin; the question then resolves itself into whether there is a source of energy in the living body, whose effects have not been explained, and if so, can it on any known or probable grounds, be considered competent to give rise to the nerve current? An endeavour will now be made to show that both parts of the question may be answered in the affirmative; in other words, that there is an available source of energy, as yet unrecognised, of which the function is therefore not yet explained, and which is quite capable of giving rise to the nerve current.

This physiologically new source of energy is *the differences of temperature between the interior and surface of the living body*. Those who are unacquainted with the principles of the modern doctrines of thermo-dynamics, will readily perceive that a difference of temperature in two bodies is a source of power, when they consider that a low-pressure steam engine depends, for its power of doing work, on the difference of temperature between its boiler and condenser; and that a current may be maintained through a copper wire, if it is connected with a thermo-electric battery of which the two ends are kept at different temperatures. In what are termed hot-blooded animals, that is, in mammals and birds, the difference of temperature between the surface and the interior is considerable under all natural circumstances, and in them there is a regulating action of the skin, by which they maintain a uniform internal temperature, always hotter than the surface, whatever that of the external medium may be. In the sluggish so-called cold-blooded animals, the temperature of the interior of the body is but slightly different from that of the air or water in which they live; that it must be higher is evident from the fact that destruction of tissue is continually going on in their bodies, which is always necessarily attended with the evolution of heat.

Such being the case, it is evident that in the difference of temperature between the surface and the interior of the living body there is an available source of energy, which is almost certainly employed advantageously throughout the whole animal kingdom; and what is more, it may reasonably be supposed to be that which gives rise to the electrical nerve current, as only one assumption is involved, and that not an improbable one, it being that a thermo-electric current is capable of being generated between soft tissues of different composition or structure. Physicists will be able to decide this question experimentally, and if they do so, they will do a service to physiology.

For the distribution of a current so generated, the construction of the nervous system is perfectly suited. Two sets of conductors are necessary, the one to carry the currents from the skin to the central organ, which arranges the direction that they must take, and the other to send them on to their destination; these are to be found in the afferent and efferent nerves. As in the telegraph system, no return conductor is necessary; for as the ends of the wires are put into connection with the earth, by which they are able to communicate, so the terminations of the nerves in the skin, muscle-corpuscles and otherwise where they lose their insulated coverings, place the extremities of the afferent and efferent nerves in communication through the intervention of the mass of body tissue. The brain and minor ganglia would then act like greater and lesser offices for the reception and transmission of currents in the required directions, being in fact the commutators of the system.

There are several of the most important phenomena exhibited by the nervous system which are very satisfactorily explained on the above hypothesis. For instance, in cold weather the impulse to action is much more powerfully felt, than in summer when the air is hot, and therefore the temperature of the surface is higher. It is well known that it is impossible to remain for more than a very short time in a hot water-bath, of which the temperature is as high as, or a little higher than, that of the body, on account of the faintness which is sure to come on, and this may be reasonably supposed to be the result of the cessation of the nerve current, which is consequent on the temperature of the surface of the body becoming the same as that of the interior. This faintness is immediately recovered from by the application of a cold douche. When great muscular exertion has to be sustained, as in running or rowing, it is always necessary to have the clothes very thin, and it is felt during the time that it is necessary for the continuance of the effort, that the surface of the body must be kept cool.

As the termination of the nerves in the skin must correspond, on this hypothesis, with the cooled end of a thermo-electric battery, therefore the brain, which is very abundantly supplied with blood, and is the part of the body to which most of the nerves are directed, must be compared with the heated end; and as it is by the conversion of heat into electric current that the nerve force is developed, it is evident that heat must, to a certain extent, disappear as such in the brain, and that that organ must consequently be colder than the blood which enters it. This is exactly what Dr. John Davy observed in the case of the rabbits he experimented on, and his results have not been shown to be incorrect.

A paper on this subject by the present writer appeared in the June number of the *Journal of Anatomy and Physiology*.

A. H. GARROD

NOTES FROM THE "CHALLENGER"

V.

ON Wednesday, March 26, we sounded (Station 25) in lat. 19° 41′ N., long. 65° 7′ W., nearly 90 miles north of St. Thomas, in 3,875 fathoms. The bottom brought up in the hydra tube was reddish mud, containing, however, a considerable quantity of carbonate of lime. It is singular that the colour and composition of this mud were not uniform. The upper layer, that which had been forced farthest into the tube, was much redder than that which was nearest the mouth of the tube, and which had consequently come from a greater depth. I am inclined to attribute this to the steepness of the slope from the plateau of the Virgin Islands. It is easy to conceive that, under the influence of currents varying from time to time in force and direction, the calcareous mud, the product of the disintegration of the coral reefs, may be washed down the incline in varying proportions.

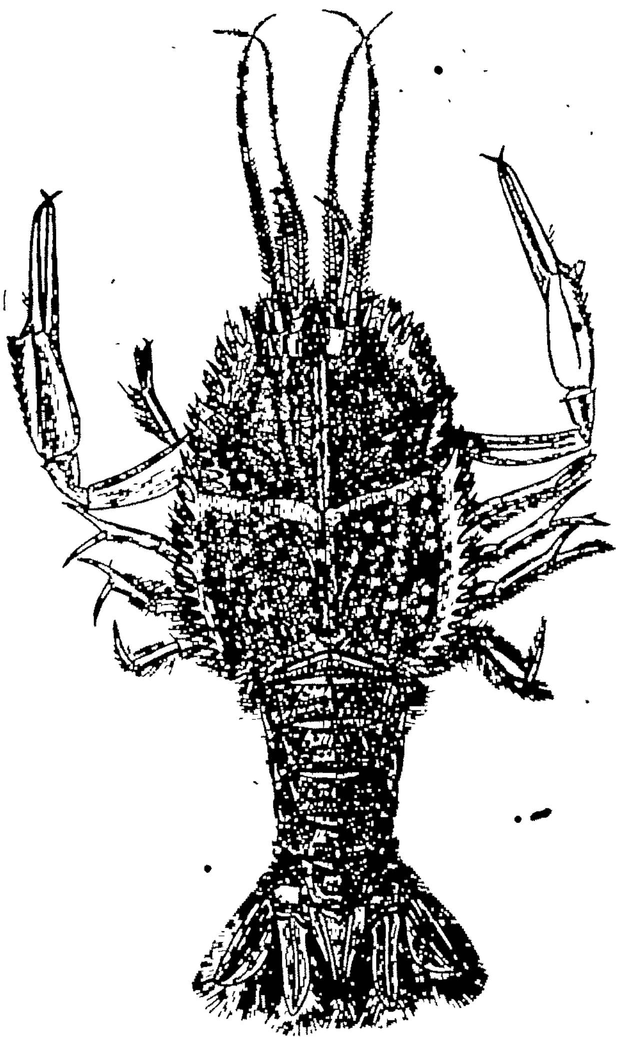


Fig. 1.—Deidamia crucifer, v. W.-S.

Two thermometers were sent down in this sounding, and a slip water-bottle. The thermometers were unable to bear the extreme pressure, and both were broken. I have already (vol. viii. p. 109) in a former report described the circumstances connected with the loss of these two instruments. The water bottle appeared to have answered its purpose. Mr. Buchanan finds that the bottom water has a specific gravity slightly greater than usual at great depths, but not materially so. The amount of carbonic acid is somewhat in excess.

As this was the deepest sounding which we had taken, we were anxious to try whether the dredge would still prove serviceable. The small dredge was accordingly lowered with the usual bar and tangles, and from the centre of the bar a "hydra" sounding tube weighted with 4 cwt. was suspended about two fathoms behind the dredge. A two-inch rope was veered to 4,400 fathoms; a toggle was stopped on the rope 500 fathoms from the dredge, and when the dredge was well down two weights of one cwt. each were slipped down the rope to the toggle. We commenced heaving in about 1.30, and at 5 P.M. the dredge appeared, with a considerable quantity of reddish-grey ooze, mottled like the contents of the sounding-tube. The whiter portion effervesced freely with acids, the redder only slightly. The mud was carefully examined, but no animals were detected except a few small foraminifera, with calcareous tests, and some considerably larger of the arenaceous type. This dredging, therefore, only confirmed our previous conviction, that very extreme depths, while not inconsistent with the existence of animal life, are not favourable to its development. In the afternoon a series of temperatures were taken at intervals of 100 fathoms from the surface to 1,500. The temperature at the surface was 24°·5 C., and that at 1,500 2°·4 C. The curve constructed from this series indicates a very rapid and uniform fall of about 20 C. during the first 600 fathoms, and generally a distribution of temperature almost identical with that of some of the later stations on the section from Santa Cruz to Sombrero. In this way we pursued our course northwards under all plain sail.

On the following day we sounded in much shallower water—2,800 fathoms. The bottom was much of the same character, and on the 28th in 2,960 fathoms with a like result, but at our next sounding in 2,850 fathoms on the 29th, the calcareous element in the mud had almost entirely disappeared, and the contents of the tube seemed to be identical with the "red clay" which occupied so large a portion of our first section. The occurrence of this clay is a large and important phenomenon. In the section of the Atlantic, from the Canaries to the West Indies, it occupies about 1,900 miles, a distance twice as great as that occupied by the globigerina mud. What its lateral extension from that line may be, we do not know; but we now find that it extends more or less from over the greater part of the distance between St. Thomas and Bermudas. The nature and source of this deposit, and the causes of its peculiar distribution in the deeper parts of the ocean, are therefore questions of the highest interest.

On the 2nd of April, at a distance of 134 miles from Bermudas, a series of temperature soundings was taken at intervals of 20 fathoms from the surface to 300 fathoms.

The pilot came on board in the afternoon of April 4 and we passed through the narrows, the reefs which make the navigation of this singular little group of islands so dangerous spreading round us in rich purple patches, contrasting with the vivid pale green of the channels of deeper water between them.

The evening was falling as we anchored in Grassy Bay and received our first impressions of Bermudas. On the Monday following we moved from Grassy Bay to the Camber, in the great Dockyard. We remained there till the 21st of April, and employed the interval in taking such a general survey of the natural beauty of the island as our time allowed.

As Bermudas, on account of its isolated position, its structure, and its peculiar conditions of temperature, presents many points of great interest, I will defer giving a detailed account of it until some investigations which we have still in hand are completed.

We met at Bermudas with a singular confirmation and illustration of our view as to the organic origin of the "red clay" of the Atlantic sea-bed.

The Islands of Bermudas consist exclusively of limestone, in some places very compact and hard, almost crystalline; more usually soft and crumbling easily when first quarried, but hardening on exposure to the air. The limestone is very irregular in the direction of its dip. In amount, however, the dip seems never to exceed 30°. The beds are thrown about in a curious way, every quarry or road-cutting showing contortions of all kinds in the strata and every amount of irregularity consistent with uniformly low angle of dip. One would imagine at first sight that the islands exhibited, on a small scale, an epitome of the geological phenomena of a disturbed palæozoic district.

Lieut. (now General) Nelson, R.E., at that time a young man, stationed at Bermudas, communicated to the Geological Society of London on April 23, 1834, a very valuable paper on the geology of Bermudas, which was published in the fifth volume of the Transactions of the Society. Lieut. Nelson pointed out that the great proportion if not the whole of the Rocks of Bermudas are formed simply by the blowing up by the wind of the fine calcareous sand the product of the disintegration of the coral, shells, serpula-tubes, and the other constituents of the Bermudas reefs, that white sand which we found to extend at varying depths through a radius of about 20 miles round the island. The sand is washed in by the sea; it is then caught at certain exposed points by the prevailing winds, blown into sand-hills 40 to 50 ft. in height, which slowly move along, forming shoreward a glacis at the angle of repose of loose sand, on which lamina after lamina is deposited, overwhelming a large tract of country with its fields, gardens, and cottages, in a comparatively short time, and advancing until its progress is stopped by an opposing slope of sufficient height, or by the binding of the sand by vegetation. On these wind-blown beds of lime, aptly called by Lieut. Nelson, Æolian formations, which are originally formed at a considerable inclination, changes in the direction and force of the wind-floods of sub-tropical rain and other transitory and accidental causes produce with great rapidity all the appearances, denudation, unconformability, curving, folding, synclinal and anticlinal axes, &c., which are produced in real rocks, if I may use the expression, by combined aqueous and metamorphic action, extending over incalculable periods of time.

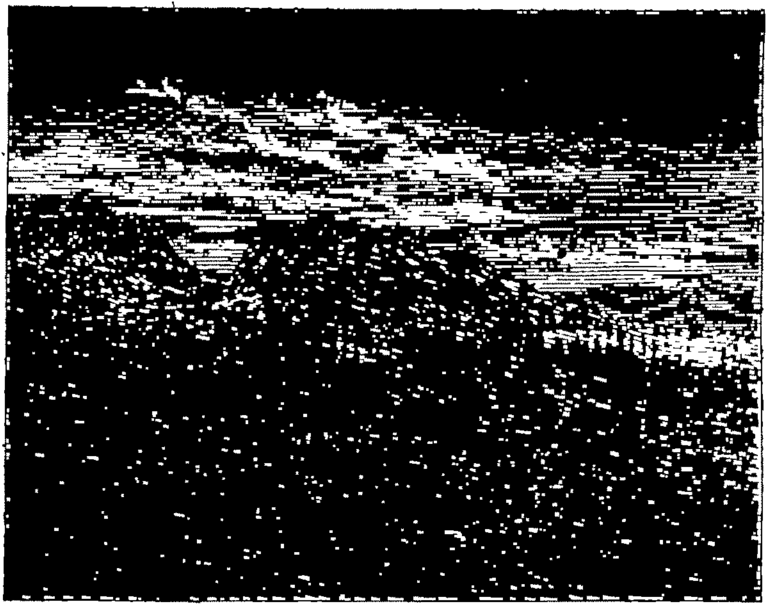


FIG. 2.—Rocks of Coral Sand in Bermudas in process of formation, showing Stratification, and the Stumps of Cedars which have been overwhelmed.

Rain-water contains a considerable quantity of free carbonic acid. Water thus charged dissolves the lime rapidly, and the solution of bicarbonate of lime percolating through the bed, loses a portion of its carbonic acid, and deposits a cement of carbonate of lime between the particles of the coral sand. This process is kept up not only by the surface rain but by the water of the sea, which, as we shall see, percolates through the porous stones of the islands. As evidence of the universality of this process, we have every crack and fissure of the rock filled with semi-crystalline stalagmite, and every here and there the rock is hollowed out into caves which in some places assume the proportions of magnificent caverns with lofty roofs, supported by huge stalagmitic columns, and fretted and enriched by curtains and fringes of stalactite.

One very striking thing about Bermudas is the total absence of running water. There is not a trace of a stream or pool, or even of a ditch. The rain, which often falls in great quantities, sinks through the soil at the spot where it falls as it might sink through a sieve. The islands are perfectly permeable to water horizontally as well as vertically, so that below the level of the sea the stone is saturated, or filled with salt water. The *fresh* water lakes and wells, of which there are many, are thus merely catches of fresh water lying upon the surface of salt water, and they are nearly all slightly brackish, and those near the sea rise and fall perceptibly with the tide.

WYVILLE THOMSON

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE

I.

DURING the last few years public attention has been frequently drawn to the subject of our national weights and measures. The administrative and social questions of the improvement of our existing system, and of the proposed introduction into this country of the decimal metric system—first established in France, and now being generally adopted on the Continent of Europe, and indeed extending to the other quarters of the world—have formed the subjects of debate in every Session of Parliament, and are still awaiting solution. The scientific questions involved in the use of weights and measures have for a much longer period engaged the attention of many of our most eminent men of Science, several of whom have been members of the various Standards Commissions from time to time appointed by the British Government. These questions are also at the present time the objects of investigation and deliberation by the large body of scientific men from all civilised countries, who compose the International Metric Commission at Paris. It may, therefore, be useful to bring together and place before the public the several points involved in the science of weighing and measuring, and to give some account of our standards of weight and measure, as well as to describe in some detail the scientific construction of our existing imperial standard yard, and pound. No sufficient means have hitherto been adopted for making the general public acquainted with this part of the subject, although they are directly interested in it, the information hitherto published respecting it having been confined to a few papers in the "Philosophical Transactions," and to reports of the several Standards Commissions, and other Parliamentary Returns. Of these papers the most important are those on the construction of the imperial standard pound, by Prof. W. H. Miller, in "Phil. Trans.," 1856, and on the construction of the new imperial standard of length, by the Astronomer Royal, now Sir G. B. Airy, K.C.B., in 1857. In the following treatment of the subject use will be made of these papers, as well as of other authoritative works relating to weights and measures.

The science of weighing and measuring comprehends the following points:—

The scientific definition of weight and measure.

The authoritative establishment of fundamental units of weight and measure of length and the construction of their material representatives as primary standards, in relation to which all numerical amounts of weight and measure are to be expressed.

The establishment of determinate aliquot parts and multiples of the primary units of weight and measure, and of other units derived from them, such as the unit of measure of capacity, &c.; and the construction and verification of their material representatives, as secondary standards, by comparison with which the accuracy of all weights and measures in ordinary use is to be determined.

The scientific methods of using standard and other weights and measures in which special accuracy is required, as well as auxiliary scientific instruments, such as balances, thermometers, barometers, micrometers, and other comparing apparatus.

The determination of the just results of weighing and measuring with these scientific instruments, after allowing for all indirect influences affecting the accuracy of the direct results of weighing and measuring; for instance, differences arising from the physical composition of bodies, variations of temperature and consequently of the expansion or contraction of the several substances, changes of condition in the medium in which the comparisons are made, &c., including also a computation of the probable errors of the final results.

The whole subject will therefore be treated under the following general heads:—

I. Definitions of weight and measure.
II. Standards of imperial weight and measure.
III. Scale of multiples and parts of imperial standard units.
IV. The metric system.
V. Weighing and measuring instruments and their use.

I. *Definitions of Weight and Measure.*

Weight or *gravity* has been defined as the quality in physical bodies by which they tend towards the centre of the earth, in a line perpendicular to its surface; or it may be defined more generally as a property inherent in all bodies, by which they are drawn to some common point, called the centre of gravity, and with a velocity in proportion as they are more or less dense, and as the medium through which they pass is more or less rare.

In following out his discovery of the theory of universal attraction and gravitation, Sir Isaac Newton demonstrated, first, that the weights of all bodies at equal distances from the centre of the earth are directly proportional to the quantity of matter that each body contains; whence it follows that the weights of bodies have no dependence on their shapes or textures, and that all spaces are not equally full of matter. Up to the time of Newton the earth was considered to be spherical, but it was demonstrated theoretically by Newton, as well as by Huygens, that the earth must be flattened at the poles. Whence it was shown by Newton, secondly, that on different parts of the earth-surface, the weight of the same body is different, owing to the spheroidal figure of the earth, which causes the body on the surface to be nearer to the centre in going from the Equator towards the Poles; and that the increase of the weight is nearly in proportion to the versed sine of double the latitude, or, which is the same thing, to the square of the sine of the latitude. He assumed the weight at the Equator to that at the Pole to be in the proportion of 229 to 230, and consequently the whole increase of weight from the Equator to the Pole to be the 229th part of the weight at the Equator.

In accordance with the principle the discovery of which is ascribed to Archimedes, that all bodies immersed in a liquid suffer a loss of weight precisely equal to the weight of the liquid displaced, it was also demonstrated that a body immersed in any fluid specifically lighter than itself loses so much of its weight as is equal to the weight of a quantity of the fluid of the same bulk with itself. Hence a body loses more of its weight in a heavier fluid than in a lighter one, and therefore it weighs more in a lighter fluid than in a heavier one, for instance, more in air than in water.

The foregoing principles laid down by Newton are universally admitted as correct, with the exception of the numerical proportions of the weight of bodies at different parts of the earth's surface; for it is important to observe that Newton founded his calculation of the earth's ellipticity on the hypothesis of its being homogeneous, which is not the case; and hence he makes the equatorial diameter greater than the polar axis, as 230 is to 229. But from the numerous experiments since made with the pendulum at different parts of the earth, it has been found that the earth is not homogeneous, or composed of concentric strata of equal density, and that the ellipticity is not so great as Newton supposed.

The method of measuring the intensity of gravity on different parts of the terrestrial spheroid, by means of the seconds pendulum, is said to be due to Borda, as originally described in a Memoir inserted in vol. iii. of the *Base du Système Métrique*. From the results of Borda's experiments, made towards the close of the last century, Laplace computed the ellipticity of the earth to be $\frac{1}{330}$, but later experiments and computations of other men of science concur in making it nearly $\frac{1}{300}$.

In the Philosophical Transactions of the Royal Society for 1818, Capt. Kater has stated the results of his pendulum experiments in London, and determined the length of the pendulum vibrating seconds, or completing one vibration in $\frac{1}{86400}$ part of a mean solar day, when measured in a vacuum at the mean level of the sea and at a temperature of 62° Fahr. to be 39·13842 inches of the Standard yard, which was legalised in 1824 as the Parliamentary Standard of length. The latitude of his place of observation in London was 51° 31′ 4″ N. He subsequently made a slight correction in this determination, making the length of the seconds pendulum to be 39·13929 inches, as shown in Phil. Trans. 1819, and this length, or rather 39·1393 inches, was declared to be the true length in the Standards Act of 1824.

It was, however, discovered by Bessel that the correction which had ordinarily been applied, and was applied by Kater, for reducing the vibrations of a pendulum, as observed in ordinary air, to vibrations in a vacuum, ought to be greatly increased. The experiments were consequently repeated by Capt. (now Sir Edward) Sabine, with special reference to the form of pendulum usually employed in England. In Phil. Trans. 1821, Sir Edward Sabine has shown as the results of his experiments on the acceleration of the pendulum in different latitudes, that the mean diminution of the force of gravity from the pole to the equator was 0·0055138, in other words, that a weight of 100 lbs. at the equator would be less by 0·55138 lb. at the pole; whilst the resulting mean ellipticity of the earth deduced from his pendulum observations, was $\frac{1}{313·6}$. Sir Edward Sabine has also shown as the result of his experiments on the length of the seconds pendulum in Greenwich Observatory, that its length vibrating 86,400 seconds in the 24 hours, at the temperature of 62° F., and in a vacuum, was found to be 39·13734 inches.

In his paper on the Yard, the Pendulum, and the Metre, Sir J. Herschel has observed that the true measure of the earth's attraction (independent of centrifugal force arising from its rotation), is best to be derived from an ideal seconds-pendulum supposed to vibrate at the extremity of the earth's polar axis; and that the mean length of the polar or of the equatorial pendulum must be derived from the general result of observations of the lines of oscillation of one and the same invariable pendulum at a multitude of geographical stations in all accessible latitudes in both hemispheres; but that no two combinations agree in giving the same precise length, in consequence of the local deviations of the intensity of gravity, due to the nature of the soil or crust of the earth, and the configuration of the ground immediately beneath and around the places of observation. And further, that since the pendulum cannot be observed at sea, the whole sea-covered surface of the globe is of necessity excluded from furnishing its quota of observations to the final or mean conclusion. Water being on the average not more than one-third the weight of an equal bulk of land, such as the earth surface consists of, and only $\frac{4}{11}$ of the mean density of the globe, the force of gravity at the surface of the sea is less than at the sea-level on land by the attractive force of as much material taken at twice the specific gravity of water (or at $\frac{4}{11}$ that of the globe), as would be required to raise the bottom to the surface.

With regard to the determination of the earth's ellipticity, as shown by actual measurements of the dimensions of our globe, and the relative length of the equatorial diameter and the polar axis of the earth, the most recent determination is that by Major Clarke, as stated in his "Comparison of Standards of Length," published in 1866. This memoir has been declared by Sir J. F. M. Herschel, to be the most complete and comprehensive discussion yet received on the subject of the earth's figure, and to be held as the ultimatum of what scientific calculation is as yet enabled to exhibit as to its true dimensions and form.

Major Clarke's results were computed, not from pendulum experiments, but from the combination of all the separate measurements of arcs of meridians in Peru, France, Prussia, Russia, Cape of Good Hope, India, and in the United Kingdom. They are as follows:—

	Feet.	Inches.	Metres.	Metres according to Capt. Kater's equivalent.
Length of Polar axis	41,706,858	500,482,296	12,712,136	12,712,020
Longer equatorial axis (long. 15° 34′ E.)	41,853,700	502,244,400	12,756,588	12,756,470
Shorter equatorial axis (long. 105° 34′ E.)	41,839,958	502,079,496	12,752,701	12,752,588
Length of meridian quadrant of Paris	32,813,524	393,762,292	10,001,472	10,001,381
Length of minimum quadrant (long. 105° 34′ E.)	32,808,772	393,704,064	10,000,024	9,999,953

In computing these equivalents, Major Clarke takes the metre at the temperature of 32° F. from his own measurements to be equal to 1·09362311 yard at 62°, that is to say to 3·28086933 ft., or to 39·37043196 in., instead of the more generally received determination by Capt. Kater of 39·37079 in. The metric length according to both these equivalents is here given.

From the determination of the earth's dimensions, it may be easily computed, that the earth's ellipticity in the longitude of Paris, is $\frac{1}{300}$, whilst its mean ellipticity in all longitudes is $\frac{1}{298}$.

Hence also the mean length of a degree of latitude in the longitude of Paris is $\frac{32,813\,524\,38}{90} = 364,591$ ft., or 69·05 miles. The mean diameter of the earth is 41,800,173 ft., or 7216⅔ miles, and its mean circumference 23,871 metres.

Thus not only each longitudinal meridian, but also the equator is slightly elliptical.

Sir H. James has shown in his preface to Major Clarke's paper that the longest meridian in 15° 34′ east longitude, nearly corresponds to the meridian in the eastern hemisphere which passes over the greatest quantity of land; and in the western hemisphere to that which passes over the greatest quantity of water, as it passes through the centre of the Pacific Ocean. The shortest meridian in 105° 34′ east longitude nearly corresponds to that which passes over the greatest quantity of land in Asia; and in the western hemisphere, and that which passes over the greatest quantity of land of North and South America.

The connection here shown to exist between the definition of weight and the measurement of the dimensions of our globe, leads naturally to the definition of the second principal head of the subject, viz. of measure.

Measure is generally understood to mean the determinations of a body with relation to a fixed standard unit, or the measure of extension; and it is in this sense that it will now be taken in discussing the "science of measuring."

The measure of extension comprehends

- The measure of length, or linear extension;
- The measure of surface, or square measure;
- The measure of volume, or solid or cubic measure;
- The measure of capacity, or the cubical quantity contained in any vessel for measuring dry goods, liquids, or aëriform fluids.

All these measures of extension are based upon one fixed standard unit of length; and as all measures of length vary according to their temperature from expansion or contraction, the length of the standard must be fixed at a normal temperature.

Strictly speaking, measure includes weight, which is the measure of the gravitation of bodies towards the centre of gravity. And measures of capacity also are almost universally derived, not from their cubical dimensions, but from the weight of pure water contained in them under determinate conditions as to temperature and atmospheric pressure.

The measure of temperature is based upon the observed rate of linear expansion by heat of a body selected for this purpose, generally mercury, taking as constant units the temperature of melting snow or ice, and of water boiling under determinate atmospheric pressure.

In defining measure, it should be added that it is also applied to the measure, or (as it is termed) *admeasurement* of the tonnage of ships, being a determination of the weight a ship is capable of carrying, with relation to its measure of cubic capacity; to value in relation to a monetary unit; to time and duration in relation to the unit of a mean solar day or a second, its 86,400th part; to velocity, by combining the measure of extension with that of time or duration; to mechanical work, the unit of which is a horse power, as it is commonly termed, or more properly the power of raising 33,000 lbs. one foot in one minute, thus combining the measures of linear extension, weight, and time; to angles, the unit being a degree or the 360th part of a circle described from the point of junction of the two straight diverging lines forming the angle; &c. &c. It is not, however, proposed here to refer further to these measures or to the scientific questions connected with them.

The measure of volume, or bulk of a body, as compared with that of another body differing in volume but equal in weight, is shown by its density, and is also expressed in terms of a fixed standard unit. The densities of bodies are in the direct ratios of their masses, or quantity of matter, and in the inverse ratios of their volume.

The density of a body is defined to be the mass contained in a unit of volume, when referred to a uniform standard. The specific density is to be distinguished from its specific gravity, which shows its weight in relation to its volume, also when referred to a uniform standard. The specific gravity of a body is defined to be the *weight* of a unit of its volume.

The specific gravity of a body is the quotient of its density when divided by the density of that substance which is considered as unity. Pure water is generally adopted as such unity. But since both these densities vary with the temperature—because the same invariable quantity of matter which the body contains is always distributed over its whole volume, and this is variable with the temperature; so that, generally speaking (with some exceptions, pure water, for instance, at certain temperatures), the body, at a higher temperature, has less density than at a lower temperarure—we must fix a certain temperature at which the body, as well as the water, must be considered. It is not necessary that this fixed temperature should be the same for the body and the water, its choice for both being quite arbitrary.

For bodies the most convenient standard temperature for expressing their density seems to be that of one of the fixed points of the thermometer; and the temperature of melting ice or snow (32° F. or 0° C.) is generally adopted. For pure water, there is a maximum of density which occurs at nearly 39° F. or 4° C., and this maximum density of pure water is generally adopted as the unit of density.

The sign $\triangle$ prefixed to the symbol of any weight, with its numerical value following, denotes the ratio of the density of the weight at the temperature of melting snow to the maximum density of pure water.

The relation of the bulk or volume of a body to its weight is expressed both by its density and its specific gravity, these terms being often used indiscriminately. But the former term is more strictly applicable to solid bodies, and the latter to liquids and gases.

To ascertain the density of a body, it is requisite that its volume should be determined, as the density cannot be directly found. The actual volume may be determined—

1. Either by cubic measurement, when the form of the body admits of this measurement being actually made; but this occurs but rarely.

2. Or by ascertaining its specific gravity, from determining the difference of its weight when weighed in air and in water. This is the readiest and most accurate mode of determining both its volume and its density, but the immersion of a body in water is not always practicable, or it may be injurious to the body under experiment.

H. W. CHISHOLM

(*To be continued.*)

NOTES

AT the Meeting of the Paris Academy of Sciences, M. Ferdinand de Lesseps was elected an "Academicien libre" in the place of M. de Verneuil, deceased. M. de Lesseps obtained 33 votes; M. Breguet, 24; MM. du Moncel, Jacquemin, and Sedillot 1 each. M. de Lesseps thus obtained 2 votes beyond the absolute majority required to render an election valid, and was therefore declared elected. The number voting, 60, was large.

THE forty-first Annual Meeting of the British Medical Association will be held in King's College, London, on Tuesday, Wednesday, Thursday, and Friday, August 5th, 6th, 7th, and 8th. The President-elect is Sir William Fergusson, Bart., F.R.S. The following are the six sections into which the meeting will be divided, and in each section a very large number of papers is already entered to be read:—Section A, Medicine; B, Surgery; C, Obstetric Medicine; D, Public Medicine; E, Psychology; F, Physiology. The sections will meet in rooms of the College appropriated for the purpose, and the Annual Museum of objects of interest in connection with medicine, surgery, and their allied sciences will be arranged in the Library of the College. The President's address will be delivered at 3 P.M. on August 5, and in the evening the Lord Mayor will hold a reception at the Mansion House. The following public addresses will be delivered:—On August 6, an address on Medicine, by Prof. E. A. Parkes, M.D., F.R.S.; on August 7, an address on Surgery, by Prof. John Wood, F.R.S.; and on August 8, an address on Physiology, by Prof. Burdon Sanderson, F.R.S. The President and Council of the Royal College of Surgeons hold a reception on the evening of August 6, and several excursions have been arranged to take place during the meeting. Altogether, to judge from the programme, the meeting promises to be a very successful one.

THE Royal Archæological Institute commenced its annual session at Exeter, on Tuesday, when the Mayor and Corporation held a reception at noon. The President, the Earl of Devon, thereafter delivered his inaugural address on the advantages of the study of Archæology, and in the afternoon an excursion took place to Rougemont Castle. In the evening, again, the Mayor held a reception in the Albert Museum. The Sectional Meetings commenced yesterday, and several interesting excursions have been arranged. The Sections are, Antiquities, Architecture, and History. One of the most attractive accompaniments of the Exeter meeting is the formation of a temporary Museum and Portrait Gallery.

THE French Association for the Advancement of Science commences its second session at Lyons on August 21, the concluding meeting to be held on August 28. As was the case at Bordeaux, there will be General Meetings, Meetings of Sections or Groups, Scientific Excursions, and Public Lectures. A large number of papers has already been entered to be read at the Sectional Meetings, by well-known scientific men, and several interesting excursions have been planned, including one to the famous pre-historic station at Solutré. So far, this year's meeting of the Association promises to be very successful. Immediately after the session of the Association is concluded, the Geological Society of France holds its annual meeting at Roanne.

DR. GOPPERT, of Breslau, the veteran writer on the subject of fossil plants, is desirous of disposing of his immense collection, in securing which he has spent more than thirty years, and made it perhaps the finest in the world, embracing, as it does, type specimens of 94 different works and 400 minor essays, represented on about 1,000 plates. The number of specimens exceeds 11,000, and includes Sigillaria from sixteen to twenty feet in length, and other specimens of equal magnitude. There are also 200 specimens of different kinds of amber with their inclosed plants, and also a series of diamonds, with various objects included in them. In addition to the fossil objects there is also a very large collection of recent plants, which serves to illustrate the first-mentioned series, such as palms, tree-ferns, cycades, bamboo, algæ, sections of wood, fruits, seeds, &c. Numerous original drawings also accompany the collection, which add much to its value.

MR. SMITH gives some very interesting details in the *Daily Telegraph* of his excavations at Nimrod. We think, however, the main result of his expedition is to shew the necessity of a more thorough and longer continued exploration of the ruins of Assyria than Mr. Smith has been able to give; and the sooner such an exploration is undertaken, the more fruitful are the results likely to be.

THE *New York Herald* of the 17th inst. publishes a letter from Dr. Petermann, the eminent German geographer, to Dr. Stramecky, the Secretary of the American Geographical Society. In it he says:—As at the departure of the expedition much stress was laid on its prospect of reaching the North Pole, the public at large, which has no idea of the difficulties surrounding the solution of geographical problems, might look upon it as a complete failure. It should not be made a reproach to Captain Hall that he held out such a prospect, for without it he would not probably have obtained either ship or money, or any other support. Placed in a similar condition, the same thing has happened to me and my friends in Germany, and it will always remain thus as long as the civilised Governments of the world devote their millions principally to the increase of their armies, and the scientific objects only figure in the Budget for the crumbs, and as long as people who are willing to add to the little knowledge we have of our own earth have to go begging for small contributions. To me the geographical results of the expedition appear of an extraordinary value At any rate they are the highest that any vessel among the numerous expeditions of all nations to the North and South Poles have ever accomplished for many centuries. I shall speak of the subject at greater length in my next Arctic report (No. 80).

AT the commencement of 1874, says the *Deutsche Zeitung*, one or two ships of the German navy are to be sent on a scientific mission to observe the transit of Venus. These vessels will have to submit their observations, which are to be extended to ocean currents and tides, to the hydrographic office of the German Admiralty.

THE first three numbers of a work on indigenous and exotic Lepidoptera have been issued by Mr. Hermann Strecker, of Reading, Pennsylvania, U.S. the object of the author being principally to bring to the cognizance of the public the many new species from all parts of the world embraced in his very extensive cabinet. While the preference will be given to those from North America, he, unlike Mr. William H. Edwards, includes some species from other countries. The illustrations, which occupy one plate for each number, are all drawn, printed, and coloured by Mr. Strecker himself in the intervals of his daily labours, and the whole work is extremely creditable to him. The work is in quarto, and it is proposed to publish one number every two months, each with a single plate, crowded as fully as possible with figures. The enterprise is well worthy of commendation, and persons desirous of obtaining the work can do so by addressing Mr. Strecker, as above. A few copies only are printed, and the drawings then erased to make way for a new set.

PROF. MEEK announces the existence of primordial species among the fossils collected by Dr. Hayden, in 1872, from near Gallatin City, Montana, U.S.—a very important geological fact. He has also found carboniferous fossils in various localities. Some of these are from the "divide" between Ross's Fork and Lincoln Valley, Montana, embracing many of the same species as occur in the noted Spurgen Hill locality, in Indiana, of the age of the St. Louis limestone.

AT noon of July 8 Prof. Agassiz formally opened the Anderson School of Natural History on Penikese Island, thus bringing to a practical beginning the great idea of a summer school of natural science as first suggested by Prof. Shaler. Our readers are sufficiently familiar with the details of the circumstances which led to the establishment of this magnificent educational enterprise—first, the donation by Mr. John Anderson, of New York, of Penikese Island, one of the Elizabeth group, situated at the entrance of Buzzard's Bay, and valued at 100,000 dollars; then his endowment of it in the sum of 50,000 dollars to meet the current expenses; and subsequently the presentation to the professor by Mr. Galloupe, of Swampscot, of a yacht worth 20,000 dollars, for use in deep-sea dredgings and other explorations in connection with the school. In a circular Prof. Agassiz gives notice to the public that the island affords no accommodation to strangers, and that no guests can be received excepting those who have been accepted as members of the school. The limit of fifty has long since been made up, one-third of them being ladies, while more than a hundred have been rejected in consequence of the limitation. A caterer has been engaged, who will provide for the table, and keep the rooms in order. There is to be no charge whatever for tuition, and as the dormitories have been built at the expense of the fund, no rent will be charged beyond a percentage of the value of the bed-room furniture. The board is to be charged at cost. Should any persons desire to make collections of specimens to carry away with them, cans and alcohol will be furnished at cost to those who are not already provided.

THE Russian astronomers have decided upon occupying twenty-four stations on the important occasion of observing the Transit of Venus. It is found that the weather will probably be highly favourable to astronomical observation at all the stations in Siberia and on the Pacific coast, as there is an average of only three cloudy days in the month of December in these parts of the Russian possessions. The extreme cold of November is well regarded as an almost insuperable hinderance to the proposed work. The following very complete outfit has been ordered for use on this occasion, viz., three heliometers and three photoheliographs, for use in measuring the position of the planet on its passage across the sun's disc; ten equatorials, for observing the apparent contacts of the limbs of the planet and sun by the use of the spectroscopic method, and for the determination of the same moments by observations with the filar micrometer; ten telescopes, for simply observing the instant of each contact; and besides these, there is for each station a complete outfit of clocks, chronometers, and instruments for determining the local time. The observers are all to practise beforehand at the Imperial Central Observatory at Pultowa. The geographical positions of those stations at which the observations result successfully will be afterwards determined by a special geographical expedition by the Russian navy. To perfect this portion of the work, a line of telegraph will be built through Siberia to Nicolaevsk.

WE have received the programme for Session 1873-74 of the University of Durham College of Physical Science, at Newcastle-on-Tyne. It contains ample information as to the amount and kind of instruction to be obtained at the Newcastle College, and full details as to the arrangements, fees, examinations, exhibitions, and scholarships. There are three exhibitions of 15*l.* each to be awarded after examination in October, one scholarship, the T. Y. Hall scholarship, of 20*l.* yearly value, tenable for three years, and two scholarships offered by Mr. Hugh Taylor, consisting of the expense for maintenance and education at the Newcastle College, for two years: those last are for sons of overmen, deputies, or pitmen, who are engaged in coal mines in the counties of Northumberland or Durham, and are between sixteen and eighteen years of age. So far as it goes, the Newcastle College seems to furnish a thorough training in scientific knowledge and method.

WE have received from Mr. F. Abbott a paper read before the Royal Society of Tasmania, giving the result of his recent observations at the Private Observatory, Hobart Town, Tasmania, of η Argus. He thus summarises the results of his most recent observations. In the eye draft of the object η Argus, Feb. 1873, the principal stars appear to have retained their relative position as shown in the drawing of last year. The dark spaces are extending and becoming more undefined, gradually filling up with small stars, fully half as many again as shown in last year's drawing; the whole field of the telescope when directed to η is studded with stars from the 7th to the 12th magnitude, too numerous to count. I have on the present occasion omitted to make a drawing of the object, as in all probability before long photography will be applied both to this and other portions of the dense Nebula between it and κ Crucis—a thing much required.

A MAGNIFICENT work, in the shape of a Photographic Album of Ethnology ("Anthropologisch-Ethnologisches Album"), from the collections of the Berlin Anthropological Society, is about to be published in parts, by Wiegandt and Hempel, of Berlin, the photographs by C. Dammann, of Hamburg. Each part will contain five leaves 48 centimetres in length by 64 centimetres in breadth, each part in a separate portfolio. The contents will be arranged in tables containing from ten to twenty photographs each, and the price of each part is twelve thalers. The first part contains two tables illustrative of the East Coast of Africa, and three tables for Asia, illustrating Eastern Siberia, Japan, Siam, &c. Appended to each portrait is a brief description indicating the country, particular district, sex, and age of the original. The immense value of such a work to ethnologists is evident.

FROM the "Report of the Radcliffe Observer to the Board of Trustees," we see that a considerable amount of regular observatory work has been done during the past year, and that the establishment is in good condition.

IN a letter to the *British Medical Journal*, Mr. J. C. Galton refers to a specimen of a human heart in which the "moderator band" recently found by Prof. Rolleston in the Cassowary, and long known to be well developed in Ruminants as a strong fibrous cord, running in the right ventricle between its outer wall and the septum, is well developed as a thick muscular band. But he remarks that from it "some of the chordæ tendineæ of the tricuspid valve take origin." Prof. Rolleston also considers that one at least of the columnæ carneæ in man, which are unattached in the middle of their course, and are in connection with the musculi papillares of the tricuspid valve, is homologous with it. In the Ruminant, however, the band is quite free and of fibrous structure, and is apparently a much more specialised development than the uncertain muscular cords found in the human heart.

THE report of work contained in the "Proceedings of the Liverpool Naturalist's Field Club for the year 1872-3," appears to us, on the whole, gratifying. The Society made nine field excursions during last summer, and, considering the unsettled state of the weather, these were well attended. The working members of the Society, during these excursions, devote themselves mainly to botanical collecting, though the majority of those who make up the parties spend their time in visiting places of antiquarian and historical interest. Prizes are given for botanical collections, and we are afraid the Society do not take the precaution of urging upon collectors the danger of extirpating the rare plants of the districts visited in their eagerness to make up prize-taking collections. Several evening meetings were held during last winter, at the first of which Mr. Fisher gave a *résumé* of the Botanical gains of the Society during the excursions. The following valuable papers were also read at these meetings:—"On the Respiration and Germination of Plants," by Dr. Carter; "Corals and Coral Islands," by the Rev. H. H. Higgins, President; "On the Intimate Relations between the Animal and Vegetable Kingdoms," by Mr. Chantrell; "On the Sap of Plants, the Physical Causes of its Ascent, and its Composition," by Mr. Davies. We have also received an "Appendix to the Flora of Liverpool," containing a considerable number of additions to that valuable work, which we noticed on its appearance about a year ago.

IT is said that the scheme which has been on foot for some time past, having for its object the closer connection of St. Andrew's University with the neighbouring town of Dundee, by the establishment of an affiliated college there, on the same principle as the Science College at Newcastle is connected with the University of Durham, has fallen through, several of the St. Andrew's professors being of opinion that if this arrangement were entered into it would ultimately end in the University being transferred across the Tay.

THE first four parts of an "Illustrated International Review of the Universal Exhibition of Vienna 1873," have come to hand. It is a handsome and well-illustrated folio, printed in French, German, and English, and promises to be an "absolutely complete encyclopædia of the Vienna Exhibition of 1873, at once descriptive, artistic, scientific, anecdotic, and biographical." If the prospectus is faithfully carried out, the work will be very valuable both in a scientific and an industrial point of view.

ADVICES to the 12th of June, dated Denver, U.S., make mention of satisfactory progress in the explorations conducted by Professor Hayden and his parties. One of the divisions of the survey at that time was established near Central City, in charge of Mr. Jackson, and consisted of Mr. Coulter as botanist, Mr. Carpenter as naturalist, and Mr. Cole as assistant naturalist. They had already obtained a large collection of plants and zoological objects, having spent two weeks high up in the mountains. Mr. Jackson had made about fifty negatives of the higher peaks, principally in the vicinity of Long's Peak. They expected to proceed shortly to the "Garden of the Gods." Mr. Gardner has been occupied in establishing his base line of triangulation. He has already erected three signal monuments thirty feet high, and twelve miles apart, all of which can be seen from the main range of mountains. One party is at work on Long's

Peak, in charge of Mr. Marvin, accompanied by Mr. Gardner, and another under Mr. Gannett, accompanied by Dr. Peale, as geologist, and Mr. Batty as naturalist. According to the *Denver News*, the cattle, finding these constructions extremely convenient places for scratching, and thinking them apparently erected for their accommodation, have at once commenced appropriating them to that purpose, and evidently with great satisfaction, as it is said that they concentrate in their vicinity for miles around.

"ANNALEN des Physikalischen Centralobservatoriums" is the German title of the record for 1871 of the work done at the great Physical Observatory of St Petersburg. It is a very thick quarto in Russian and German, and contains full and well-arranged meteorological statistics for fifty-five Russian towns for the year 1871.

THE following are the principal additions to the Brighton Aquarium during the past week:—10 Thornback Rays (*Raia clavata*), 1 Large Tope (*Galeus canis*), 1 Large Smooth Hound (*Mustelus vulgaris*) 3 Three-bearded Rockling (*Motella tricirrata*), 1,000 Sticklebacks (*Gasterosteus spinosus*), 1 fine group of *Actinoloba dianthus* (orange variety); a Smooth Hound (*Mustelus vulgaris*) gave birth to seven young ones, which died immediately, or were born dead.

THE additions to the Zoological Society's Gardens during the past week include two Maugé's Dasyures (*Dasyurus maugei*) from Australia, presented by Mr. George Heath; a Tytler's Paradoxure (*Paradoxurus tytleri*) from the Andaman Islands, presented by Mr. J. S. Campbell; a Bactrian Camel (*Camelus bactrianus*) from Asia; a Gibbon (*Hylobates* sp. ?); a Crowned Eagle (*Spizaetus coronatus*) from Senegal; three Blue-crowned hanging Parrakeets (*Loriculus galgulus*) from Malacca; an Egyptian Fox (*Canis niloticus*); an Egyptian Vulture (*Neophron percnopterus*), purchased; an Ocelot (*Felis pardalis*) from America; a Hobby (*Hypotriorchis subbuteo*) from this country, and four red-billed Tree Ducks (*Dendrocygna autumnalis*) from America, deposited.

ON THE TEMPERATURE AT WHICH BACTERIA, VIBRIONES, AND THEIR SUPPOSED GERMS ARE KILLED[*]

WHILST a heat of 140° F. (60° C.) appears to be destructive to *Bacteria*, *Vibriones*, and their supposed germs in a neutral saline solution, a heat of 149° or of 158° F. is often necessary to prevent the occurrence of putrefaction in the inoculated fluids when specimens of organic infusions are employed. What is the reason of this difference? Is it owing to the fact that living organisms are enabled to withstand the destructive influence of heat better in such fluids than when immersed in neutral saline solutions? At first sight it might seem that this was the conclusion to be drawn. We must not, however, rest satisfied with mere superficial considerations.

The problem is an interesting one; yet it should be clearly understood that its solution, whatever it may be, cannot in the least affect the validity of the conclusion arrived at in my last paper, viz., that living matter is certainly capable of arising *de novo*. We were enabled to arrive at the conclusion above mentioned regarding Archebiosis by starting with the undoubted fact that a heat of 158° F. reduces to a state of potential death all the *Bacteria*, *Vibriones*, and their supposed germs which an organic infusion may contain. The inquiry upon which I now propose to enter, therefore, touching the degree of heat *below this point* which may suffice to kill such organisms and their supposed germs in an organic infusion, and touching the cause of the delayed putrefaction apt to take place in inoculated organic infusions which have been heated to temperatures above 140° and below 158° F., is one lying altogether outside the chain of fact and inference by which the occurrence of Archebiosis is proved.

It seems to me that the solution of the problems which form the subject of the present communication can only be safely attempted by keeping constantly before our minds two main considerations:—

Thus, in the experiments whose results it is now our object to endeavour to explain, the fluids have been inoculated with a compound consisting partly (*a*) of living units, and partly (*b*) of a drop of a solution of organic matter in a state of molecular change; so that in many cases where putrefaction has been initiated after the inoculating compound has been heated to certain temperatures, there is the possibility that this process of putrefaction may have been induced (in spite of the death of the organisms and their germs) owing to the influence of *b*, the dissolved organic matter of the inoculating compound; that is to say, the heat to which the mixture has been exposed may have been adequate to kill all the living units entering into the inoculating compound, although it may not have been sufficient to prevent its not-living organic matter acting as a ferment upon the infusion.

And there are, I think, the very best reasons for concluding that in all the cases in which turbidity has occurred after the organic mixtures have been subjected to a heat of 140° F. (60° C.) and upwards, this turbidity has been due, not to the survival of the living units, but rather to the fact that the mere dead organic matter of the inoculating compound has acted upon the more unstable organic infusions in a way which it was not able to do upon the boiled saline fluids.

The reasons upon which these conclusions are based are the following:—

1. Because the turbidity which has occurred in inoculated organic infusions that have been subjected to a temperature of 140° F. has always manifested itself appreciably later, and advanced much more slowly than in similar mixtures which had not been heated above 131° F.; whilst it has commenced even later, and progressed still more slowly, when occurring in mixtures previously heated to 149° F. Such facts might be accounted for by the supposition that exposure in these organic fluids to the slightly higher temperature suffices to retard the rate of growth and multiplication of the living units of the inoculating compound, although the facts are equally explicable upon the supposition that the later and less energetic putrefactions are due to the sole influence of the mere organic matter of the inoculating compound.

2. So far as the evidence embodied in the Tables goes, it tends to show that the more unstable different specimens of similar infusions are (that is, the stronger they are), the more rapidly and frequently does late turbidity ensue, and the more this late turbidity approaches, both in time of onset and in rate of increase, to that which occurs when inoculated infusions are not heated to more than 131° F.—when both living and non-living elements of the inoculating compound act conjointly as ferments. Such facts show quite clearly that where the intrinsic or predisposing causes of change are strong, there less potent exciting agencies are more readily capable of coming into play; but they still do not enable us to decide whether the exciting cause of this delayed turbidity is in part the living element whose vitality and rate of reproduction has been lowered by the heat, or whether the effects are wholly attributable to the mere organic matter of the inoculating compound.

So far, therefore, we have concomitant variations which are equally compatible with either hypothesis. But it will be found that each of the three succeeding arguments speaks more and more plainly against the possible influence of the living element, and in favour of the action of the organic matter of the inoculating compound, as an efficient exciting cause of the delayed putrefactions occurring in the cases in question.

3. As stated in my last communication,[*] when single drops of slightly turbid infusions of hay or turnip previously heated to 140° F. are mounted and securely cemented as microscopical specimens, no increase of turbidity takes place, although drops of similar infusions heated only to 122° F. do notably increase in turbidity (owing to the multiplication of *Bacteria*) when mounted in a similar manner. Under such restrictive conditions as these, in fact, a drop of an inoculated and previously heated organic infusion behaves in precisely the same manner as a drop of a similarly treated ammonic-tartrate solution. In each case, when heated to 140° F., turbidity does not occur, apparently because there are no living units to multiply, and because in

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[*] Extracts from a paper by Dr. H. Charlton Bastian, F.R.S., read before the Royal Society May 1, 1873

[*] See NATURE, vol. vii. p. 435.

these mere thin films of fluid dead ferments are as incapable of operating upon the organic fluids as they are upon the ammonic-tartrate solutions.

4. Because, in the case of the inoculation of fluids which are not easily amenable to the influence of dead ferments, such as a solution containing ammonic tartrate and sodic phosphate, this delayed turbidity does not occur at all. Such inoculated fluids become rapidly turbid when heated to 131° F., though they remain clear after a brief exposure to a temperature of 140° F. When the living units in the inoculating compound are boiled, there is nothing left to induce turbidity in such solutions. The mere fact that these fluids do not undergo change when exposed to the air proves conclusively that they are very slightly amenable to the influence of the ordinary dead organic particles and fragments with which the atmosphere abounds. The absence of delayed turbidity in these fluids serves, therefore, to throw much light upon the cause of its occurrence in the organic infusions.

5. And, lastly, I can adduce crucial evidence supplied by the "Method of Difference," speaking with its accustomed clearness. Two portions of the same hay- or turnip-infusion can be inoculated in such a manner as to supply us with the information we require. In the one case we may employ a drop of a turbid ammonic-tartrate solution previously heated to 140° F., in which, therefore, the living units would certainly be killed: whilst in the other we may add an unheated drop of the same turbid saline solution to the organic fluid, and then heat this mixture also to the temperature of 140° F. The comparative behaviour of these two inoculated fluids (placed, in the ordinary manner, in previously boiled corked phials) should be capable of showing us whether the living elements of the inoculating compound were able to survive when heated in the organic infusion. If they did survive, the fluids inoculated in this manner ought to undergo putrefaction earlier and more rapidly than those inoculated with the drop of turbid fluid, in which we know that the *Bacteria*, *Vibriones*, and their supposed germs would have been reduced to a state of potential death. With the view of settling this question, therefore, the following experiments were made:—

Description of Experiments	Results.	Inferences.
A. Boiled ammonic-tartrate solution, inoculated with an unheated drop of a similar solution turbid with *Bacteria*, &c.	Turbid in 40 hrs.	That boiled ammonic-tartrate solution is a fluid inoculable by living *Bacteria*, &c., and favourable for their growth and rapid multiplication.
B. Boiled ammonic-tartrate solution, inoculated with a drop of a turbid saline solution previously heated to 140° F.	Clear at expiration of 8th day.	That *Bacteria*, *Vibriones*, and their supposed germs are either killed or deprived of all power of multiplication when heated to 140° F. in this fluid
C. Boiled turnip- and hay-infusions, inoculated with a drop of a turbid saline solution previously heated to 140° F.	Turnip-infusions turbid in 2½ days. Hay-infusions clear at expiration of 8th day.	The precisely similar behaviour of the turnip- and hay-infusions of series C and series D respectively shows that *Bacteria*, *Vibriones*, and their supposed germs are as inoperative in series D as they are known to be in series C, whilst the behaviour of the hay-infusions shows that they are little amenable to the influence of the drop of the saline fluid when its living units are killed.
D. Boiled turnip- and hay-infusions, inoculated with a drop of an unheated turbid saline solution, the inoculated fluid being subsequently heated to 140° F.	Turnip-infusions turbid in 2½ days. Hay-infusions clear at expiration of 8th day.	
E. Boiled turnip- and hay-infusions, inoculated with a drop of an unheated saline solution, the inoculated fluid being subsequently heated to 131° F.	Turnip-infusions turbid in 28 hrs. Hay-infusions turbid in 38 hrs.	Shows that a heat of 131° F. is not sufficient to kill *Bacteria*, *Vibriones*, and their supposed germs in organic infusions, and, again, that turnip-infusions are more rapidly influenced by such an inoculating agent than some hay-infusions.*

No experiments could speak more decisively. Those of series B show that *Bacteria*, *Vibriones*, and their supposed germs are either actually or potentially killed when heated to 140° F. in the neutral saline fluid, which the experiments of series A show to be eminently favourable for their growth and reproduction. Being certain, therefore, that the living units are killed in the drops with which the fluids of series C were inoculated (because they were drops of the same fluid as was employed in series B), we may be equally certain that the turbidity and putrefaction which did ensue in the turnip-solutions of series C were due to the influence of the mere dead constituents of these drops of the turbid saline fluid; whilst, seeing that the behaviour of the fluids of series D was precisely similar to those of series C, we have a perfect right to infer that this series of fluids (D) was as devoid of living units as those of C are known to be—that is, that *Bacteria*, *Vibriones*, and their supposed germs are killed by the temperature of 140° F. in organic fluids, just as they are in saline fluids, although, as shown by the experiments of series E, they do not succumb to a heat of 131° F.

The evidence now in our possession shows, therefore, that whilst the temperature at which living ferments cease to be operative varies within very narrow limits (131°–140° F.), that which destroys the virtues of non-living ferments varies within much wider limits, and depends not only upon the amount of heat employed, but also upon the nature of the putrescible or fermentable liquid to which such ferment is added, in conjunction with the degree of heat and other conditions to which the mixture is subsequently exposed.* Here, therefore, we have evidence as to the existence of a most important difference between living and not-living ferments, which has always been either unrecognised or more or less deliberately ignored by M. Pasteur and his followers.† This difference is, moreover, thoroughly in accordance with the broad physico-chemical theory of fermentation which has been so ably expounded by Baron Liebig and others, and the truth of which may now be regarded as definitely established. According to this theory "living" matter, as a ferment, would take rank merely as a chemical compound having a tolerably definite constitution; and this, we might reasonably infer, would, like other chemical compounds, be endowed with definite properties, and amongst others that of being decomposed or radically altered by exposure to a certain amount of heat. Looked at also from this essentially chemical point of view, it would be only reasonable to expect that the molecular movements of living ferments with a lowered vitality might not be more marked or energetic than those which many not-living organic substances are apt to undergo; and this being the case, we might expect that there would often be a great practical difficulty in ascertaining whether a ferment belonging to the arbitrary and artificial (though, in a sense, justifiable and natural) category of "living" things had or had not been in operation.

Dr. Bastian then refers to certain statements made by M. Pasteur, and afterwards classifies the various fermentable fluids under three main divisions:—I. Self-fermentable fluids; II. Fluids which will not ferment without the aid of unheated organic matter, either not-living or living; III. Fluids which will only ferment under the initiating influence of living matter.

Dr. Bastian's conclusions from these investigations are thus expressed:—

Thus it can now be proved, by evidence of a most unmistakable nature, that the process of putrefaction which invariably occurs in previously boiled putrescible infusions contained in flasks with narrow but open necks is not commonly (is, perhaps, only very rarely) initiated by living germs or organisms derived from the atmosphere; it can also be proved that putrefaction and the appearance of swarms of living organisms may occur in some boiled fluids when they are simply exposed to air which has been filtered through a firm plug of cotton-wool or though the narrow and bent neck of a flask, to air whose particles have been destroyed by heat, or even in fluids hermetically sealed in flasks from which all air has been expelled. The evidence in our possession is therefore most complete on this part of the subject: it shows beyond all doubt, not only that putrefaction may and does very frequently occur under conditions in which the advent of atmospheric particles, whether living or dead, is no longer possible, but also that living particles derived from the atmosphere can only be very rare and altogether exceptional initiators of the putrefaction which invariably occurs in previously boiled infusions exposed to the air.

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* These experiments of series C, D, and E were many times repeated with specimens of the same turnip- and hay-infusions, the specific gravity of the former being about 1008 and that of the latter 1005. Different specimens of hay especially vary so much that it becomes absolutely essential to use portions of the same infusion for the comparative experiments of these different series.

* See "The Beginnings of Life," vol. i. p. 437.

† See, for instance, all M. Pasteur's celebrated experiments in which he had recourse to an "ensemencement des poussières qui existent en suspension dans l'air," as recorded in chaps. iv. and v. of his memoir in "Ann. de Chimie et de Physique," 1862. M. Pasteur was engaged in an investigation, one of the avowed objects of which was to determine whether fermentation could or could not take place without the intervention of living organisms, which M. Pasteur held (in opposition to many other chemists) to be the only true ferments. In his inoculating compound (dust filtered from the atmosphere), there was, as M. Pasteur was fully aware, a large amount of what his scientific opponents considered non-living ferment, whilst *possibly* there existed a certain number of living ferments. In explaining the results of his experiments, however, M. Pasteur and others thought he was pursuing a logical and scientific method when he attributed these results to the action of the possibly existing element of the inoculating compound, whilst he ignored altogether the other element which was certainly present in comparatively large quantity, and the testing of whose efficacy was the ostensible object of his research.

Again, the evidence which we now possess with reference to the influence of heat upon *Bacteria*, *Vibriones*, and their supposed germs is no less decisive. It has been unmistakably proved that such organisms and their imaginary germs are either actually or potentially killed by a brief exposure to the temperature of 140° F. when in the moist state; and it had also been previously established that they are invariably killed by desiccation even at much lower temperatures.*

But if living germs do not come from the air to contaminate the previously boiled fluids, and if it is not possible for any of them to have escaped the destructive influence of heat in the boiling fluid or on the walls of the vessel in which the fluid is contained, what can be the mode of origin of the swarms of living things which so rapidly and invariably appear in such infusions when contained in open flasks, and which so frequently appear when the infusions are contained in flasks whose necks are closed against atmospheric particles of all kinds? They can only have arisen by the process which I have termed Archebiosis.

CONCLUSIONS

If a previously boiled ammonic-tartrate solution remains free from *Bacteria* and *Vibriones* when exposed to the air, it is because the air does not contain living organisms of this kind or their supposed germs, and because mere dead organic particles are not capable of initiating putrefaction in such a fluid.

And if ordinary organic infusions previously boiled and exposed to the air do rapidly putrefy, though *some* of the same infusions when exposed only to filtered air remain pure, it is because such fluids are, in the absence of living units, quite amenable to the influence of the dead organic particles which the air so abundantly contains, although they are not self-fermentable.

Whilst if other more changeable fluids, after previous boiling, when exposed to filtered air or cut off altogether from contact with air, do nevertheless undergo putrefaction or fermentation, it is because these fluids are self-fermentable, and need neither living units nor dead organic particles to initiate those putrefactive or fermentative changes which lead to the evolution of living organisms.

SCIENTIFIC SERIALS

THE June number of the *Journal of Anatomy and Physiology* contains several papers of special interest, as well as the excellent summaries by Profs. Turner and Rutherford, of the progress of Anatomy and Physiology during the last six months. Prof. Turner describes, for the first time, the Visceral Anatomy of the Greenland Shark (*Læmargus borealis*) from two specimens caught near the Bell Rock. The larger was 11 feet 8 inches long, and the other 8½ feet: they were both females. The most important peculiarities of this fish, wherein it differs from other sharks are, that the *bursa entiana* is not developed; that there are two large duodenal cæca, one of which is closely adherent to the pyloric tube, as well as a true pancreas, corresponding with the similar condition found by Alessandrini in the Sturgeon; and that there are no oviducts, so that the ova must be discharged into the peritoneal cavity. From these peculiarities the author places *Læmargus* in a family by itself, named by him *Læmargidæ*.—Prof. Turner also, in a short paper on the so-called claw at the end of the tail of the lion, shows that no true claw exists, but that the tip of the tail is hairless, and becomes hard on drying.—Prof. Rutherford tabulates experiments proving that the retardation of the pulse in the rabbit, which follows closure of the nostrils, depends on the obstruction of the respiration, and not as Drs. Brown-Séquard and Sanderson supposed, on direct reflex action. Mr. Dewar and Dr McKendrick describe experiments on the Physiological Action of Light, an account of which has already appeared in this journal.—Mr. Blake, of San Francisco, has a paper on the action of the salts of the metals sodium, lithium, cæsium, &c., when introduced directly into the blood. Mr. A. H. Smee, in a paper on the physical nature of the coagulation of the blood, endeavours to prove that it coagulates in obedience to a purely physical law, namely, the power of soluble colloid matter to pectinise, or spontaneously to coagulate. Mr. Garrod, on the law which regulates the frequency of the pulse, proposes as a substitue for that given by Marey, the following:—the heart re-commences to beat when the arterial tension has fallen an invariable proportion, this being the only possible explanation of the facts that pulse rate varies with arterial resistance and not with blood pressure. He also gives a new theory of the source of nerve force.—Dr. Charles, Prof. Curzon, and Prof. Drachmann, record peculiarities in anthropotomy, the first in the arterial system, the second in the muscular and nervous system, and the third in the muscular.—There is an excellent and very careful review, by Mr. Trotter, of the Rev. Samuel Haughton's "Principles of Animal Mechanics," which will be very valuable to many physiologists, who here have the opportunity of seeing the opinion of a mathematician, who is also a biologist, of a work which might by itself lead them to think that the physiological basis for work was in a better position than it really is.

Bulletin Mensuel de la Société d'Acclimatation de Paris for June. A great portion is devoted to the description of the best modes of rearing silkworms and the more suitable kinds of food for feeding them. A paper is devoted to the Japanese Mulberry (*Morus japonica*), which is being introduced into France as producing a superior food for the silkworm.—The cultivation of various kinds of beans and melons is advocated by M. Bossin, and his paper might be read with advantage in this country, where these vegetables are not sufficiently valued as an article of diet. Not only the acclimatisation of useful, but the destruction of hurtful animals, plants, and insects, forms part of the programme of the society, and we have therefore some remarks on insecticides and on the preservation of insectivorous birds.—The American notes on pisciculture, on the grey wolf, and the commerce of Chicago are interesting. A black monkey from Sumatra has just arrived at the Jardin d'Acclimatation, but it is not expected to live.

SOCIETIES AND ACADEMIES

LONDON

Quekett Microscopical Club, July 25.—Dr. Braithwaite, F.L.S., president, in the chair.—This being the annual meeting, the report of the committee for the past year was read, *and testified to the continued prosperity* of the club, which now numbers 570 members.—The president delivered the annual address, in the course of which he noticed the progress of microscopical investigation in Botany and Zoology during the past year.—The ballot then took place for the election of officers. Dr. Braithwaite was re-elected president; Dr. Matthews, Messrs. B. T. Lowne, T. W. Burr, and C. F. White, vice-presidents; and Messrs. Bywater, Crisp, Hailes, Hind, Waller, and T. C. White, were elected to fill the six vacancies on the committee. Mr. J. E. Ingpen succeeded Mr. T. C. White, who retires from the office of hon. sec. (owing to increase of his professional duties), after four years of unremitting and valuable service. The proceedings terminated with the usual *conversazione*.

BELGIUM

Royal Academy of Sciences, May 13.—Reports were given in on the following papers:—On the Superficial Tension of Liquids considered in reference to certain movements observed on their surface, by M. G. Van der Mensbrugghe, which it was resolved to print in the *Memoires*.—On the Osculatory Sphere, a note by M. L. Saltel, which is printed in the *Bulletin*.—On the chloric acetonitriles, by M. L. Bisschopinck, also printed in the *Bulletin*.—Essay on the state of vegetation at the epoch of the Heersien Marls of Gelinden, by Count G. de Saporta and Dr. A. F. Marion. It was resolved to print this paper with the plates in the *Memoires*.—The following communications were made:—On frozen alcoholic drinks carried to very low temperatures, and on the cooling and freezing of ordinary or sparkling wines, which will appear in the *Bulletin* for June.—Third addition to the synopsis of the Caloptergines, by M. de Selys-Longchamps. His first list was published in 1853, and additions in 1859 and 1869; the present long list contains descriptions of many new species, as well as corrections of and additions to species already described. The author is indebted for the greater part of his material to Mr. MacLachlan.

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* See the experiments and conclusions of Dr. Burdon Sanderson in Thirteenth Report of Med Officer of Privy Council, p. 61. This fact of the inability of these organisms and their germs to resist desiccation shows the futility of some objections which have been from time to time raised by those who thought that *Bacteria*, *Vibriones*, and their germs might resist the destructive influence of heat by adhesion to the glass above the level of the fluid, or even in the fluid itself, just as dried and very thick-coated seeds have been known to do. Dry heat would seem to be even more fatal to such organisms and their germs than a moist heat of the same degree, owing to their extreme inability to resist desiccation; if they become dry they are killed at a temperature of 104° F., whilst if they remain moist they succumb, as we have seen, to a temperature of 140° F.

PHILADELPHIA

Academy of Natural Sciences, May 6.—Dr. Carson, vice-president, in the chair. Double Flowers in *Epigæa repens*.—Mr. Thomas Meehan observed, that on several occasions, during the few past years, it had been noticed among the *variations in nature*, that the tendency to produce double flowers was, by no means, the special prerogative of the florist to originate. Many of our commonest wild flowers, which no one would think of cultivating, had double forms in cultivation which were no doubt originally found wild. Thus we had a double *Ranunculus acris*, *R. bulbosus*, *R. Ficaria*, *R. repens*, and some others. There were, in plants, two methods by which a double flower was produced. The axis of a flower was simply a branch very much retarded in its development, and generally there were, on this arrested branch, many nodes between the series forming the calyx or corolla, and the regular stamens and carpels, which were entirely suppressed. But when a double flower was produced, sometimes these usually suppressed nodes would become developed, in which case there was a great increase in the number of petals, without any disturbance in the staminal characters. But at other times there was no disturbance in the normal character of the axis. The stamens themselves merely became petaloid. This was the case in the *Epigæa*, recently found by Dr. Darrach.—Influence of Cohesion on Change of Characters in *Orchideæ*.—Mr. Meehan also said that in the early part of the winter he had exhibited some flowers of *Phaius Tankervillæ*, in which, by the mere cohesion of one of the dorsal petals with the column, a flower differing very much from the general condition was the result. Since that time Dr. Maxwell T. Masters, in the issue of the *Gardener's Chronicle* for April 12th, notices the receipt of a *Phaius Wallichii* in which there had been produced three spurs and regular petals, looking, Dr. M. says, rather like those of a gladiolus than of an orchid.

May 13.—Dr. Ruschenberger, president, in the chair. The following paper was presented for publication:—"Observations on Nests of *Sayornis fuscus*," by Thos. G. Gentry.—Prof. Cope exhibited and described some extinct turtles from the Eocene strata of Wyoming.

May 20.—"Descriptions of new species of Orthoptera, collected in Nevada, Utah, and Arizona, by the Expedition under Lieut. G. M. Wheeler," by Cyrus Thomas.—"Observations on the Habits of the Neuters of *Formica sanguinea*," by T. G. Gentry.—*Lilium Washingtonianum*.—Mr. Thomas Meehan referred to a paper by Prof. Alphonso Wood, entitled a "Sketch of the Natural Order of Liliaceæ," of the Pacific coast, published in the volume of the Proceedings for 1868, in which he describes a "new species" of Lilium, as *L. Washingtonianum*, giving, as a reason for the name, that it was generally known as the "Lady Washington" by the miners. Prof. W. said, in his paper, that it was remarkable so fine a plant had been overlooked by other botanists. It so happens that it had not been overlooked, but had been described ten years previously by Dr. Kellogg, in the Proceedings of the California Academy for 1858.—"On a Species of Delphinus," by Dr. H. C. Chapman.

PARIS

Academy of Sciences, July 21.—M. de Quatrefages, president, in the chair.—The following papers were read:—Note on changes of rate in isochronous regulators, by M. Yvon Villarceau.—Third note on guano, by M. Chevreul.—New researches tending to prove that the co-ordinating power over bodily movements lies in the cerebellum, &c., by M. Bouillaud.—The laws of friction and concussion on the thermo-dynamical theory, by M. A. Ledieu.—On the movement of a spherical segment on an inclined plane, by Gen. Didion.—On the spectra of iron, and some other metals, by Father A. Secchi. The author had failed when examining the iron spectrum given by a battery of fifty cells, to observe the line 1474K, and he gave, in the present paper, an account of a further search for it. The same battery power, with new acids, was used; various samples of iron were burnt in the arc, either as iron poles or placed in hollow carbon points, and the sunlight was reflected into the spectroscope with a heliostat. The line in question could not be found in any sample of iron used. His other observations are on the "structure" spectra of carbon and aluminium; he observes that each line of the columnar bands is itself resolvable into a mass of fine lines.—On the permeability of the Fontainbleau sands, by M. Belgrand.—On the movement of the wash produced in artificial canals, and on causing water to rise along an inclined bank to a sensibly constant height. A letter from Mr. Nordenskiöld, dated Mossel Bay, latitude 79° 54′ N. was read by M. Daubrée.—New spectroscopic observations of the sun which do not agree with certain sun-spot theories, by Father Tacchini. The theories are those of M. Faye and Father Secchi. The author describes watching a facula over the sun and observing its appearance on the limb which was accompanied by the reversal of large numbers of metallic lines in the chromosphere. This, Tacchini considered as evidence of an eruption, and as militating against Faye's theory because he considers that theory not to allow of eruptions, and against Secchi also, he having stated that faculæ were eruptions, and spots the erupted matter, and yet this facula had no spots during half a revolution.—On Euler's constant and Binet's function, by M. E. Catalan.—Researches on electric condensation, by M. V. Neyreneuf.—Studies on nitrification in soils, by M. T. Schlœsing.—On a combination of picric acid, with acetic anhydride, by MM. Tommasi and David. The authors considered this body as a picrate, in which one atom of metal is replaced by acetyl.—On pyrogallic acid in the presence of iodic acid, by M. Jacquemin.—On a natural combination of ferric and cuprous oxides, and on the production of atacamite, by M. C. Friedel.—On the spontaneous changes of eggs, by M. Gayon.—An attempt to determine, by comparative embryology, the analogous portions of the intestines in the superior vertebrata, by M. Campana. During the meeting, an election was made to the place of *Membre libre*, vacant by the death of M. Verneuil. M. de Lesseps obtained 33 votes, M. Breguet 24 votes, MM. du Moncel, Jacquemin and Sedillot, 1 each. M. de Lesseps was therefore declared duly elected.

BOOKS RECEIVED

AMERICAN.—Views of Nature: Ezra C. Seaman (Scribner & Co., N.Y.).

FRENCH.—Traité Générale de Photographie. 6th ed.: D. v. Monckhoven (G. Masson, Paris).

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ERRATA.—P. 201, col. 1, 1st line below table, after = insert λ. P. 246, title of Fig. 2, for *Salenica* read *Salenia*.

THURSDAY, AUGUST 7, 1873

GUSTAV ROSE

THE son-in-law of Gustav Rose, Professor G. vom Rath, has sent us the Nekrolog which affection and custom in the Fatherland unite in issuing in honour of those who are no more.

The first line of this tribute to the memory of the great mineralogist tells truly that Germany has lost one of her great ones in this learned and noble man: but it is for us to say that it is even a wider world than his fatherland that has lost in him one of its conspicuous citizens. For the two brothers Heinrich and Gustav Rose formed a double star in the constellation of illustrious men who have illuminated with a brilliancy all its own the first half of this great century; and, indeed, for now fifty years their twin lights have guided the course of their contemporary and of a younger generation of wayfarers on the track of Science.

Certainly the death of a man like Gustav Rose is calculated to call up some wonderment in our minds as we look back over the brief period that even his 76 years of life embrace, and think in what relation that little space of time stands to the long history of man in regard to the sciences that these two illustrious brothers cultivated so pertinaciously and so well. Berzelius spoke of looking back within his own memory to the dark age of phlogistic chemistry. Heinrich Rose first reduced to a scientific system the methods of inorganic chemical analysis, as J. von Liebig did afterwards for organic chemistry; it is but yesterday that the one, and but a few brief years since the other died. And now Gustav Rose, the first man in Germany who used the reflective goniometer, has followed them and Mitscherlich and Haussmann, and Haidinger. There still remain Breithaupt and Naumann, Wohler, and a few other honoured men on whom the patriarch's mantle must successively devolve. Let us at least pay the tribute due to the memory of the last of these illustrious workers whose chair is empty by endeavouring to take a survey of the work he did, and by recognising the debt we owe him for the results that have accrued to our knowledge from the toil "Ohne Hast und ohne Rast," of fifty out of his seventy-six years, and no less for the example he has set of method and of energy in achieving them.

The sciences that Gustav Rose devoted himself to, crystallography and mineralogy, have been for many years so little or so superficially studied in England, that probably few of our countrymen are familiar with the continuous succession and admirable quality of the work turned out from the study of one of the soundest-minded, and, let us add, one of the soundest-hearted men that Germany ranked among her sons.

His country's troubles, though they ended as far as the great war was concerned in 1815, had called into the ranks even the youngest of the four brothers Rose. Their father, a not undistinguished pharmaceutical chemist in Berlin, had died in 1807, leaving his children to the care of his widow, who appears to have borne out the tradition of able men owing much to remarkable characteristics in their mothers. Young Gustav was not old enough in the days of the terrible conflicts to have borne his musket. But he was seventeen, in time to make the long march from Berlin to Orleans; and after the peace in 1815 he set himself to obtain a livelihood in the occupation of mining. Overtaken by an attack of inflammation of the lungs, his thoughts became directed into a new direction. For the contagious passion for the pursuit of truth in its most tangible form by the path of natural science seized him by contact with his elder brother Heinrich; and Gustav followed his example in going to Stockholm for a similar object to that which has drawn so many Englishmen and English-speaking men since to Germany. Berzelius was then in Sweden what afterwards were Heinrich Rose, Wöhler, Liebig, in the Fatherland; the great master in the science as in the practice of chemistry. Gustav Rose was twenty-six when he ceased to be a student, and of the fifty years that have run out their sands since 1823, there is scarce one that has not recorded some work or memoir by the great crystallographer; and in some of those years he produced several.

And Gustav Rose was a crystallographer and mineralogist in the completest sense. The first man in Germany, as we have said, who adopted the use of Dr. Wollaston's reflective goniometer, he aided Mitscherlich in his discovery of Isomorphism; and this must have been one of his earliest labours.

His first paper was an exercise in Latin on the Crystallography of Sphene; and in 1830 he brought out his treatise on Crystallography, in which recognising the simplicity introduced by the use of geometrical axes as employed by Weiss, he adopted that method of expression for the relations of the faces of a crystal, a method which has in fact been only carried out to its last logical form and simplest expression by the admirable system of our countryman Prof. W. H. Miller.

It is not easy now to transport ourselves back to the time when scientific men of high eminence deliberately closed, or rather refused to open, their eyes to the chemical composition of a mineral as the most fundamental point in its definition and description, and to its chemical relations as affording the only philosophical basis on which to form a classification of minerals. But this difficulty of placing ourselves in the position taken up by Mohs and his school, very much arises from our not appreciating the situation of chemical and crystallographic research in their mutual valuation twenty years before the death of Mohs. We may for instance take two garnets, one consisting of aluminium and magnesium silicate, another of iron and calcium silicate. The two minerals contain notably differing proportions of the only ingredient they have in common, namely silica; and yet their crystalline forms are the same, and the mineralogist could not fail to recognise so close a parallelism and similarity between the two minerals as to compel him to unite them under one general "natural-history" division.

The chemistry of that day, however, was not yet ripe for acknowledging such a classification. But when, on the other hand, the mineralogist assembled under one group minerals that differed in the way that, for instance, Linavite and blue copper carbonate (chessylite) differ in their chemical composition, or such widely different minerals as diamond and topaz, on the ground that they were hard and lustrous, and had the character of precious stones; then the remonstrance of the chemist was founded in truth and reason.

It was the discovery of isomorphism that explained the anomalies and enigmas which thus in many cases seemed to justify the mineralogist in standing apart from the chemist, and preferring to discriminate, define, and classify minerals by appealing to superficial characteristics, rather than to the most fundamental feature of such bodies, their chemical molecular structure.

It came now to be seen that in the language of the earlier chemistry alumina and sesquioxide of iron, on the one hand, were able to represent the same ingredient in the garnet, while on the other hand, also, the lime, the magnesia, and the protoxide of iron might equally represent one another in the silicate in question, provided that the chemical structure of the compound was not altered, that is to say, could be expressed by a general formula that was equally applicable to each variety of the mineral; the identity of the crystallographic features of all those garnets being the evidence that the unity of the mineral type had not been overstepped by the interchanges of the elements. The application of this great discovery left chemistry master of the situation, and relegated into the regions of darkness the systems of classification that were not built on chemical and crystallographic principles. It was Mitscherlich, aided as vom Rath tells us by the young Gustav Rose, who made this grand announcement to the world in the year 1823. The light which was thus shed on the dark and till then uncertain problems that might connect the crystalline form with chemical structure, gave, as it were, new life to the vigorous school that owed its chemical precision to the great Professor at Stockholm, the school to which the two Roses and Wöhler belonged. The purely chemical problems of mineralogy received their constant attention; and Gustave Rose, by publishing his crystallography, asserted the co-ordinate functions of the goniometer and the balance in the future discussions of all the larger questions of the mineralogist.

He, in fact, unconsciously perhaps, was now initiating the method to which, with a fine unity of purpose, he adhered through his life.

Thus, for instance, we find him in 1831-33 discussing the somewhat paradoxical resemblance in the crystallographic constants of the minerals augite and hornblende, as suggested by Uralite, a mineral uniting the outline form of the one with the internal structure of the other; in fact a pseudomorph of hornblende after the form of augite.

Then in 1836 came his masterly memoir on the forms of Aragonite, the distinction of which from calcite had been established by Haüy in the beginning of the century. Afterwards, among a mass of works, we find memoirs on the differences of crystallographic habit in Albite, and the nearly related variety of the same felspar pericline, a subject to which he returned in later times; on the dimorphism of iridium, of palladium, and again of zinc; several treating on the marvellous connection by which certain kinds of hemisymmetry in crystals are associated with the localisation on them of opposite electric conditions under changes of temperature (pyro-electricity), which he illustrated in the case of the tourmaline, and among his latest memoirs by a most masterly one on pyrites and cobalt-glance. Quartz he made an object of especial study, explaining the character of its twin forms; and no memoirs in the whole range of crystallographic research, not excepting the splendid work in which Des-Cloiseaux capped, as it were, the labour of Rose, can surpass, in originality and precision, that by this great master on the crystallography of quartz.

Meteorites and the minerals which they contain have challenged the attention and been a sort of exercising-ground for several of the great mineralogists of Germany. Berzelius, indeed, set the example, but it was Rose who, in 1825, measured the first olivine crystal from the Pallas meteorite, and he, Haidinger, Breithaupt, and Wöhler, have all contributed invaluable material for the scientific history of these very difficult and interesting objects of investigation. And to G. Rose we owe the most penetrating insight into their structure, and the best attempt thus far made at classifying them. So, too, the sum of his thought and labour on the classification of minerals was given in his "crystallo-chemische mineral-system," published twenty-one years ago, in which he, so to say, demolished, by leaving no further excuse for perpetuating, the system which was identified with the name of Mohs, or indeed any other system to which chemical law was not the master key.

But one great work that Gustave Rose might have done, and better done perhaps than any living man, was the writing a treatise on Petrography. Mineralogy, the science of minerals, stands to petrography, the science that describes rocks and investigates their history, somewhat as biography stands to history itself, or as histology to physiology. The reason why a geologist is hardly ever a master of petrography is that he is so seldom, in England, at least, a mineralogist. And it is precisely because Gustav Rose was, and Naumann is, a complete mineralogist and crystallographer, and that both have profoundly studied the characters of the minerals in association which form rocks, that either of these two veteran professors might have written—alas! a month ago we might have said may yet write—such a treatise on rocks as probably no other living man could write. Gustav Rose began an admirable training in the field for such a study when, in the company of A. von Humboldt and G. Ehrenberg, he traversed European Russia and found himself among the rocks of the Ourals in 1829. The results of this historical progress were given to the world in two volumes in 1837-1842. The memoirs which he published subsequently to this time and to his becoming full professor (he had been extraordinary professor since 1826) of mineralogy at Berlin, treat very frequently of rock minerals; and indeed deal, in the majority of instances, with those more ordinary minerals which perform an important function as constituents of rocks; quartz, felspar, mica, hornblende, augite, seem never to weary him in observation or exhaust his powers of telling some new fact regarding them. One of his latest papers on the very common mineral, mica, is one of the most admirable of his researches. It was published, like most of his memoirs, in Poggendorff's *Annalen*, and treated on the interpenetration by one another, of various kinds of mica, and of these, with hematite and pennine.

It would be unnecessary, for the purpose of this slight sketch of Gustav Rose's labours, to go further into details regarding his works. He is gone; but his work lives after him.

The two Roses were men of a distinguished presence. Heinrich was the taller, but each was a man of spare and somewhat stately figure, with an eye of peculiar force and truthfulness of glance; an eye that spoke out the character of the man, that beamed with kindliness and was ever staunch to truth. N. S. M.

CHALLIS'S "MATHEMATICAL PRINCIPLES OF PHYSICS"

An Essay on the Mathematical Principles of Physics, &c. By the Rev. James Challis, M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge, and Fellow of Trinity College. (Cambridge: Deighton, Bell, and Co., 1873.)

THIS essay is a sort of abstract or general account of the mathematical and physical researches on which the author has been so long engaged, portions of which have appeared from time to time in the *Philosophical Magazine*, and also in his larger work on the "Principles of Mathematics and Physics." It is always desirable that mathematical results should be expressed in intelligible language, as well as in the symbolic form in which they were at first obtained, and we have to thank Professor Challis for this essay, which though, or rather because, it hardly contains a single equation, sets forth his system more clearly than has been done in some of his previous mathematical papers.

The aim of this essay, and of the author's long-continued labours, is to advance the theoretical study of Physics. He regards the material universe as "a vast and wonderful *mechanism*, of which not the least wonderful quality is, its being so constructed that we can understand it." The Book of Nature, in fact, contains elementary chapters, and, to those who know where to look for them, the mastery of one chapter is a preparation for the study of the next. The discovery of the calculation necessary to determine the acceleration of a particle whose position is given in terms of the time led to the Newtonian epoch of Natural Philosophy. The study from the cultivation of which our author looks for the "inauguration of a new scientific epoch," is that of the motion of fluids, commonly called Hydrodynamics. The scientific method which he recommends is that described by Newton as the "foundation of all philosophy," namely, that the properties which we attribute to the least parts of matter must be consistent with those of which experiments on sensible bodies have made us cognizant.

The world, according to Professor Challis, is made up of atoms and æther. The atoms are spheres, unalterable in magnitude, and endowed with inertia, but with no other property whatever. The æther is a perfect fluid, endowed with inertia, and exerting a pressure proportional to its density. It is truly continuous (and therefore does not consist of atoms), and it fills up all the interstices of the atoms.

Here, then, we have set before us with perfect clearness the two constituents of the universe: the atoms, which we can picture in our minds as so many marbles; and the æther, which behaves exactly as air would do if Boyle's law were strictly accurate, if its temperature were invariable, if it were destitute of viscosity, and if gravity did not act on it.

We have no difficulty, therefore, in forming an adequate conception of the properties of the elements from which we have to construct a world. The hypothesis is at least an honest one. It attributes to the elements of things no properties except those which we can clearly define. It stands, therefore, on a different scientific level from those waxen hypotheses in which the atoms are endowed with a new system of attractive or repulsive forces whenever a new phenomenon has to be explained.

But the task still before us is a herculean one. It is no less than to explain all actions between bodies or parts of bodies, whether in apparent contact or at stellar distances, by the motions of this all-embracing æther, and the pressure thence resulting.

One kind of motion of the æther is evidently a wave-motion, like that of sound-waves in air. How will such waves affect an atom? Will they propel it forward like the driftwood which is flung upon the shore, or will they draw it back like the shingle which is carried out by the returning wave? Or will they make it oscillate about a fixed position without any advance or recession on the whole?

We have no intention of going through the calculations necessary to solve this problem. They are not contained in this essay, and Professor Challis admits that he has been unable to determine the absolute amount of the constant term which indicates the permanent effect of the waves on an atom. This is unfortunate, as it gives us no immediate prospect of making those numerical comparisons with observed facts which are necessary for the verification of the theory. Let us, however, suppose this purely mathematical difficulty surmounted, and let us admit with Professor Challis that if the wave-length of the undulations is very small compared with the diameter of the atom, the atom will be urged in the direction of wave-propagation, or in other words *repelled* from the origin of the waves. If on the other hand the wave-length is very great compared with the diameter of the atom, the atom will be urged in the direction opposite to that in which the waves travel, that is, it will be *attracted* towards the source of the waves.

The amount of this attraction or repulsion will depend on the mean of the square of the velocity of the periodic motion of the particles of the æther, and since the amplitude of a diverging wave is inversely as the distance from the centre of divergence, the force will be inversely as the square of this distance, according to Newton's law.

We must remember, however, that the problem is only imperfectly solved, as we do not know the absolute value of this force, and we have not yet arrived at an explanation of the fact that the attraction of gravitation is in exact proportion to the mass of the attracted body, whatever be its chemical nature. (See p. 36.)

Admitting these results, and supposing the great ocean of æther to be traversed by waves, these waves impinge on the atoms, and are reflected in the form of diverging waves. These, in their turn, beat other atoms, and cause attraction or repulsion, according as their wave-length is great or small. Thus the waves of shortest period perform the office of repelling atom from atom, and rendering their collision for ever impossible. Other waves, somewhat longer, bind the atoms together in molecular groups. Others contribute to the elasticity of bodies of sensible size, while the long waves are the cause of universal gravitation, holding the planets in their courses, and preserving the most ancient heavens in all their freshness and strength. Then besides the waves of æther, our author contemplates its streams, spiral and otherwise, by which he accounts for electric, magnetic, and galvanic phenomena.

Without pretending to have verified all or any of the calculations on which this theory is based, or to have compared the electric, magnetic, and galvanic phenomena, as described in the Essay, with those actually observed, we may venture to make a few remarks upon the theory of action at a distance here put forth.

The explanation of any action between distant bodies by means of a clearly-conceivable process going on in the intervening medium is an achievement of the highest scientific value. Of all such actions, that of gravitation is the most universal and the most mysterious. Whatever theory of the constitution of bodies holds out a prospect of the ultimate explanation of the process by which gravitation is effected, men of science will be found ready to devote the whole remainder of their lives to the development of that theory.

The only theory hitherto put forth as a dynamical theory of gravitation is that of Lesage, who adopts the Lucretian theory of atoms and void.

Gravitation on this theory is accounted for by the impact of atoms of incalculable minuteness, which are flying through the heavens with inconceivable velocity and in every possible direction. These "ultramundane corpuscules" falling on a solitary heavenly body would strike it on every side with equal impetus, and would have no effect upon it in the way of resultant force. If, however, another heavenly body were in existence, each would screen the other from a portion of the corpuscular bombardment, and the two bodies would be attracted to each other. The merits and the defects of this theory have been recently pointed out by Sir W. Thomson. If the corpuscules are perfectly elastic one body cannot protect the other from the storm, for it will reflect exactly as many corpuscules as it intercepts. If they are inelastic, as Lesage supposes, what becomes of them after collision? Why are not bodies always growing by the perpetual accumulation of them? How do they get swept away? and what becomes of their energy? Why do they not volatilise the earth in a few minutes? I shall not enter on Sir W. Thomson's improvement of this theory, as it involves a different kind of hydro-dynamics from that cultivated in the Essay, but in whatever way we regard Lesage's theory, the cause of gravitation in the universe can be represented only as depending on an ever fresh supply of something *from without*.

Though Prof. Challis has not, as far as we can see, stated in what manner his æthereal waves are originally produced, it would seem that on his theory also the primary waves, by whose action the waves diverging from the atoms are generated, must themselves be propagated from somewhere *outside* the world of stars.

On either theory, therefore, the universe is not even temporarily automatic, but must be fed from moment to moment by an agency external to itself.

If the corpuscules of the one theory, or the æthereal waves of the other, were from any cause to be supplied at a different rate, the value of every force in the universe would suffer change.

On both theories, too, the preservation of the universe is effected only by the unceasing expenditure of enormous quantities of work, so that the conservation of energy in physical operations, which has been the subject of so many measurements, and the study of which has led to so many discoveries, is apparent only, and is merely a kind of "moveable equilibrium" between supply and destruction.

It may seem a sort of anticlimax to descend from these highest heavens of invention down to the "equations of condition" of fluid motion. But it would not be right to pass by the fact that the fluids treated of in this Essay are not in all respects similar to those met with elsewhere. In all their motions they obey a law, which our author was the first to lay down, in addition—or perhaps in some cases in opposition—to those prescribed for them by Lagrange, Poisson, &c.

It is true that a perfect fluid, originally at rest, and afterwards acted on only by such forces as occur in nature, will freely obey this law, and that not only in the form laid down by Prof. Challis, in which its rigour is partially relaxed by the introduction of an arbitrary factor, but in its original severe simplicity, as the condition of the existence of a velocity-potential.

But, on the one hand, problems in which the motion is assumed to violate this condition have been solved by Helmholtz and Sir W. Thomson, who tell us what the fluid will then do; and, on the other hand, Professor Challis's fluid is able, in virtue of the new equation, to transmit plane waves consisting of transverse displacements. As this is what takes place in the luminiferous æther, other physicists refuse to regard that æther as a fluid, because, according to their definition, the action between any contiguous portions of a fluid is entirely normal to the surface which separates them.

It is not necessary, however, for us to say any more on this subject, as the Essay before us does not contain, in an explicit form, the equation referred to, but is devoted rather to the exposition of those wider theories of the constitution of matter and the phenomena of nature, some of which we have endeavoured to describe.

HENSLEY'S "SCHOLAR'S ARITHMETIC"

The Scholar's Arithmetic. By Lewis Hensley, M.A. (Clarendon Press Series, 1873.)

THERE is scarcely any subject more carelessly taught than arithmetic; and, if one would wish to ascertain the reason of this, he has merely to glance at the text-books which have been hitherto most commonly employed. Lately, however, several books of some worth have been presented to the public, and for these we are indebted in a great measure to the late Prof. De Morgan, whose "Elements of Arithmetic," published so far back as 1830, is still regarded as the very best handbook for advanced students. It has, nevertheless, some peculiarities—we cannot call them defects—which have prevented schools from adopting it up to the present time as a text-book; it presupposes too much special talent on the part of the teacher, and contains but few of the modern methods of calculation.

Two main points should ever be kept in view in teaching a subject like arithmetic: first, its principles; secondly, the application of these principles to the affairs of life. In our opinion, the former is undoubtedly the more important if the subject be regarded as an instrument of education. For arithmetical principles are, if properly explained, so very readily comprehended, that a beginner is not likely to find a more delightful path along which he may proceed to the extensive domains of mathematics; but, being generally regarded as a mere catalogue of empirical rules and as a means for exercising the memory, arithmetic becomes, not educational, but instructive, an act of drudgery, and of no more real assistance as a branch of education than needlework or spelling. Explain the ordinary system of numeration to a pupil, let him thoroughly understand the meaning of digit-value and of grade-value, and he will then require but little deep thought, though it will be excellent mental training, to find out for himself the reasons of the four simple rules with respect both to integers *and decimals*. Or, in some cases, let him construct a rule for himself. We do not remember to have ever seen what could fairly be called an arithmetical rider; ordinary problems are not riders, for they are scarcely more difficult than a geometrical theorem with the position and letters of the figure altered. The teacher would occasionally be called in to assist at these exercises; but assistance sought for is far more valuable than that which is spontaneously proffered, and its effect more lasting. Mr. Hensley's "Scholar's Arithmetic" is one of the very few books in which we find decimals discussed in their proper place; indeed it is difficult to understand how this branch of the subject can be logically postponed till a later period if our system of numeration is rationally explained, as of course it should be, at the very commencement of the course.

Pursuing the subject systematically, the pupil should be introduced next to other systems of numeration; and should have at least a little practice in such complex contrivances as long measure and troy weight. Certainly the contrast would be abundantly sufficient to mould any young rational being into a most ardent advocate of the metric system. But we cannot say that Mr. Hensley brings out so strongly as perhaps he might the vast difference between the two methods; his chapter on decimals, treating as it does of conceptions and quantities almost unknown to the great majority of British pupils, is somewhat too abstract. Yet we are glad to recognise in him an outspoken adherent of a universal decimal system, and he seems to look with becoming contempt upon our insular stupidity in fondly cherishing our marvellous weights and measures.

Fractions and proportion are the only other important branches of elementary arithmetic; and, when these are mastered, not only is the attainment of a first-rate knowledge of mental and commercial arithmetic a matter that requires merely time and practice, but algebra becomes thereby most highly attractive as a now comprehensible generalisation, and geometry more alluring even to the unmathematical pupil.

The above-mentioned fundamental divisions and their applications to business, the reader will find fully and ably discussed in the "Scholar's Arithmetic"; and Mr. Hensley has wisely interspersed these all-essential chapters with a few on short methods of calculation, processes of verification, engaging problems, and other similar topics which usually attract the attention and excite the interest of a thoughtful student. There is a short though very lucid chapter on involution and evolution; but, as Mr. Hensley remarks in his preface, he has intentionally passed over subjects which are most easily explained algebraically. There are also more than thirty pages of examination papers from various sources, over and above the numerous examples scattered through the book, as well as a short though sufficient index and glossary. The book is perhaps rather too bulky, and in parts very unequal as regards the difficulty of adjacent sections; but these are trivial failings which will not interfere with its use in schools, and we feel no hesitation in pronouncing it to be one of the most attractive educational works that have appeared on this subject, and it will doubtless be of very great assistance to every earnest teacher.

TEMPLE ORME

OUR BOOK SHELF

The Human Mind; A System of Mental Philosophy for the General Reader. By James G. Murphy, LL.D., Author of Commentaries on Genesis, Exodus, and Leviticus. (Belfast: William Mullan.)

THIS book shows that its author possesses at least one common characteristic of mental philosophers, namely, an inordinately good opinion of his own ability. And, lest the reader should not discover for himself what Prof. Murphy has actually done in psychology (which might happen), he is explicitly told in the preface that, while building on the foundations laid by Reid and Hamilton, Prof. Murphy has, in his own opinion, produced a work which he can venture to submit to "the mental philosopher, as a somewhat nearer approach to the real character of the mind than that of Reid, the founder, or even Hamilton, the lucid and eloquent expositor and defender, of the true system of mental philosophy." Another recommendation put forward in the preface is that this treatise is "among the briefest of those that have gone over the whole field of the mind. Perhaps, we cannot tell. But to us the marvel is that the book should have ever come to an end. We have made several honest attempts to read portions of the respectable looking volume, but have never been able to get beyond a few sentences; for we felt as if launched on a shoreless ocean where we might sail on and on, or round and round for ever, and we could not keep our eyes open on the prospectless outlook. We fear some of the mental philosophers, to whom the book is submitted, will not give it very earnest consideration. What seems to be a main object with Prof. Murphy, and which is, as it appears to us, rather inconsistent with a scientific treatment of the phenomena of mind, is to establish the existence and discover the attributes of Deity. But there are few readers, we should think, who will find much interest or pleasure in his mode of handling this part of his theme. There is not a little of the irreverent jargon with which metaphysical theologians have so often shocked all truly religious people. Here, for example, is a reflection that ought perhaps to leave no doubt as to the honesty of the Almighty, whatever other effect it may have on a religious mind: "He is the Creator of all actual things, which are therefore already His own by an absolute and indefeasible right. He has therefore no temptation to take that which does not belong to Him."

S.

Second Report of the Winchester College Natural History Society. Second and Third Years. (Winchester: J. Wells, 1873.)

THIS Report contains a record of the doings of the Society from May 1871 to February 1873, and is thoroughly encouraging, and certainly a great contrast to the Reports of the two other public schools recently noticed in these pages. The Winchester Report proves that, by judicious management, a School Natural History Society may be made to yield most gratifying results.

The present Report for the two last years, although its record of the earlier papers is incomplete, shows that the Society has been fully alive, and has been growing quietly and steadily, and doing real and satisfactory work. The numbers of the Society are not at this time actually full. But it appears that the elder half of the members are nearly all of them real workers, and it is hoped that the younger half are learning to be the same. It is of more consequence, as the Preface rightly says, that those who belong to the Society should be working members, than that its numbers should be swelled by names. The meetings of the Society have been well attended, and there has been apparent an increasing appreciation of the opportunities afforded by the meetings for showing and seeing objects of interest, as well as for reading and hearing papers.

It is satisfactory to see that old members take an interest in the society after leaving school, several of them contributing valuable papers.

In general, however, we are extremely glad to see, the papers have been those of actual members, and the Society may well feel satisfaction at the increasing readiness, ability, and completeness shown by the leading members in supplying papers at its meetings. The papers which have been read by the Secretaries, Hall, Goddard, and Forbes, may, perhaps, the Preface says, and we think justly, be specially remarked, as combining ability with knowledge based upon personal observation. It is in these papers that the growth of the Society's work has been chiefly seen, and in which its main value consists.

The collections belonging to the School have been considerably increased. The cabinets attached to the Moberly Library now contain about 4,000 specimens, and more are waiting to be mounted and added.

Among the "desiderata" the Preface mentions the following, in case old Wykehamists, or other friends of the School, may be able and willing to supply them:—In Entomology, specimens of Notodontidæ and Pyralides, amongst Lepidoptera; and of any other orders than Lepidoptera. In Conchology, recent Brachiopoda, and Pteropoda. In Geology, Fossils from any of the Primary Formations, Wealden Beds, Red Crag, and Coralline Crag.

The Report contains a number of very interesting papers, mostly by Messrs. E. H. Goddard, W. A. Forbes, and C. S. Rayner, evidently three of the most industrious members of the Society: all the papers are evidence of original observation and independent thought on the part of the writers. The first-mentioned contributes the following papers:—"Hymenoptera," "Botany and Entomology" (in which the localities in the district are indicated in which the collector will reap the best harvest of flowers and butterflies), and one on "Gall Insects." Mr. W. A. Forbes contributes papers on "Coleoptera," "British Reptiles," and "Mimicry and Protective Resemblance." Mr. Raynor contributes a useful paper "On the Different Methods of obtaining Lepidoptera," and a very careful and interesting one "On the Different Modes of Concealment and Defence practised by Insects." The Report also contains a paper on "The Diamond Fields of South Africa," sent by Mr. E. A. Hall. Appended are very full and carefully compiled Botanical, Entomological, and Geological Lists. We hope the next Report will contain a list of the local Fauna, which it is proposed to form.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Perception and Instinct in the Lower Animals

I HAVE waited some time in the expectation that some of your readers would have asked Mr. Wallace a very obvious question with regard to the incident he adduces of a dog finding his master five months after having been lost, and in a house which the latter "had not contemplated going to or even seen before the loss of the dog." (NATURE, vol. viii. p. 66.) In seeking to account for this thoroughly authentic and highly remarkable case, Mr. Wallace observes: "Could it have obtained information from other dogs . . . .? Could the odour of persons and furniture linger two months in the streets? These are almost the only conceivable sources of information; for the most thorough-going advocates for a "sense of direction" will hardly maintain that it could enable a dog to go straight to his master, wherever he might happen to be." Now, there is yet a third supposition open to us, and it is one which, in the absence of information, is certainly the most probable. Can Mr. Wallace's friend remember whether he had been walking in the vicinity of his new house during the day upon which the dog returned? *i.e.* can he be sure the dog did not trace his footsteps? That a keen-scented terrier is able to distinguish and to follow his master's track in a public thoroughfare, however densely it may be crowded, I know from the success of searching experiments.

With regard to dogs communicating information to one another, I may mention that I have often observed them doing so. According to my experience, the dogs must be much above the average in intelligence, and the gesture they invariably employ is a contact of heads with a motion between a rub and a butt. It is quite different from anything that occurs in play, and is always followed by some definite course of action. I must add, however, that although the information thus conveyed is always definite, I have never known a case in which it was complex—anything like asking or telling the way being, I believe, quite out of the question; so far, at least, as this action is concerned. One example will suffice. A Skye terrier (not quite pure) was asleep in the room where I was, while his son lay upon a wall which separates the lawn from the high road. The young dog, when alone, would never attack a strange one, but was a keen fighter when in company with his father. Upon the present occasion a large mongrel passed along the road, and, shortly afterwards, the old dog awoke and went sleepily down stairs. When he arrived upon the door-step his son ran up to him and made the sign just described. His whole manner immediately altered to that of high animation, and, clearing the wall together, the two animals ran down the road as terriers only can when pursuing an enemy. I watched them for a mile and a half, within which distance their speed never abated, although the object of their pursuit had not, from the first, been in sight.

As the instinct question seems to have come to a close it is desirable to observe that the only outcome of its discussion has been to intensify the previous belief in the existence of some unexplained faculty, which may be provisionally termed a sense of direction. Mr. Wallace, in his general reply, avowedly ignores all those cases adduced by your correspondents in which his theory cannot possibly apply; *e.g.* dogs describing the third side of a triangle, or returning by land whence they had been taken by sea. He says: "Several of the writers argue as if I had maintained that in all cases dogs, &c. find their way, wholly or mainly, by smell; whereas I strictly limited it to the case in which their other senses could not be used" (vol. viii. p. 65). Now, whether or not Mr. Wallace originally intended his letter to raise the general issue as to the presence in dogs of a sense of direction, this has certainly been its effect, so that the instances he here refers to are not in any way beside the question which immediately arose. I have much too high an esteem for Mr. Wallace to say anything that might lead to a discussion with him, but it is evident that these remarks have no such tendency; for, if he admits, as he candidly does in the sentence just quoted, that his theory cannot apply to all cases, it necessarily follows that, even could he prove it to be true in some, the fact, although of considerable psychological interest, would leave the question as to a sense of direction just where it was before.

It should be borne in mind that dogs are not the only animals in which this sense appears to be present. It is popularly believed to occur in members of at least two orders of Insects, viz. white ants and bees, but I am not aware that any authentic cases have been recorded. Horses and cats seem to possess it in a high degree, and sheep must either have wonderful memories, or owe their return, in numerous cases, to the faculty in question. Still more wonderful, if we deny them this faculty, must be the memory of migratory birds, some of which return, after months of absence and over thousands of miles, to the same nests in successive seasons. If we allow them this faculty it is not, from analogy, improbable that migratory mammals and even fishes are likewise endowed with it. The most conspicuous example, however, is perhaps that afforded by carrier pigeons. To take one case: two or three years ago some of these birds were flown from the Crystal Palace to Brussels, and it stands, if I remember correctly, upon the authority of Mr. Tegetmeier, that they arrived within a few minutes of a telegram despatched from the Palace at the moment they were liberated. Now, in this case, even the extravagant supposition sometimes made that carrier pigeons are guided by the sight of their destination is excluded, for, as these birds are not high-flyers, the curvature of the earth between London and Brussels would prevent them from seeing the latter. And, even if we imagine that these particular pigeons occasionally towered to obviate this difficulty, yet the curvature of the intervening clouds would have imposed another quite as effectual.

There is still one important point which has not been noticed during the discussion of this subject. We possess indications that this sense of direction, like other mental capacities, admits of cultivation by exercise, and, indeed, that it may remain altogether latent and useless until thus developed. If these indications represent generalities we have at once an adequate explanation of the apparently capricious manner in which this faculty occurs.* As this communication is already too long, I shall here be brief.

It is, I believe, a recognised doctrine among fanciers that carrier pigeons, however purely bred, must be educated by flying short distances before they can be depended upon for long ones. I remember having myself lost a valuable bird by flying him, for the first time, at a distance of 500 yards from his nest. Although in full view of it he became utterly confused, taking long flights in various directions, and ultimately went straight out to sea.

Here is an analogous case in a mammal:—I kept a terrier, of highly intelligent parentage, enclosed in a yard with high walls from the time of its birth until it was eighteen months old, and then took it out for the first time, along the sea-shore. The experiment elicited several facts of psychological interest, and one of them has bearing upon the present subject. Part of the coast over which we went and returned was rough with large shingle, and the terrier's locomotive power being very limited, it was unable, on the homeward journey, to keep up with my pace. Desiring to see what it would do if left alone, I continued for half a mile, and waited to see it come up. As it did not do so, I returned, and found that the animal had actually reversed its direction and gone fully a quarter of a mile from the place where I had left it. After having been taken out short distances seven or eight times, it was inadvertently lost in a neighbouring wood. Now, it had only been in the wood once before, yet its appreciation of direction had made so great an advance that it returned an hour afterwards. As this terrier never evinced any disposition to track footsteps, I do not think its return was due to scent. Anyhow, in a few weeks it became an inveterate wanderer, roaming over the country far and wide. GEORGE J. ROMANES

Dunskaith, Ross-shire, July 7

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* In connection with these points compare the suggestive remarks of Mr. Darwin, contained in the two concluding paragraphs of his article on Instinct (NATURE, vol. vii. p. 418).

Comte on the Survival of the Fittest

MR. JEVONS called attention some time ago to the desirability of preparing a list of past thinkers and writers who have held, in strength or weakness, the doctrines of Darwin and Spencer. Mr. Darwin has himself named a few of those authors, and Prof. Haeckel has extended the number. Recent communications in NATURE show that the list is as yet incomplete. In reading Comte's "Cours de Philosophie Positive" a few years ago, I was impressed with the general similarity of certain doctrines therein stated with some of Darwin's theories. Referring recently to the 42nd lesson of that course (t. iii.)—"Considerations générales sur la philosophie biotaxique," I find that Comte, in reviewing the Lamarck-Cuvier controversy, says:—

"Toute la célèbre argumentation de Lamarck reposait finalement sur la combinaison générale de ces deux principes incontestable, mais jusqu'ici trop mal circonscrits: 1°, l'aptitude essentielle d'un organisme quelconque, et surtout d'un organisme animal, à se modifier conformément aux circonstances extérieures où il est placé, et qui sollicitent l'exercise predominant de tel organe spécial, correspondant à telle faculte devenue plus nécessaire; 2°, la tendance, non moins certaine, á fixer dans les races, par la seule transmission héréditaire, les modifications d'abord directes et individuelles, de manière à les augmenter graduellement á chaque génération nouvelle, si l'action du milieu ambiant persévère identiquement. On conçoit sans peine, en effet, que, si cette double propriété pouvait être admise d'une manière rigoureusement indéfinie, tous les organismes pourraient être enviságés comme ayant être, à la longue, successivement produits les uns par les autres, du moins en disposant de la nature, de l'intensité, et de la durée des influences extérieures avec cette prodigalite illimitée qui en coûtant aucun effort à la naive imagination de Lamarck." (1st ed. "Cours de Philosophie Positive," t. iii. pp. 560 and 561.)

Modification and heredity are here strongly asserted, and the conditions of unlimited transformation as strongly sketched. In continuance of the same argument, Comte, on p. 563, objects to Lamarck's hypothesis, of which he thought very highly as a logical effort:—

"Qu'il repose, ce me semble, sur une notion profondément erronée de la nature générale de l'organisme vivant. Sans doute, chaque organisme determiné est en relation nécessaire avec une système egalement déterminé de circonstances extérieures, comme je l'ai établi dans la quarantième leçon. Mais il n'en résulte nullement que la première de ces deux forces co-relatives ait dû être produite par la seconde, pas plus qu'elle n'a pu la produire: il s'agit seulement d'un équilibre mutuel entre deux puissances hétérogènes et indépendantes. Si l'on conçoit que tous les organismes possibles soient successivement placés, pendant un temps convenable, dans tous les milieux imaginables, la plupart de ces organismes finiront, de toute nécessité, par disparaître, pour ne laisser subsister, que ceux qui pouvaient satisfaire aux lois générales de cet équilibre fondemental: *c'est probablement d'après une suite d'éliminations analogues que l'harmonie biologique a dû s'établir peu à peu sur notre planète, où nous la voyons encore, en effet, se modifier sans cesse d'une manière semblable.* Or, la notion d'un tel équilibre général deviendrait inintelligible et même contradictoire, si l'organisme était supposé modifiable à l'infini sous l'influence suprême du milieu ambiant, sans avoir aucune impulsion propre et indestructible."

The struggle for existence and the survival of the fittest or natural selection are here acknowledged. What is more, the fact that the eliminations due to unfitness for the environment or medium have produced and is producing biological harmony, is pointed out. I have not met with any passages outside of the writings of the new school, which are more explicit than these, though it must not be understood that their author was a transformationist. The preface to the volume in which this occurs is dated "Paris, le 24 Février, 1838." In his general table appended to the sixth volume of his work, Comte says that the Leçon from which these extracts are taken was written between the 9th and 15th of August, 1836. J. D. BELL

New York

The Glacial Period

CAN you inform me if anyone has suggested the following explanation of the existence of the glacial period? And is the explanation I am about to offer a possible one? I put the question in all diffidence, for I have not carefully studied the theory of heat: you must therefore regard any utterance of mine on the subject as merely "a random arrow from the brain." Well, then, it seems to me that the quantity of heat given out in a unit of time from a unit of surface of an intensely heated globe, such as the sun, does not follow the law of radiation of bodies moderately heated. What I mean is this:—It is quite possible that at a time when the sun's mean temperature was higher than it is now, his rate of radiation might have been less; the quantity of heat emitted by him in a unit of time less than it is now. For since his chromosphere must have been thicker, and his solid or fluid nucleus somewhat less in diameter, I suppose that the radiation of the nucleus must have been more retarded by the chromosphere than is at present the case. It is true, that owing to the increased pressure at the surface of the nucleus due to a thicker chromosphere, the temperature there may have been a little higher; but I do not think that difference would make up for the increase in absorption of the chromosphere.

Assuming then that the sun gives out more heat now in a given time than he did during the glacial period, and that the earth had already so far cooled down that her surface was not sensibly more warmed by internal heat than it is in our own epoch, the mean temperature of the earth's climate would have been lower, and the sea-level line of perpetual snow nearer the Equator in both hemispheres; and glaciers would have covered vast tracks of country which are now denuded of them.

Again, let us go back some millions of years in the world's history, till we arrive at the carboniferous period. The sun then would probably be emitting less heat than even during the glacial period; but the earth would not have cooled down to such an extent, and her internal heat would be sensible at the surface. The mean climate of the globe would have probably been warmer then than it is now, and the temperature more equally distributed, depending not so much on solar as on terrestrial radiation. This being supposed, the vegetation of England and India in those days must have presented less difference than what we find at present. Does the flora of our English and Indian coal-beds support or upset this conclusion? Can any of your correspondents answer this query, or set me right if I am wrong in my hypothesis of solar radiation?

Hampstead, July 22 J. H. Röhrs

P.S.—Is there any good mathematical treatise on heat, English or French, up to the latest information on the subject? Can you or any of your correspondents recommend me such a treatise?

Telescope Tube for Celestial Photography

I have not yet seen any satisfactory plan suggested of getting over the difficulty experienced in celestial photography by the expansion and contraction of telescope tubes, by changes of temperature in metal tubes.

I therefore venture to suggest the following plan, which may be so arranged as to keep the object-glass and camera-slide *exactly* the same distance apart, and so keep the true focus when once found. The arrangement would have to be modified according to the metal of which the tube is made, but taking a brass one (the most common), with the camera attached to the eyepiece-slide, the correction will be effected by attaching to the main tube, near the eyepiece, two zinc rods the length of the main tube, upon which they must rest loosely; to the free ends of these, near the object-glass, attach a rod of iron extending to the eye-tube; let this iron rod be attached to the eye-tube when the sensitive-plate is exactly in focus; any change in temperature will then have no effect on the focus, for the expansion and contraction of the three metals will keep the distance from object-glass to sensitive-plate constant. All who have worked with a telescope giving sharp definition, will know that this is not an unnecessary precaution, as it may seem to some.

Sydney Observatory, June 14 H. C. Russell

Colour of the Emerald, etc.

I have to beg "A. H." to refer again to Nature (July 24) p. 254, col. 1, line 23, where he will find it stated that "the emeralds employed were canutillos from Santa Fé de Bogotá. Their specific gravity was 2·69." It is evident, therefore, that they could only be the green silicate of alumina and glucina.

The green sapphire, known also as the "oriental emerald," is the rarest of all gems; and Mr. Harry Emanuel, in his work, "Diamonds and Precious Stones," speaking of it says, "In the whole course of my experience I have only met with one specimen." Its specific gravity would at once distinguish it from the true emerald.

The Beryl A. was colourless, opaque, and had a specific gravity of 2·65. Greville Williams

INSTINCT, PERCEPTION, AND REASONING POWER OF ANIMALS

THE correctness of the following facts, bearing on the above question, I can warrant:—

A beautiful greyhound bitch in my possession had puppies, and I gave one of them, about a month old, to a friend of mine who was also living in Montpellier at that time. Some few days subsequently, on going to call at my friend's house, I took the greyhound with me. She appeared delighted at finding her puppy again, and expressed her strong feeling by lavishing on it, in her own way, the most tender marks of affection. After a few days I paid a second visit to my friend (unaccompanied by the greyhound), when he informed me that, in consequence of the earnest request of one of his friends, he had been induced to give him the puppy, which had thus been removed to a considerable distance. I returned home, and on my arrival was struck with the peculiar manner in which the animal met me. There was nothing of her usual expression of delight—no barking, no jumping to and fro—but she met me with a serious and thoughtful look, and began slowly to smell my clothes in different places, with the most earnest perseverance. Nor was she content with a mere cursory effort to discover the particular object, whatever it was, which, no doubt, she had in view; but she continued the same course of proceeding for at least a quarter of an hour, in fact, till I found it quite necessary to bring it to a close.

From the above statement of the conduct of the animal, the impression on my own mind was that I must have carried away from my friend's house some subtle effluvia, which tended to bring back to the mother the recollection of her puppy. And this caused me some additional surprise, inasmuch as greyhounds are possessed of great keenness of sight, but are generally considered as rather deficient in their power of smelling. The conclusion is still more remarkable. During the space of about two years I usually paid my friend a visit twice a week, and on every occasion, on my return home, the greyhound would invariably go through the same ceremony. At length the proceeding became altogether so striking that it was quite unnecessary for my wife and family (perhaps from a little innocent curiosity) to ask, "Where have you been?" They could save themselves the trouble of a question and say "I see that you have been calling on your friend."

My cousins were residing in a small village about thirty kilom. from Montpellier, and on one occasion, when I was going to spend some days with them, I took, for the first time, my greyhound with me. It so happened that not far off there was a hound bitch that belonged to one of my cousins' neighbours, and between these two animals (from the beginning of my short stay) there arose the deepest hatred and animosity, and conflicts of the most ferocious kind were matters of daily, almost hourly, occurrence. Time altogether failed in producing any better feeling between them, and to the end of my visit each was ever ready and anxious to try their strength whenever the opportunity offered. In the course of the following year I paid a second visit to the same place, accompanied by my greyhound, and about three-quarters of an hour before I reached the village the animal, as if struck with a sudden idea, rushed forward at her full speed, and all attempts to call her back proved quite ineffectual. On reaching the village I found that a terrible encounter had already taken place between the two heroines, who were on the point of renewing the attack after a temporary cessation of hostilities.

The following anecdote relating to the same greyhound seems to prove that these animals may sometimes exhibit a higher standard of reasoning power than according to general opinion they possess.

I was passing some days in the country with my aunt, who had a middle-sized spaniel bitch, of a somewhat sullen and treacherous temper. This spaniel observed, with an evident feeling of jealousy, that my greyhound was making herself quite at home in my aunt's kitchen, and whenever she had a favourable opportunity, without exposing herself to too much danger, she never failed to give an angry bite to her unsuspecting rival, and immediately to rush for shelter under a kneading-trough, from which position my greyhound was unable to dislodge her.

After a short time the spaniel had puppies, and she was placed with two of them in a corn-loft, over the kitchen, from which there was a door which led to it by a flight of stairs; the door was usually kept closed in consequence of the known animosity between the two rivals. For some days the new mother, entirely occupied with the care of her little ones, did not descend to the kitchen, and my greyhound occasionally showed a strong desire to go up to the loft and see what was going on there. When the puppies were about seven or eight days old, their mother began to re-appear in the kitchen, and to observe towards the greyhound the same line of conduct, with the exception only of an appearance of increased hatred. At length, on one occasion, when the spaniel was eating her dinner, and the corn-loft door happened to be partly open, my greyhound, taking advantage of the opportunity, sprang up the stairs of the loft. I observed the circumstance, and on calling her down she immediately obeyed, and made her appearance before me with a look of perfect satisfaction. About an hour afterwards my aunt's husband, on going to the loft, found both the puppies dead, without the least mark of external violence, and he was at a loss to imagine what could have caused their death. For myself I had an impression on my own mind as to the cause of death, but I did not consider it necessary at the time to mention it to others. I opened the bodies of the puppies, and found my opinion confirmed. The skin was externally sound through its elasticity, but the fangs of the greyhound had done their work, and the liver had been bruised into a kind of marmalade—exactly in the same manner as the greyhound usually crushes the liver of the hare or the rabbit, which, literally speaking, are no sooner seized than dead.

In November last, when I was staying with my cousin, I was much interested in observing the proceedings of various kinds of poultry in a field almost contiguous to the house. There were six or seven young guinea fowls, ducks, hens, &c., and also a pair of old guinea fowls, which kept always by themselves, and continued running to and fro with that perpetual restlessness which is natural to them. In the midst, however, of their wildest proceedings they always appeared to keep an eye on the young guinea fowls, and whenever any of the other poultry happened to approach the spot where they were, the old guinea fowls invariably ran with all speed and drove them away. Two large hens alone seemed to be exempt from this rough treatment, and to have full permission to come near the young guinea fowls or not, just as they liked. One of the hens, in particular, seemed to enjoy some special privileges, and in case of any apparent danger, there was some immediate proof of care and protection on the part of the old guinea fowls.

The above circumstances excited my curiosity, and I obtained the following explanation:—

One of these hens had hatched some guinea fowls' eggs, but after three days had neglected to perform the new functions incumbent on her, and had left the young brood to themselves. Fortunately, the other hen, which had previously exhibited the well-known symptoms of the fever of incubation, adopted the deserted young ones, and had nursed them with the greatest affection till they were able to take care of themselves. The old guinea fowls, it appears, had observed all these circumstances, and had retained a grateful recollection of them.

Under the roof of a small tower at my father's house in the country, a large number of sparrows (consulting their own convenience, rather than that of others), had established their nests; but in consequence of the extensive injury caused to the corn-fields by their depredations at harvest-time, my father, with a view to lessen their number, gave direction that all the nests should be removed, and thus, by this wholesale order of destruction, about 80 nests with 366 eggs suddenly disappeared. Their fondest hopes being thus blighted, and the expected fruit of all their labour nipped, as it were, in the bud, the sparrows betook themselves to noisy meetings, and, in their own way, to expressions of anger and resentment. This proceeding continued for at least a week, after which they dispersed, and went in search of some other less dangerous shelter for their future progeny. In the following year some sparrows, which had built their nests under other buildings of our country house, and which had been left unmolested, returned to them; but from that time to the present day (forty-eight years) I can safely affirm that no sparrow has ever rebuilt her nest under the roof of the tower. The singular facts of the case are these: the sparrows decidedly object and decline to build any more nests under the roof of the tower, but they are quite willing to avail themselves of the shelter of the position during the severe nights of the winter season.

Montpellier DR. PALADILHE

THE GROWTH OF SALMON

SINCE the time of Magna Charta it has been an object, directly or indirectly, on the part of the Legislature, to protect the supplies of salmon with which our rivers used to be so abundantly stocked: but notwithstanding the laws which have at various times been enacted, this fish gradually became scarcer till, in 1861, all the old laws were repealed, and fresh and more stringent regulations made for protecting and increasing our salmon supplies. In addition to the fostering care which is bestowed, under the Salmon Fishery Acts of 1861 and 1865, on the fish in the rivers, means have been adopted to artificially rear salmon, so as to increase their numbers more rapidly than could be done in the ordinary course of nature. Mr. Frank Buckland has been the pioneer of this system of artificial breeding of salmon and trout, and the experiments and operations which have been carried on during the last few years have thrown great light on the hitherto unknown habits of this "king of fish."

Anyone who looks into the fishmongers' shops just now can see what a clean, fresh-run salmon, ready for cooking, is like—a silvery, plump creature, whose "lines" are made for speed in water, and whose graceful curves give the completest idea of vigour and strength in stemming a rapid current of water.

But very few people, probably, know what sort of an appearance this beautiful fish presents in its infancy. Hidden away during that period in the upper waters of our salmon rivers, and ultimately in the depths of the sea, it is lost to sight till it grows large enough to be taken by the salmon nets; and until lately very little was known of its natural history, or of its habits, though the experience of the last few years has revealed many interesting facts concerning the development of this fish, through the egg, fry, smolt, and grilse stages, till it becomes a full-grown salmon.

Fig. 1 represents the egg—natural size—of a salmon just laid. Each female salmon carries, on an average, 800 to 900 of such eggs to every pound of her weight. They are generally of a pinky opal colour, elastic to the touch, covered with a soft horny membrane, with a minute opening through which a particle of the spawn—the soft roe—of the male fish enters, and the egg is fertilised. From this moment the young fish gradually develops, under the influence of the cold running water. At the end of about 35 days—more or less according to the temperature, which should be about 40°—two little black specks can be seen, as at Fig. 2, which are the eyes of the embryo fish; the vertebræ may be discerned in the form of a faint red line, and a small red globule which shortly afterwards appears, represents the vital organs of the embryo fish.

At the end of about 80 to 100 days from the deposition of the egg the fish has so increased in size that it bursts the "shell" and makes its *début* in the form represented at Fig. 3. The sac or umbilical vesicle attached to the under part of the fish contains a secretion resembling albumen, which affords nourishment to the infant fish for the first six weeks or so of its existence. By that time it is quite absorbed, and for the first time we see a perfect fish, Fig. 4, with its fins, gills, and scales, which have hitherto been present, but imperceptible except under the microscope, fully formed: and now the young salmon begins to feed. His growth is not very rapid for some months, the lines *a b c* representing the average length of a salmon at 2, 3, and 4 months old. At 2 years old the fish is about 9 to 12 inches long.

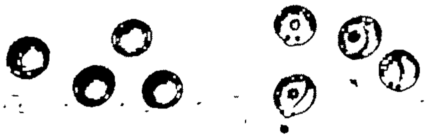
FIG. 1. FIG. 2.

Fig. 1.—New-laid Salmon Egg. Fig. 2.—Egg after about 35 days.

As soon as they are large enough and strong enough, the "smolts," as they are now called, descend to the sea; here they are lost sight of until they return up the river as "grilse." The actual duration of their stay in the sea is not yet known, from one to three years being variously estimated as the probable length of time. The object of this migration to the sea is to find the food which is necessary for the secretion of the fat of the fish, who lives on the *Infusoria*, smaller fish and crustaceans, and the spawn of sea-fish which abound in our seas. The length of their stay in salt-water is regulated, no doubt, by various circumstances, and is not the same in every case. When the salmon has laid up a sufficient supply of fat in its body and on its pyloric appendages, which are a wonderful provision of Nature for the secretion of an amount of fat sufficient to supply it during its sojourn in fresh waters, it ascends the river, its roe or spawn developing as it ascends; till, about Christmas-time, or sometimes earlier, it reaches the shallow headstreams of the river, in the gravelly beds of which it deposits its eggs, returning immediately afterwards to the sea, no longer in the bright, plump, muscular condition in which it ascended, but a lean, lank, ugly, wounded beast, which one would hardly recognise as *Salmo salar*. Fig. 5 represents the head of a "kelt," as those salmon are called which have newly spawned. The curved projection, or hook, on the lower jaw, is a cartilaginous membrane, the use of which nobody knows. The fish is in a very weakly condition, as his fat is gone, and he perhaps assumes this appearance to frighten other animals, which might otherwise be tempted to attack him. The drawing is taken from the photograph of a salmon, weighing 20lb., which was found dead on the banks of one of our Welsh rivers.

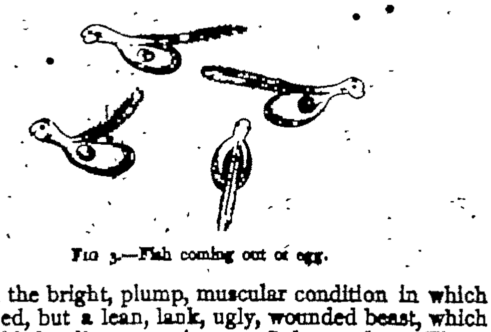
FIG 3.—Fish coming out of egg.

This fish, had it survived, would have returned to sea, recovered its fat, and presently come back worth 2*l.* or 3*l.*, whereas, by dying in this condition, it was worth nothing. It had, however, done its duty by depositing perhaps 16,000 eggs. Only a very small percentage, however, of the eggs laid ever become adult fish. Floods wash them out of their gravel nests; ducks, and other birds, eat them; beetles and various insects attack them; they are smothered with mud, or left high and dry on the shore; the young fish are poisoned by pollutions, or diverted into mill leats and canals, and so lost; trout eat them wholesale; in fact the whole of their earliest existence is a very living death, and it is a wonder, with all the ordeals they have to pass through, that we have any salmon left. To kill them legitimately for food for ourselves is bad enough, and we ought to do all we can to protect them when young.

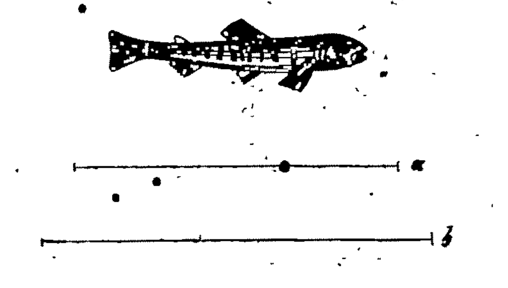

FIG. 4.—Young Salmon six weeks old. *a*, *b*, *c*, size of salmon at two, three, and four months respectively.

In the artificial system of breeding salmon the adult fish are caught just as they are on the spawning beds, and the eggs taken from them; the ova and milt are properly mixed together, and the eggs placed in troughs of water so arranged as to imitate as closely as possible the natural conditions necessary for the development and growth of the fish. Properly managed, 90 per cent. of the eggs will hatch out: the young fish are turned into the river when they are about a year old; if they can be kept two years in tanks large enough, with plenty of running water, so much the better for the prospect of their reaching the sea in safety.

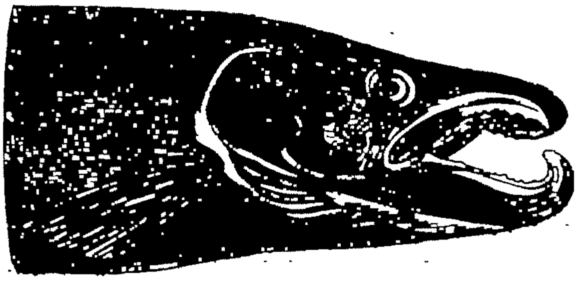
FIG. 5.—Head of a Kelt.

When we can make up our minds to keep all our pollutions out of our rivers, and build "salmon ladders" over all the weirs, so as to give the fish a fair field, and enable them to run up stream unimpeded, then, and then only, shall we see salmon as plentiful throughout the country as it is said to have been in the North a century ago, when apprentices are reputed to have stipulated in their indentures that they should be fed on salmon not more than three days a week. Without this all our efforts to stock our barren rivers with artificially bred fry will prove comparatively unavailing.

C. E. FRYER

THE GLACIAL DRIFTS OF NORTH LONDON

THE landscape memorials of the great glacial period in Britain have hitherto been chiefly looked for by the tourist in the northern and mountainous districts of our island. The vast and wide-spreading products of the same epoch which lie in the lower and more southerly districts of England, as far as the Valley of the Thames, have had to wait longer for their due recognition. In the interval, the Londoner addicted to geologising has been fain to go to Snowdonia, Borrodaile, and the Highlands of Scotland—to the region of perched blocks and terminal moraines—for memorials of the Ice Age within our own coasts. Nor is it to be wondered at that the districts in which glacial action on a grand and cosmical scale was first detected in Britain, and which still afford the more obvious monuments of the glacial period, should so long have monopolised attention. But the time seems now to have come for the drifts of the southern regions to take their proper place in the gallery of glacial phenomena.

So recently have these drifts changed their character in the eyes of geologists that it may be worth while to summarise their history, and indicate the conclusions which have now been arrived at with regard to them as well as one or two important moot points which will perhaps remain doubtful for some time to come.

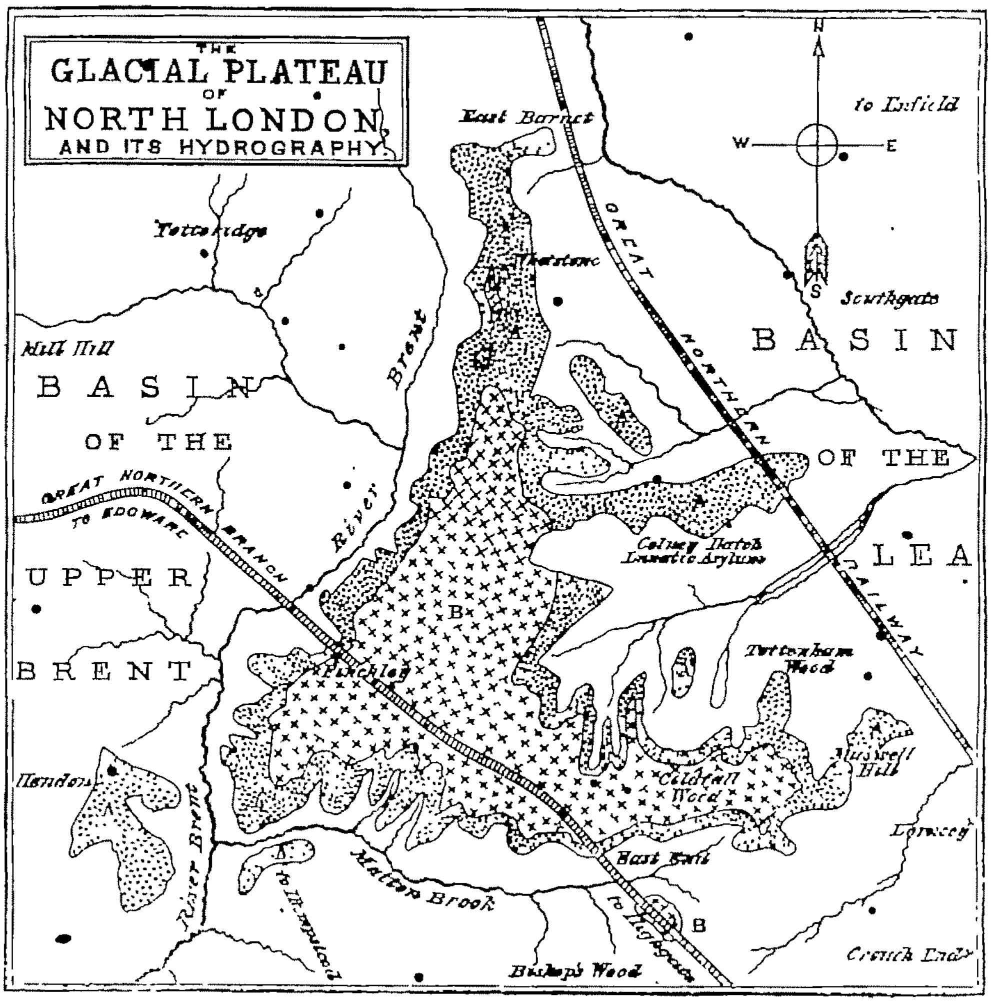

FIG. 1—A, Glacial Sands and Gravels. B, Glacial Clay. Unshaded Parts—London Clay.

It seems only yesterday that the glacial drifts of the lower and southern districts of England were looked upon as a mere congeries of rubbish heaps and "diluvium"—chaotic and unintelligible relics of some mysterious and partly hypothetical period. Now, however, these deposits are no longer slighted by geologists. In the hands of one or two earnest workers—notably Mr. Searles V. Wood, jun.—the glacial clays, and sands, and gravels of England are rising into the dignity of a system. The North London glacial drifts may be taken as typical in most respects of the great and wide-spreading deposits which are found in the inland counties most remote from the homes of the old British glaciers.

The Finchley and Muswell Hill drift lying on the northern heights of London overlooking the Thames Valley occupies a position of great geological interest and significance. Muswell Hill figures in the very early annals of the beds which are known to be of glacial origin. In the year 1835, Mr. N. T. Wetherell, of Highgate, made the discovery which has given such repute to the spot. In Coldfall Wood, just beneath the vegetable soil, Mr. Wetherell found one of those strange medleys which geologists were then wont to dismiss as "diluvium." Here, as far south as the Thames Valley, were water-worn fragments of granite, mountain limestone, coal, red chalk—indeed rock-specimens from all the northern formations, with a similarly heterogeneous collection of fossil remains. Agassiz had not as yet broached the great conception of the glacial period; the diluvium reigned supreme. Year by year more extensive patches of fossiliferous clays and gravels were found adjacent to Muswell Hill. From Finchley and Whetstone an abundance of fossils proper to the chalk and oolite formations was obtained, and whole hampers of belemnites were sent off to Prof. Phillips at Oxford for the purpose of his monograph on that genus. But the drift itself remained an isolated phenomenon. It was left to men of the younger generation to attack a problem as worthy of solution as the problems of Cambria and Siluria.

During the last five or six years, the Finchley and Muswell Hill drift has excited fresh attention. The Great Northern Branch Railway from East End to Finchley has exposed some fine sections, and a body of earnest field-geologists—the Geologists' Association—had been at hand to take advantage of the opportunities thus afforded. In the same period Mr. Wood has published his "Sequence of the Glacial Beds," and the Geological Survey a map of the superficial deposits of the district.

Lying on the hills and plateaux, the North London drifts have a scenic interest. They form noticeable features in the Middlesex landscapes, as may be seen in the accompanying geological map of the district (Fig. 1). The valleys and streams around the plateaux delineate in an instructive manner the extent of the glacial beds, whilst they suggest the action of those meteorological forces which have reduced these beds to their present limits since their elevation above the sea.

But unlike the moraines of Snowdonia and other mountain districts, these much older lower ground accumulations are not, in the view of most English glacialists, the immediate deposits of land ice. Contrary to the beliefs of the Scotch geologists, who would regard them as the equivalents of the Till, they are referred to the era of the great submergence of England beneath the glacial sea. They are the transported material of the submarine terminal moraine. As the ice-foot retired before the submerging sea, it left behind it the *débris* of the rocks it had degraded to be transported by bergs and rafts over the Middlesex of the future.

The glacial deposits at Finchley station, although they conform to the general character of such beds in the south-western counties, have certain features which may prove to be more developed here than elsewhere, and may, at some future time, help to connect these deposits with their more local sources of supply. The preponderance of the characteristic Oxford clay fossil *Gryphæa dilatata* is remarkable, and whilst the chalk and the Oxford clay are the most largely represented, the formations of which the fewest traces are found are the gault and the London clay. Foreign blocks, transported by ice, are generally absent from the district. Blocks of Sarsen sandstone are not uncommon, but it is worthy of notice that they are only found in the drift.

The vast sources of supply for the flint pebbles which abound in the glacial gravels of the district are still represented in the small and local remainders which cap the high ground at Totteridge, and are found at Barnet, where these second-hand accumulations of the Lower or Middle Bagshot, disturbed or redeposited, are free from the quartzites of the glacial gravels, and exhibit an unmixed Eocene lineage.

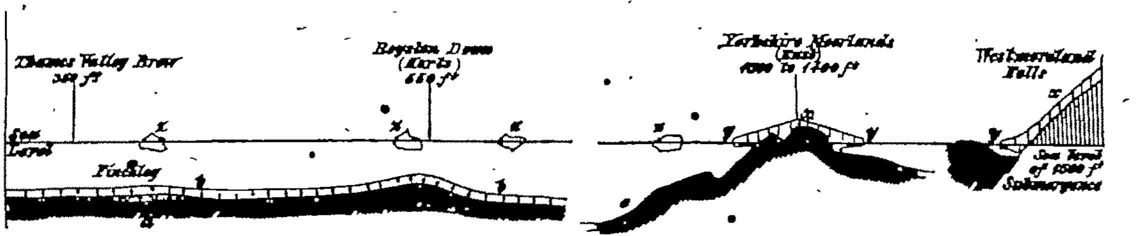

FIG. 2.—Section showing the degree of submergence indicated by the upper Glacial Deposits. References—a, The Middle Glacial. b, The Chalky Boulder Clay. c, The Purple Clay without Chalk. x, The Ice Sheet. y, The Ice-foot. z, Floating Ice.

After years of untold labour, which offer a noble example of private enterprise in the cause of geology, Mr. Searles V. Wood, jun., has established the succession of the glacial beds of the east of England and the central counties, which is here given in abstract:—

Post-glacial Beds

I. UPPER GLACIAL.—1. The purple boulder clay of Yorkshire without chalk. 2. The purple boulder clay with chalk. 3. *The Great Chalky Boulder Clay of the South of England* (*e.g.* at Finchley and Muswell Hill).

II. MIDDLE GLACIAL.—*The Middle Glacial Sands and Pebbly Gravels of the South-East of England and the Central Counties* (*e.g.*, the Finchley and Muswell Hill sands and pebbly gravels).

III. LOWER GLACIAL.—The Contorted Drift of Norfolk, the Cromer Till, and the Pebbly Sands of Norfolk and Suffolk (Upper Crag).

A few words further in explication of this sequence will show how wide an area of England is concerned in the deposits with which the Finchley drifts are thus correlated.

The deposit to which the Finchley chalk boulder clay belongs stretches in an intermittent way from the lower Thames Valley to Central Lincolnshire, and from the Eastern Counties of Norfolk and Suffolk to East Staffordshire. The Finchley sands and gravels extend (mostly covered by the boulder clay) over nearly all the three large counties of Norfolk, Suffolk, and Essex, and are present in Herts, Bucks, and Leicester. So there is no insignificant number of geologists, away from the region of the old glaciers, who may study in their own locality the memorials of the great glacial period in England.

Inasmuch as the maps which Nature has laid down in the ground beneath us are historical as well as physical, this sequence introduces us to a series of consecutive events in the earlier history of the great Glacial Period. In the lower glacial the age of Ice begins. In the next deposit we notice a pause in the Arctic conditions which had prevailed. The formation of that characteristically glacial deposit, the boulder clay, was arrested, and the sands and gravels (middle glacial), of milder waters, took its place. Then the Arctic conditions returned, and brought in the chalky boulder clay. At length the higher rocks were brought within the reach of the sea, until the Yorkshire Wolds were submerged, and eventually the Westmoreland fells yielded their *débris* to be spread over the sea-bottom (Fig 2). That the glacial period should have left its memorials so far south in our island as the valley of the Thames, was a matter of incredulity among many geologists, even so recently as ten years since, when Sir Charles Lyell had compelled attention to the Muswell Hill drift in the "Antiquity of Man." That the drift of the glacial period did not once extend over the counties south of the Thames has yet to be demonstrated, and those geologists who hold that we have already discovered the original southern limits of the glacial clays and gravels in England, have yet to explain the condition of these deposits of the north brow of the Thames Valley, where they are as pelagic in character as they are a hundred miles farther north.

The dwellers in the south of England have thus been compensated for their distance from the bolder region of the old British glaciers, of perched blocks, and terminal moraines. The glacial period has now been brought home, as it were, to their own doors. By the classification of the glacial beds which we now possess, patches of clay and gravel which seemed to have a sporadic and insignificant character are seen to belong to a great and historical series. In the presence of such "diluvium" as that of Muswell Hill, with its astonishing medley of organic remains, it needs no longer to be asked,—

> "What seas receding from what former world
> Consigned these tribes to stony sepulchres?"

We know now that it was an icy sea.

HENRY WALKER

FLIGHT NOT AN ACQUISITION

A FEW weeks ago, when at Ravenscroft (the residence of Lord Amberley), I shut up five unfledged swallows in a small box not much larger than the nest from which they were taken. The little box, which had a wire front, was hung on the wall near the nest, and the young swallows were fed by their parents through the wires. In this confinement, where they could not even extend their wings, they were kept until after they were fully fledged. I was not at Ravenscroft when the birds were liberated, but the following observations were made by Lord and Lady Amberley, who have kindly communicated them to me. On going to set the prisoners free, one was found dead—they were all alive on the previous day. The remaining four were allowed to escape one at a time. Two of these were perceptibly wavering and unsteady in their flight. One of them after a flight of about 90 yards disappeared among some trees; the other, which flew more steadily, made a sweeping circuit in the air after the manner of its kind, and alighted, or attempted to alight, on a branchless stump of a beech; at least it was no more seen. I give the unabridged account of No. 3 and of No. 4 as it stands in the notes made at the time by Lady Amberley. "No. 3 (which was seen on the wing for about half-a-minute), flew near the ground first round Wellingtonia, over to the other side of kitchen garden, past bee-house, back to the lawn, round again, and into a beech tree. No. 4 flew well near the ground, over a hedge twelve feet high to the kitchen garden, through an opening into the beeches, and was last seen close to the ground." The following remarks were added subsequently: "The swallows never flew against anything, nor was there in their avoiding objects any appreciable difference between them and old birds. No. 3 swept round the Wellingtonia, and No. 4 rose over the hedge just as we see the old swallows doing every hour of the day." It remains to add that each of these birds was weighted with a small collar of coloured cloth, put on for the purpose of marking them; and that an old swallow on being set free encumbered by a similar adornment, exhibited the same unsteadiness in its flight.

There is little need to make any remark on the above facts. In proving the flight of birds, and their power of guiding their course through the air in accordance with their sensations of sight, not to be an acquisition, they support the general doctrine that all of what may be called the professional knowledge and skill of the various species of animals come to them by intuition, and not as the results of their individual experiences. With wings there comes to the bird the power to use them. Why, then, should we believe that because the human infant is born without teeth, it should, when they do make their appearance, have to discover their use? The swallow, the first time it is in the air takes care, or rather does not need to take care, not to dash its brains out against a stone wall. Why, then, should we believe man to have no instinctive faculty of interpreting his visual sensations?

DOUGLAS A. SPALDING

BRITISH ARCHÆOLOGICAL INSTITUTE

THE annual meeting of this Institute commenced at Exeter on Tuesday, July 29, the President for the year being the Earl of Devon. Many valuable papers were read, and many interesting excursions made in the neighbourhood; the reception by the Mayor, the local authorities, and the inhabitants generally, has been most enthusiastic. The Congress was brought to a close on Tuesday last, and is declared to have been the most successful meeting of the kind ever held. Of the many valuable papers read we give the following by Dr. E. A. Freeman, on "The Place of Exeter in English History."

He remarked that it sometimes came into the mind of an English traveller in other lands that the cities of his own country must seem of small account in the eyes of a traveller from the land which he visited. He spoke of course as an antiquary and not of modern prosperity and splendour. As a rule an English town did not make the same impression as an artistic and antiquarian object as did towns of Italy, Germany, Burgundy, France, or Aquitaine But whilst we had few cities as rich at once in history and art as many of those on the Continent, yet we need not grieve; for whatever was taken from particular districts was added to the general history of our country. Why was the history of Nuremburg greater than that of Exeter? Simply because the history of England was greater than that of Germany, The domestic history of an English town which had always been content to be a municipality, and had never aspired to be a sovereign commonwealth, seemed tame beside the stirring annals of the free cities of Italy and Germany. But for that especial reason it had a value of its own, it had not struggled for the greatness of its own, but it had done its work as part of a greater whole—it had played its part in building up a nation. And the comparison beween the lowly English municipality and the proud Italian or German commonwealth had also an interest of another kind. The difference between the two was simply the difference implied in the absence of political independence in the one case and its presence in the other, The difference was purely external—the internal constitution—history and revolutions—often presenting the most striking analogies. In both might often be seen the change from democracy to oligarchy, and from oligarchy to democracy. In both they might see men who, in old Greece, would have taken their places as demagogues, perhaps tyrants. Exeter had something to tell of Earl and Countess of Devon; Bristol of its half-citizens, half tyrants, the Lords of Berkeley. In the free cities of the Continent we saw what English cities might have been if the royal power in England had been no stronger than that of the Emperors, and if England had therefore split up into separate states like Germany, Italy, and Gaul. In England the constant tendency had been to unity and to make every local power subordinate to that of the king, and it was this that had made the difference between a municipality like Exeter and a commonwealth like Florence. In Exeter reflections of this kind had a special fitness. No city in England had a history which came so near to that of the great continental cities; none could boast of a longer unbroken existence nor was so direct a link between the earliest and the latest days of the history of our island. None had in all ages more steadily kept the character of a local capital, the undisputed head and centre of a great district. And none had come so near to being something more than a local capital, none had had so fair a chance as Exeter of once becoming an independent commonwealth, the head of a confederation of smaller boroughs, perhaps the mistress of dependent towns and subject districts. It was not then with mere words of form that he might congratulate the Institute on finding themselves at last within the walls of the great city of Western England. They had been to many other places, to York, Lincoln, and Chester, and if Exeter must yield to these in the wealth of actually surviving monuments, it assuredly did not yield to any of them in the historic interest of its long annals. It had, in truth, peculiar interest of its own in which it stood alone amongst the cities of England; she was among cities what Glastonbury was among churches, it was one of the few ties which directly bound the Englishman to the Roman and the Briton. It was the great trophy of that stage of English conquest when our forefathers, weaned from the fierce creed of Woden and Thunder, deemed it enough to conquer and no longer sought to destroy.

Exeter, Isca, Caer Wisc was a city of the same class as Bourges and Chartres. Here was what was found commonly in Gaul but rarely in Britain—the Celtic hill-fort which had grown into the Roman city, which had lived on through Teutonic conquest, and which still, after all changes, kept its place as the undoubted head of its own district. In Wessex such a history was unique; in all Southern England London was the only—and that but an imperfect—parallel. The name carried on the same lesson which was taught by the site. Caer Wisc had never lost its name. It had been Latinised into Isca, Teutonised into Exancester, and cut short into modern Exeter, but through all conquests, through all changes of language, it had proclaimed itself as the city by the Exe. In this respect the continuity of its being had been more perfect than that of most of the cities of Northern Gaul. The name and the site of Exeter at once distinguished it from most of the ordinary classes of English towns. . . .

The first question which now suggests itself was one which he could not answer—when did the city first become a West Saxon possession? When did the British Caer-Wisca, the Roman Isca, pass into the English Exancaster? Of that he could find no date—no trustworthy mension. The first distinct and undoubted mention of the city he could find was in the days of Alfred, where it figured as an English fortress of great importance, more than once taken and retaken by the great king and his Danish enemies. He was as little able to fix the date of the English conquest of Isca as he was to fix that of its original foundation by the Britons. John Shillingford said that Exeter was a walled city before the incarnation of Christ, and though it was not likely to have been a walled city in any sense that would satisfy either a modern or Roman engineer, yet it was likely enough to have been already a fortified port before Cæsar landed in Britain. At all events the first definite mention of it was in the time of the wars of Alfred. But though it was English by allegiance, it was not until two centuries later that it became wholly English in blood and speech. In Athelstan's day the city was still partly Welsh, partly English, each forming a city within a city. To this state of things Athelstan deemed it right to put a stop and to put the supremacy in the chief city of the western peninsula beyond a doubt. Exeter was a port which needed to be strongly fortified, and to be in the hands of none but what were thoroughly trustworthy. The British inhabitants were driven out, and to the confusion of those who say Englishmen could not put stones and mortar together until a hundred and forty years later, the city was encircled by a wall of square stones and strengthened by towers, marking a fourth stage in the history of English fortification. If anyone asked him where the wall of Ethelstan was now he could only say that a later visitor to Exeter took care that there should not be much of it left for them to see. Still there were some small fragments, but suppose not a stone was left, yet as he understood evidence, the fact that a thing was recorded to have been destroyed was one of the best proofs that it once existed. The distinguishing point in this stage of the history of Exeter was this, that it alone of the great cities of Britain did not fall into the hands of the English invaders till after the horrors of conquest had been softened by the influence of Christianity. When Caer Wisc became an English possession there was no fear that any West Saxon prince should deal with it as Ethelfrith had dealt by Deva. When Isca was taken the West Saxons had ceased to be destroyers, and deemed it enough to be conquerors. Thus it was that Exeter stood alone as the one great English city which had lived an unbroken life from pre-English and even from pre-Roman days.

Whatever was the exact date when it became an English possession, it was with the driving out of the Welsh inhabitants under Ethelstan that it became purely English. As such it filled during the whole of the tenth and eleventh centuries a prominent place among the cities of England and a place altogether without a rival among the cities of its own part of the country. Later in the century the fortress by the Exe was the chief bulwark of Western England during the renewed Danish invasions of the reign of Ethelred. It was a spirit-stirring tale to read how the second millennium of the Christian era was ushered in by the record which told how the heathen host sailed up the Exe and strove to break down the wall which guarded the city, how the burghers bore up against every onslaught, and how they withstood the invaders. Exeter was saved, but the unready King had no help or reward for the men who saved it, and the local force of Devon and Somerset had to strive how they could against the full might of the invader, and the devastation of the land around followed at once upon the successful defence of the city. In the next year Exeter became part of the "Morning gift" of the Norman Lady, and Hugh, "The French Churl," as our chroniclers call him, was sent by his foreign mistress to command in an English city, and through his cowardice or treason Sweyn was able to break down and spoil the city. It was not clear whether all the walls were broken down then, but it was quite certain that sixty years afterwards, Exeter was strongly fortified according to the best military art.

After the city's capture by Sweyn nothing more was heard of it during the Danish wars, and the only further knowledge of it between the Danish and Norman invasions consisted of the foundation of the bishopric, and this was accompanied by several circumstances which marked it as an event belonging to an age of transition. It was among the last instances of one set of tendencies, among the earliest instances of another. The reign of Edward the Confessor was the last time (excepting the reign of Edward the Sixth) when two English bishoprics were joined together without a new one being formed to keep up the number. It had happened more than once in earlier times; it happened twice under Edward when the bishoprics of Devonshire and Cornwall were united, and those of Dorset and Wiltshire. But this also was the first instance of a movement for bringing into England the continental rule that the bishopric should be placed in the greatest city of the diocese.

The great ecclesiastical change of the eleventh century had carried him on beyond the great time which stood out above all others in the history of Exeter, when they might say that for eighteen days Exeter was England. The tale of the great siege he had told elsewhere in full detail, and he would not tell it again now, but the story of the resistance of the western lands and their capital to the full power of the Conqueror, was one that never ought to pass away from the memories of Englishmen. The bravery of the inhabitants formed a tale which, even in that stirring time, spoke more than any other—save the tale of the great battle itself—to the hearts of all who loved to bear in mind how long and hard a work it was to make England yield to her foreign master. But whilst our hearts beat with those of the defenders of Exeter, yet we saw none the less now that it was for the good of England that Exeter fell. A question was here decided, greater than that whether Harold, Edgar, or William should reign—the question whether England should be one. When Exeter stood forward for one moment to claim the rank of a free, imperial city, and her rulers expressed themselves willing to receive William as an external lord, but refused to admit him within her walls as her immediate sovereign, they saw that the tendency was at work in England by which the kingdom of the continent was split up into loose collections of independent cities and principalities, and the path was opening by which Exeter might have come to be another Lubeck, the head of a Damonian house, another Bern, the mistress of the subject lands of the Western Peninsula. Such a dream might sound wild in our ears, and we might be sure that no such ideas were present in any such definite shape to the minds of the defenders of Exeter. But any such designs were probably just as little known to the minds of those who in any German or Italian city took the first steps in the course by which from a municipality, or less, the city grew into a sovereign commonwealth. Historically, the separate defence of Exeter was simply an instance of the way in which, after Harold was gone, England was conquered bit by bit. York never dreamed of helping Exeter, and Exeter, if it had the wish, had not the power to help York. But it was none the less true when we saw a confederation of western towns, with the great city of the district at their head, suddenly starting into life to check the progress of the Conqueror—we saw that a spirit had been kindled which, had it not been checked at once, might have grown into something, of which those who manned the walls of Exeter assuredly never thought. We could hardly mourn that such a tendency was stopped even by the arm of a foreign conqueror. We could hardly mourn that the greatness of Exeter was not purchased at the cost of the greatness of England. But it was worth while to stop and think how near England once was to running the same course as other lands. From the sacrifice of the general welfare of the whole to the greater brilliance of particular members of the whole, we had been saved by a variety of causes, and not the least of them by the personal character of a series of great kings working in the cause of national unity, from West Saxon Egbert to Norman William. The tendency of the patriotic movements in William's reign was to division; the tendency of William's own rule was to union. The aims of the Exeter patricians could not have been long reconciled with the aims of the sons of Harold, nor could the aims of either have been reconciled for a moment with those of the partisans of the Etheling Edgar, or of the Danish Sweyn. We sympathised with the defenders of Exeter, York, Ely, and Durham, but from the moment England lost the one man of her own sons who was fit to guide her, her best fate in the long run was to pass as an individual kingdom into the hands of the victorious rival.

With the subjection of Exeter by William might fairly be ended the tale of the place of Exeter in English history. It was then settled for Exeter that she was to be an English city—no separate commonwealth—a member of the individual English kingdom, but still a city that was to remain the undisputed head of its own district. Its history from this time was less the history of Exeter than the history of those events in English history that took place at Exeter. It still had its municipal, ecclesiastical, its commercial history, but no longer a separate political being of its own. It was no longer an object to be striven for by men of contending nations, nor something that might be cut off from the English realm either by the success of a foreign conqueror or the independence of its own citizens.

In the other sense of the word, as pointing out those events of English history of which Exeter was the scene, the place of Exeter in English history was one which yielded to that of no other city in the land save London itself. It was with a true instinct that the two men who open the two great eras in local history—English Ethelstan and Norman William—both gave such special heed to the military defences of the city. No city in England had stood more sieges. It stood one, and perhaps two more, before William's own reign was ended—indeed before William had brought the conquest of the whole land to an end by the taking of Chester. The men of Exeter had withstood William as long as he came before them as a foreign invader; when his power was once fully established, when the Castle on the Red Mount held down the city in fetters, they seemed to have had no inclination to join in hopeless insurrections against him. When, a year and a half after the great siege, the Castle was again besieged by West Saxon insurgents, the citizens seemed to have joined the Norman garrison in resisting the attack. According to one account they had already done the like to the sons of Harold and their Irish auxiliaries. The wars of Stephen did not pass without a siege of Exeter, in which king and citizens joined to besiege the rebellious lord of Rougemont, and at last to starve him within the towers of which legend was already beginning to speak as the work of the Cæsars.

To pass to later times, the Tudor era saw two sieges of the city, one at the hands of a pretender to the crown, and another at the hands of the religious insurgents of the further West. Twice again in the wars of the next century Exeter passed from the one side to the other by dint of siege, and at the last she received an invader at whose coming no siege was needed. The entry of William the Deliverer through the Western Gate formed the balance—the contrast—to the entry of William the Conqueror through the Eastern Gate. The city had resisted to the utmost when a foreign invader, under the guise of an English king, came to demand her obedience. But no eighteen days' siege, no blinded hostages, no undermined ramparts were needed when a kinsman and a deliverer came under the guise of a foreign invader. In the army of William of Normandy Englishmen were pressed to complete the conquest of England, but in the army of William of Orange, strangers came to awake her sons to begin the work of her deliverance. In the person of the earlier William the Crown of England passed away for the first time to a king wholly alien in speech and feeling; in the later William it in truth came back to one who was even in mere descent, and yet more fully in his native land and native speech, nearer than all that came between them to the old stock of Hengist and Cedric. The one was the first king who reigned over England purely by the edge of the sword, the other the last king who reigned over England purely, by the choice of the nation. The coming of each of the men who entered Exeter in such opposite characters marked an era in our history. The unwilling greeting which Exeter gave to the one William and the willing greeting which she gave to the other, marked the wide difference in the external aspect of the two revolutions. And yet both revolutions had worked for the same end; the great actors in both were, however unwittingly fellow workers in the same cause. It was no small place in English history which belonged to the city whose name stood out in so marked a way in the tale alike of the revolution of the eleventh and the seventeenth centuries. It was no small matter, as we drew near by the western bridge or the eastern isthmus, as we passed where once stood the eastern and the western gates, as we trod the line of the old Roman streets, to think that we were following the march of the Conqueror and the Deliverer; it was no small matter, as we entered the minster of Leofric, Warlewast, and Grandison, to think that the *Te Deum* was there sung alike for the overthrow of English freedom and for its recovery. It was no mean lesson if we had to connect with the remembrances of this ancient city—among so many associations of British, Roman, and English days—the thought that rose above all the rest, the thought that there was no city in the land whose name marked a greater stage in the history of the Conquest of England, that there was none whose name marked a greater stage in the history of her deliverances.

NOTES

FOREIGN honours have been recently falling in showers on the heads of English scientific men. Not long ago the Emperor of Brazil nominated as Knights of the Imperial Order of the Rose, the following gentlemen:—Sir G. B. Airy, Dr. Warren De La Rue, Dr. Birch, Prof. Abel, Major Moncrieff, Capt. Andrew Noble, and Mr. J. Norman Lockyer. The other day, King Oscar II. of Sweden, at his coronation at Stockholm, marked his appreciation of the services rendered by science by conferring distinctions on several men of learning, both Swedes and foreigners. Among the latter were the following eminent scientific men of this country:—Sir Charles Lyell and Sir George B. Airy, named Commanders of the First Class of the Order of the Polar Star; and Professor John Tyndal, Professor Thomas Henry Huxley, and the Director of the Botanical Gardens at Kew (Dr. Joseph Dalton Hooker), named Knights of the same Order.

WE understand that one of the evening discourses at the meeting of the British Association next month will be delivered by Prof. W. C. Williamson, of Manchester, on "Coal and Coal Plants." It is also hoped that Prof. Clerk-Maxwell will deliver a discourse on "Molecules." Several papers on subjects of local interest have also been promised. The following is a list of the vice-presidents and other officers of the Association, the president-elect, as we have already announced, being Prof. A. W. Williamson, F.R.S.:—Vice-Presidents elect: the Earl of Rosse, F.R.S.; Lord Houghton, F.R.S.; W. E. Forster, M.P.; the Mayor of Bradford; J. P. Gassiot, F.R.S.; Prof. Phillips, F.R.S.; John Hawkshaw, F.R.S. Local Secretaries for the meeting at Bradford: the Rev. J. R. Campbell, D.D.; Mr. R. Goddard; Mr. Piele Thompson. Local Treasurer for the meeting at Bradford: Mr. Alfred Harris, jun. General Secretaries: Capt. Douglas Galton, C.B. R.E. F.R.S., Dr. Michael Foster, F.R.S., Trinity College, Cambridge. Assistant General Secretary: George Griffiths, M.A. General Treasurer: William Spottiswoode, F.R.S. Auditors: John Ball, F.R.S.; J. Gwyn Jeffreys, F.R.S.; Colonel Lane Fox, F.G.S. The sections are the following:—A, Mathematical and Physical Science.—President: Prof. Henry J. Stephen Smith, F.R.S. Vice-Presidents: Prof. Balfour Stewart, F.R.S., and Prof. Henrici. Secretaries: Prof. W. K. Clifford, M.A.; J. W. L. Glaisher, Prof. A. S Herschel, and Prof. Forbes. B, Chemical Science.—President: Dr. W. J. Russell, F.R.S. Vice-Presidents: Prof. Roscoe and I. Lowthian Bell. Secretaries: W. Chandler Roberts, F.C.S.; Dr. Armstrong; and Prof. Thorpe. C, Geology.—President: Prof. Phillips, D.C.L. Vice-President: W. Pengelly Secretaries: Louis C. Miall; William Topley, F.G.S.; R. Tiddeman. D, Biology.—Vice-Presidents: Dr. Beddoe and Prof. Rutherford, M.D. Department of Zoology and Botany.—Secretaries: Prof. Thiselton-Dyer and Prof. Lawson. Department of Anatomy and Physiology.—Secretaries: E. Ray Lankester and Dr. Pye-Smith. Department of Anthropology.—Secretaries: F. W. Rudler, F.G.S., and J. H. Lamprey. E, Geography.—President: Sir Rutherford Alcock. Vice-Presidents: Major-Gen. Sir Henry Rawlinson and John Ball. Secretaries: H. W. Bates, F.R.G.S.; Keith Johnston, F.R.G.S.; and Clements R. Markham, C.B., F.R.S. F, Economic Science and Statistics.—President: Mr. W. E. Forster, M.P. Vice-Presidents: Dr. Farr; Lord Houghton, F.R.S.; E. Baines, M.P. Secretary: J. G. Fitch, M.A. G, Mechanical Science.—President: W. Froude, LL.D. Vice-President: A. Bessemer. Secretaries: H. M. Brunel; J. N. Shoolbred; H. Bauerman.

ON Tuesday the forty-first annual meeting of the British Medical Association was opened in King's College, London, the large hall of which was crowded on the occasion of the general assembly, at 3 o'clock. The General Meeting was presided over by Mr. A. Baker, surgeon to the General Hospital, Birmingham, and president of the Association. After the retiring president had addressed the meeting, Sir W. Fergusson took the chair as president of the Association for the coming year, and read an address of considerable length. It was difficult in the present time, he said, for a president of an association like that to find a suitable subject for an address as, whatever topic he started with he was immediately surrounded with so many specialists, who of course knew everything better than himself, that he did not know where to stand. The president then entered at much length on the subject of the valley of the Thames and the importance of pure water in a hygienic sense. He suggested that, without having recourse to the expensive process of going to the lakes of Cumberland and Westmoreland for a supply of pure water, there were many streams and rivulets and water sheds where the waters could be confined in lake above lake, and utilised for the supply of London and the large towns. In the evening the Lord and Lady Mayoress held a reception at the Mansion House, which was attended principally by medical gentlemen and their wives and daughters. More than 3,000 were received during the evening.

AMONG the distinguished foreigners now attending the meeting of the British Medical Association in London, may be mentioned—Professors Virchow, Oscar Liebreich, and Baron von Langenbeck, of Berlin; Prof. Busch, of Bonn; Prof. Marey, of Paris; Prof, Chauveau, of Lyons: Prof. Spiegelberg, of Breslau; Prof. Lazarewitch, of Charkow; and Dr. Fordyce Barker, of New York.

ON Monday, the annual meeting of the Cambrian Archæological Association was opened at Knighton, Radnorshire. This Association was established some thirty years ago for the purpose of investigating and preserving the objects of antiquity which abound in the Principality. The first Congress was held at Aberystwith, and the present is the 28th of the series. The President for the past year was Sir J. Russell Bailey, M.P., and the President-elect is the Hon. A. Walsh. The week's programme opened on Tuesday night with the annual meeting and reception of report, after which the President for the year, Sir J. R. Bailey, was to resign the chair to his successor, the Hon. A. Walsh, who was to deliver the inaugural address. The rest of the week will be occupied with excursions, and meetings for the reading of papers.

MR. G. KITCHENER has been elected to the headmastership of the High School, Newcastle-under-Lyne, Staffordshire, in the middle of the Potteries. It is to be the first "First Grade" established as a semi-classical school (*i.e.* without Greek in the ordinary school course). The time thus made available, will enable more attention to be given to Mathematics and Science. The scheme directs that Chemistry and Design should be specially taught, with a view to the Potter's art. The school is to be opened in the spring of 1875.

It has long been familiar to geologists that the western and southern coast-line of Scotland is pierced with caves at different levels, indicating former successive levels at which the sea waves worked. Unfortunately, owing to the want of limestone or very calcareous rocks, these caves as a rule present none of that stalagmite deposit which has elsewhere served so abundantly to cover over and preserve the remains of the ancient denizens of our country with traces of the presence of man himself. The caves usually open directly upon the coast, with free exposure to the air, so that their floors show nothing but damp boulders and pools of water from the drip of the roof. Recently, however, a remarkable exception to these ordinary conditions has been observed on the wild cliff line to the south-west of the bay of Kirkcudbright; the Silurian greywacke is there traversed with strings and veins of calcite along lines of joint and fracture, and at one point where an old sea cave occurs, the walls and floor at the cave mouth, and for a few yards inwards, have a coating of solid calcareous matter. Beneath this coating in the substance of the breccia, which extends across the cave mouth, as well as throughout the cave earth behind the breccia, a great quantity of bones, with traces of human occupation, has been found. A systematic investigation of the cave, commenced last autumn, is being carried on under the direction of Mr. A. J. Corrie and Mr. W. Bruce-Clarke, the discoverers of the osseous layer. At the present time the following, among other remains, have been noted: bones of ox, red-deer, goat, horse, pig, pine-marten, rabbit, watermole, and other small rodents, together with numerous remains of birds, and a few frog and fish bones. Intermingled with these occur fragments of bronze, bone needles, and other bone implements, to the number of more than twenty. One piece of worked stone (a fragment of greywacke) has been found, but as yet not a single chip of flint. A full account of the cave will be published as soon as the investigations are completed.

A conference of the City Companies, under the presidency of his Royal Highness the Prince of Wales, was held at Marlborough House, on Monday, July 21, with the view of discussing how technical education might be promoted by those companies acting in concert with the International Exhibition. It was unanimously agreed that the City Companies should give their best support to the object which the meeting had in view, and Mr. Cole, C.B., explained that the Commissioners had determined that, during the months of August, September, and October, schools should be admitted to the Exhibition by ticket, at three-pence each scholar, and that, during the month of August at least, frequent lectures each day would be given on the various subjects and processes exhibited. He suggested that the City Companies, in addition to sending their own schools to attend these lectures, might purchase tickets, and place them at the disposal of the London School Board, to enable them to award them as prizes. Such tickets might also be distributed among other public schools.

At a meeting held at Grosvenor House on July 17, a Provisional and an Executive Committee were formed for the establishment of a National Training School for Cookery in connection with South Kensington. The Committee of Management of the Lectures on Cookery at the International Exhibition have been urged to take this step from the comparatively great success monetarily and otherwise, of these lectures. The meeting agreed that the Executive Committee of the present School for Cookery be an Executive Committee to prepare a scheme and issue the same. The meeting also agreed to the following resolutions:—1. That the establishment of a Training School for Cookery, to be in alliance with the School Boards and Training Schools throughout the country, is most desirable at the present time. 2. That the aim of the proposed school should be to teach the best methods of cooking articles of food in general consumption among all classes. 3. That an Association should be formed with the intention of making the School self-supporting. 4 That it would be prudent to secure a capital, say 5,000*l*., which might be raised by means of donations giving the privilege of nominating students in the School, as well as by means of a guarantee fund; it is estimated that an expenditure of about 1,000*l*. would be required to fit up a practical school or laboratory. The Provisional Committee, containing several Royal and noble names, were authorised to take the necessary measures to establish the school by means of shares, donations, and guarantees. Assuming the necessary capital to be provided—and we hope there will be no difficulty nor delay in doing so—the Executive Committee hope that they may be able, before the end of the year, to establish courses of practical instruction in the kitchen, as well as lectures. Arrangements will be made so that courses may be severally attended by pupil-teachers in training for public education, by domestic servants, and by ladies. The experiment of this school will be first tried in London, and if it succeeds, similar schools will be established in the large towns. We sincerely hope such a laudable scheme will meet with perfect success. All communications on the subject of the school should be addressed to the Secretary (*pro tem.*) of the school, Annual International Exhibitions, Kensington Gore, London, S.W.

Arrangements have been made with Mr. P. Simmonds for the delivery each day of six short lectures on the industries illustrated in this year's International Exhibition. These lectures will be commenced on Saturday next.

On Saturday a deputation from the Trades' Guild of Learning which was formed for the promotion of technical education in the various trades and industries of the United Kingdom, waited upon the Marquis of Ripon at the Privy Council Office, with a view of urging upon the Government the desirability of taking further steps to promote a higher technical education. The deputation included Sir A. Brady, Mr. H. Solly, and others. Sir A. Brady said what the working-men wanted was not money but a fair start. They felt that enough had not been done in utilising the resources of the South Kensington Museum. The Chancellor of the Exchequer had acted very penuriously in the matter. One way in which they could be assisted was by the establishment of a class of instructed teachers and the attaching art schools to the museums. The Rev. Mr. Solly said that the great body of the intelligent artisans, who were largely represented on the council, found that the benefit of the services they received from the Educational Department almost wholly failed to reach themselves. This failure arose principally from the following causes:—First, because the sources of information were not readily accessible as to what the Department really aimed at with a view to assist them. Secondly, the workmen in the East-end of London found the cost of the journey to the South Kensington Museum to be too great in time and money, and therefore they desired that two or three other well-furnished museums should be established in other parts of the metropolis. The next point was that the Department should not only assist classes which had made some progress, but classes in their incipient stages, and which required nursing. The last and most important point of all was, that however able the Government teachers were to instruct in Science and Art, they were not able to give that practical instruction in any trade which the workman might pre-eminently need and desire. The apprenticeship system had practically broken down. The Marquis of Ripon said that if Mr. Solly's paper were sent in it should receive careful consideration.

NOTWITHSTANDING the vast importance of St. Paul's Cathedral and the impossibility of making up for its loss were it destroyed, until recently it was in imminent danger of being shattered by every thunderstorm that passed over it. The lightning-rods that were supplied to it 120 years ago have long been utterly useless, and from its position, size, and certain peculiarities of structure, the noble building formed a tempting object of attraction to the destructive stroke of lightning. Happily, we learn from the *Telegraphic Journal*, this is no longer the case. The authorities, dissatisfied with the electrical state of the building, upon the report of Mr. John Faulkner, Associate of the Society of Telegraph Engineers, of Manchester, commissioned him to prepare a plan for the fitting of the cathedral with an efficient system of conductors. The plan submitted was approved, and the fitting is now completed. In metallic connection with cross and ball and scrolls are eight copper conductors, each being a ½-inch strand of copper wires. The octagonal strand has been adopted as giving most metal in the least space. These eight conductors then pass to the metallic railing of the Golden Gallery, with which they are in metallic connection. Thence they are carried down to the dome, to the metallic surface of which they are again connected at several portions of their length. Then down the rain-falls, over the leaden roofs of the aisles, in the angles formed by the aisles themselves, again down the rain-falls to the sewers. Further, the choir and nave roofs are connected together by a saddle or conductor stretching over them both, and joined to the conductors proceeding from the summit of the west towers. Even this did not satisfy the zealous care of Mr. Faulkner, who tested, sheet by sheet, the electrical condition of the leads, connecting the worse insulated sheets by copper bands to the better conducting surfaces. Thus the dome, aisle-roofs, and ball and cross, and the two west towers, form one immense metallic conductor, and the danger arising from interior gas-piping is removed; for Coulomb and Faraday have proved beyond doubt that electricity accumulates upon the surface only of bodies. In the sewers, which always afford a moist earth connection, the copper strands are riveted to copper plates, and these again are pegged into the earth. By this means as good an earth connection is obtained at the top of the cross, at the very summit of the Cathedral, as is found in the sewers at its base.

THE Examinations in the Crystal Palace School of Practical Engineering, for the Easter term, commenced last Saturday, and will close on Friday, August 9th. The Autumn Term will commence on Monday, September 8. The Principal will attend in the school from 10 till 4 each day, from Saturday, August 2nd, to Friday, the 8th, to pass Candidates for admission.

AN earthquake occurred at Jamaica at 0·30 A.M. on July 1, which created much alarm. It lasted nearly five seconds.

AMONG Mr. Murray's announcements of forthcoming works are—"Personal Recollections, from Early Life to Old Age," by Mary Somerville; "The Geography of India, Ancient and Modern," by Colonel Yule, C.B.; "The Naturalist in Nicaragua," by Thomas Belt, F.G.S.; and a popular edition of Mr. H. W. Bates's "The River Amazons."

A NEW and cheap edition of Mr. James B. Jordan's "Elementary Crystallography" has been published by Mr. Murby as one of his series of science manuals. To any one commencing the study of crystallography this manual will be very useful, especially as appended to the letterpress is a series of carefully drawn nets for the construction of models illustrative of the simple crystalline forms.

THE report of the annual meeting of the Perthshire Society of Natural Science shows that Society to be in a prosperous and good working condition. The number of members is large, and among them is a fair proportion of workers. We are glad to see that excursions have been started, and hope they will be continued; no richer field, we are sure, than the County of Perth, especially for Botany, exists in this country. The Society, under the energetic management of Dr. Buchanan White, of Dunkeld, is publishing a Fauna and Flora of Perthshire, and it is under its auspices the *Scottish Naturalist* is brought out. During the last summer Mr. J. Allen Harper turned out, for the purpose of acclimatisation, about 7,000 living specimens of the following molluscs: *Helix virgata*, *H. pisana*, and *Bulimus acutus*. The annual address of the president, Col. Drummond Hay, occupies the greater part of the report, and gives many interesting details concerning the birds of Perthshire. The Society has entered on the seventh year of its existence.

THE following are the chief additions to the Brighton Aquarium during the past week:—4 Corkwing Wrasse (*Crenilabrus melops*); 7 Pogge (*Agonus cataphractus*); 1,000 Prawns (*Palæmon serratus*); several groups of *Serpula contortuplicata* and *Alcyonium digitatum*. Four young rough-hounds (*Scyllium canicula*) have been hatched from eggs laid during the last week in January. The period of their development in *ovo* is therefore six months. A large number of young Squid (*Loligo vulgaris*) have also been hatched from spawn brought in by fishermen.

THE additions to the Zoological Society's Gardens during the last week include an Ocelot (*Felis pardalis*) from America, presented by Mr. A. B. Keymer and Mr. C. C. Lovesy; a Togue Monkey (*Macacus pileatus*) from Ceylon, presented by the Sergeants of the 1st Batt. Scots Fusiliers; a grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. G. S. Daintry; a starred Tortoise (*Testudo stellata*) from India, presented by Capt. C. S. Sturt; two lesser black-backed Gulls (*Larus fuscus*), presented by Mr. C. W. Wood; two crested Pigeons (*Ocyphaps lophotes*), hatched in the Gardens; a Hoffmann's Sloth (*Cholopus hoffmanni*) from Panama, and a black-eared Marmoset (*Hapale jacchus*) deposited.

METEOROLOGY IN HAVANNA [*]

TO judge from the pamphlets mentioned below, the practical study of Meteorology seems to be pursued at the Cuba Observatory with diligence and a harvest of good results. The care and skill with which they are compiled must lead to the conclusion that science will receive very valuable aid both in meteorological and magnetic research from this station of the West Indies.

The observatory is situated at a height of 19·297 metres above the sea level, in N. lat. 23° 8′ 14″·5, its longitude being 76° 9′ 42″·8 west of San Fernando, and therefore 82° 22′ 6·95 west of Greenwich. The first volume is a yearly meteorological and magnetic report, and consists entirely of monthly tables and curves of the daily mean results of the barometer, thermometer, tension, humidity, wind, evaporation, rain, and state of sky. For each month the daily maximum, minimum, and mean values are given, and then follows a table of the monthly means for every even-hour of the day and night. The direction and force of the wind are shown on a circular diagram, and the mean daily values of the barometer, thermometer, tension, and humidity are exhibited by broken lines. Rain curves are added from May.

Regular two-hourly observations of the Magnetic Declination were commenced on April 1, 1871, and the same details are given as for the barometer, &c. To these were added at the commencement of the following month similar observations of the horizontal magnetic force. For these elements of terrestrial magnetism two-hourly, as well as daily mean, curves are traced for each month.

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[*] Observaciones Magneticas y Meteorologicas del real Colegio de Belen de la compania de Jesus en la Habana, de 30 Nov 1870 a 30 Nov. 1871.
Memoria de la Marcha regular o periodica, et irregular, del Barometro en la Habana des de 1858 a 1871 inclusivo, por el R. P. B. Vines, S.J.
Observaciones correspondientes al mes de Octubre de 1870, con la descripcion de los huracanes ocurridos en la Isla en dicho mes.

In the general table, that closes the report, we notice that the prevailing wind never deviates, in any season, more than 13° 31′ from the east, and in spring it is only 3° 36′ N. of E. The rainfall for the seasons given in millimetres was in winter, 71·1; in spring, 181·0; in summer, 480·0; and in autumn, 547·2; the number of rainy days being respectively 13, 15, 33, and 39.

The coincidence of magnetic disturbances with local storms, with hurricanes in Florida and St. Thomas, with Auroras visible in distant lands, and with similar magnetic perturbations in England, was remarked in August, September, and November. The frequent disturbances of the needle noted in October certainly do not agree with photographic records in England, this month having been remarkably free from perturbations of this nature.

The second book contains the results of a continued series of barometric observations between the years 1858 and 1871. The reliance we may place on the accuracy of the work can be estimated from the fact, that the correction of 1·07mm. for sea level was the result of 2,000 comparisons.

A very regular double period is apparent in the daily range, which may be represented by the expression $h=k\sin(a+t)+k'\sin(b+2t)$; but the range for the day hours is somewhat in excess of that of the night. The minima occur at 2—4 A.M. and 3—4 P.M., and the maxima at 9—10 A.M. and 10 P.M., the times varying slightly with the seasons.

In December, January, and February, the amplitude of the daily range is greatest, and then gradually decreasing it attains its minimum in June and July. This confirms the law of Ramond, cited by Kaemtz, that the amplitude of the barometric range within the tropics is least in the rainy season. This annual variation of the daily range is, remarks our author, the more worthy of note, as it is directly opposed to what has been observed in Europe, where the range is greatest in summer. This remark appears to me to require some modification, for on turning to the monthly mean range observed, for instance, at Stonyhurst, during the last quarter of a century, I find a perfect agreement with the annual variation for Cuba. The mean values for the several months at Stonyhurst are 1·448, 1·415, 1·378, 1·167, 0·970, 0·896, 0·869, 0·927, 1·217, 1·323, 1·451, 1·449. These means are, it is true, obtained from the extreme monthly maxima and minima, but our author informs us that the amplitude of the extraordinary oscillations, if we eliminate the four greatest which were due to hurricanes, resembles the mean annual variation of the range, being greatest in January and least in July. The mean values of the extraordinary oscillations being almost identical in November, December, February, and March, give this annual daily range curve at Cuba a singular symmetry. The periodic recurrence of summer storms at fixed hours will account for the diminution of the range in that season.

The mean values of the Daily Range have been deduced by several methods: 1. From the absolute maxima and minima, by which the irregular oscillations are not sufficiently eliminated. 2. By Humboldt's method, from the maxima and minima at fixed hours. 3. By Kaemtz's method, from the arithmetical means of the maxima and minima. 4. From Bravais formula, $R=\sqrt{\frac{d^2+d_1^2+\dots d_n^2}{n}}$; d, d_1 . . . being the differences between the monthly mean and those of certain fixed hours. There is a striking agreement in the results from all these methods, but the second shows in certain cases signs of a suspicious irregularity.

Besides the Daily Range, and an annual variation of this range, there exists a yearly variation of the mean value analogous to the diurnal, having its principal maximum and minimum in January and October, and secondary ones in July and May. This double inflexion of the mean annual curve is peculiar to Cuba, since there is generally in the same latitude only a single maximum in January, and a minimum in July.

The abnormal inflexion occurs during the month of June, July, August, and September. Kaemtz, in his "Météorologie," who is followed by Marie Davy, fixed the principal minimum in August, but this and other lesser differences arise probably from not eliminating extraordinary perturbations, and from the confessed imperfection of his series of observations.

The observations of fourteen years are insufficient to determine any certain law respecting the years of hurricanes; but an inspection of the yearly curves shows that 1865 and 1870 are distinguished from the rest by the almost identity of the means for February, March, and April, followed by a rapid rise from May to July, a fall from July to October, and a still more marked rise from October to January.

The third pamphlet gives a very interesting and detailed account of the terrible hurricanes that caused such wide-spread desolation in the October of 1870. The first storm occurred on the 7th and 8th, the second on the 19th and 20th.

The author adopts the theory of cyclones first enunciated by Redfield in 1831, and since accepted and modified by many eminent meteorologists. Situated N. of the Equator he considers the storm to be rotating in the direction from E. to W. through N. and the centre to be at the same time moving N.W. within the tropics, and N.E. in higher latitudes. The resultant path he finds to be a spiral wrapped round a parabola, the folds of the spiral being closest at the apex of the curve. The position of the centre or vortex is given at any moment by the height of the barometer and the direction of the wind. The barometer being lowest at the centre, the reading of the mercurial column, corrected for daily range and for the particular wind, furnishes data for determining the distance of the centre, whilst the angular bearing of the latter is known from its being at right angles to the direction of the wind, and to the rigtht hand of an observer facing the wind. This follows necessarily from the circular motion of the cyclone, and falls, as a particular case, under the general law of Buys Ballot, since we know that the barometer is lowest at the vortex. The latter is thought to move in a cycloidal curve with loops at the cusps, which just fall on the parabolic trajectory. The vortex is thus almost always to the E. of the parabola. The double motion of translation and rotation causes the effects of the hurricane to be much more disastrous in the N. hemisphere to the E. of the parabolic path than on the W. side, and the velocity of the wind at a given distance from the vortex for any points of the compass may be found from the formula $V=\sqrt{t^2+r^2+2tr\cos\theta}$, where t and r are the velocities of translation and rotation, and θ is measured from the E. point when the storm is moving N. The calm at the centre of the cyclone gives a ready means of estimating the velocity of translation. The storm of the 7th was felt from the 4th to the 13th, the maximum humidity lasting till the 12th. The rate at which the vortex crossed the island was only four miles an hour, and this remained almost constant throughout. The second storm was much more sudden and rapid, and the increasing rate, from 9½ to 20 miles an hour, at which the vortex was travelling, showed that the second branch of the parabola had been reached before passing Cuba.

Equal heights of the barometer combined with the directions of the wind enable the meteorologist to lay down the parabolic trajectory with considerable accuracy, either from observations at a single station, or at several. Thus on the 7th at 2 P.M. the corrected barometer read the same at Havannah and at Cienfuegos, the wind being S. by W. at the latter, and N.E. at the former station, the vortex was therefore at that time S.E. of Havannah, and a few degrees N. of W. from Cienfuegos, but equally distant from the two places. The more rapid changes and greater fall of the barometer, together with the increase in the velocity of the wind, show that the storm passed more centrally over Havannah than over Cienfuegos. The discharges of electric fluid were very intense, and at Cardenas an appearance similar to the aurora borealis was visible for ten minutes. The magnetic needles were much disturbed. The inundations from the rising of the sea were very destructive, and on the 7th the existing wind favoured the rise. This rise under the centre of the cyclone seems to follow from the removal of pressure, and the inrush of air of different temperatures fully accounts for the heavy rainfall. The diminution of atmospheric pressure is also offered as a probable explanation of the slight shocks of earthquake, due perhaps to the violent expansion of certain gases confined within the cavities that abound in the island.

A careful consideration of the accounts published in the local papers, and a personal inspection of the localities, tended strongly to confirm the results of theory.

Cuba, from its situation just within the Tropic of Cancer, and at the entrance to the Gulf of Mexico, is admirably placed for the study of these cyclonic storms, and eight of those which have been best observed are traced on a map appended to the pamphlet, showing that in most cases the apex of the parabolic curve is not far from the island. It is a subject of congratulation that an observatory so well conducted, and so situated, has, by the kind assistance of Sir E. Sabine, been provided with a set of magnetic instruments by which the connection of terrestrial magnetism with the most violent of our tropical storms may be thoroughly investigated.

SCIENTIFIC SERIALS

THE *Monthly Microscopical Journal* for this month commences with a paper by Mr. W. H. Dallinger and Dr. Drysdale, entitled "Researches on the Life History of a Cercomonad: a lesson in Biogenesis,"—in which they describe, as the result of a very thorough and long-continued series of observations, the life history of a new Cercomonad, which is thus summarised:—"When mature, it multiplies by fission for a period extending over from two to eight days. It then becomes peculiarly amœboid; two individuals coalesce, slowly increase in size, and become a tightly distended cyst. The cyst bursts, and incalculable hosts of immeasurably small sporules are poured out, as if in a viscid fluid, and densely packed; these are scattered, slowly enlarge, acquire flagella, become active, attain rapidly the parent form, and once more increase by fission." They show also that the granules can withstand a temperature much higher than can the mature forms.—Dr. Royston-Pigott makes remarks on the Confirmation given by Dr. Colonel Woodward to the "Colour test," which comes into play in proving that spherical aberration is reduced to a minimum in objectives.—Dr. Dawson remarks on Mr. Carruthers' views of Prototaxites, the latter author having described it as a gigantic seaweed, called by him *Nematophycus*. Dr. Dawson gives further reasons for maintaining his original opinion that it is phænogamous.—Prof. Rupert Jones continues his excellent papers on Ancient Water-fleas of the Ostracodous and Phyllopodous tribes (Bivalve Entomostraca). This is followed by an article on the pathological relations of the diphtheritic membrane and the croupous cast, by Mr. Jabez Hogg, which would have been more in place in a medical journal. The Wenham-Toller controversy is maintained by the latter and some others, and there are abstracts of several interesting papers, with notices of Vol. III. of Stricker's Histological Manual and Dr. Frey's work on the microscope.

Poggendorff's Annalen der Physik und Chemie: No. 4, 1873.—In this number appears the sixth of the series of papers on internal friction of gases, by O. E. Meyer and F. Springmuhl. The authors, having formerly examined the transpiration of atmospheric air through capillary tubes, have further observed that of carbonic acid, of oxygen, and of hydrogen, and find the Poiseuille law to hold good for these gases also. In most of the experiments the gas streamed from one vessel into another containing the same gas at lower pressure; but the case of a gas streaming into a vessel containing another kind of gas was also examined. The velocity of transpiration proved the same, and there was no counter-current of the second gas through the capillary tube, as in the case of diffusion. In an appended note the authors criticise some experiments of von Lang.—Dr. Rontgen gives details of a careful determination of the relation of specific heat at constant pressure to that at constant volume, for the gases, air, carbonic acid, and hydrogen; the mean numbers obtained being 1·4053, 1·3052, and 1·3852 respectively. The writer discusses these results in their bearing on the mechanical equivalent of heat, and the velocity of sound, and compares the work of previous experimenters on the subject.—The concluding part of a paper by F. Rudorff on solubility of salt mixtures appears in this number; and A. Potier replies to certain strictures, by Quincke, on some recent observations of his, as to reflection from metals and glass. Among the remaining matter may be noted an important memoir by G. Rose (communicated to the Berlin Academy), on the behaviour of the diamond and graphite on being heated. The author describes and illustrates the regular forms produced in the diamond through combustion, treats of the general heating effects where air is excluded and where it is not, the natural blackening of diamonds, the so-called carbonate, and connected topics.—A note by F. Zollner, detailing further experiments to show that electrical currents are produced by current water (a statement which was questioned by Beetz a short time since), also deserves attention.

Der Naturforscher, June 1873.—Among the more important papers in this issue we may note the account of Pettenkofer and Voit's recent researches on the value of fat as a nutritive substance. They find that fat is very largely absorbed from the alimentary canal, but after long feeding with great quantities of fat the absorption becomes less; also that (contrary to a common opinion), fat is much more readily decomposed into simpler products than albumen. The decomposition of food-fat depends on that of albumen, on the amount of albumen present, and on the proportion of it fixed in the organs, to what is in circulation. The results given in this memoir have an important practical bearing. Another physiological paper treats of the influence of food on the structure of digestive organs: the experimenter, H. Crampe, thinks that the nature of food, alone, affords no sufficient explanation of the differences found in these.—An article on the loss of free nitrogen in putrefaction describes some interesting experiments by Messrs. Konig and Kiesow. In physics and chemistry we find notes on the change of length and electricity produced by the galvanic battery, on the action of electricity on carbon compounds, on Dr. Gladstone's new air battery, on the action of electrical force on non-conductors, &c.—There are two French astronomical papers, one on an attempt to measure the diameter of Sirius; the other, on MM. Cornu and Baille's new determination of the mean density of the earth. Geology, meteorology, and other branches of science, are also represented.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, July 28.—M. de Quatrefages, president, in the chair.—The following papers were read:—On the exponential function, by M. Hermite.—An examination of the theory of the thrust of earthworks against their sustaining walls, by M. de Saint-Venant. This was a criticism on M. Curie's late papers on this subject.—On a proposed regular service of train transports between Dover and Calais, by M. Dupuy de Lôme. The author, in conjunction with Mr. Scott Russell, has devised a method of transporting entire trains by means of large steamers. Part of the paper was devoted to a project of a new port west of Calais, as that place is useless for the purpose; at Dover everything is ready for such a purpose, there being now 40 ft. of water at the end of the Admiralty pier at low tide. The proposed scheme would be able to carry 800,000 passengers, and 870,000 tons of goods annually.—On electric cauterisation applied to surgery, by M. Sédillot.—New researches on the solar diameter, by Father Secchi. The author had found the sun's diameter, observed spectroscopically in the lines C and B, to be less than that given by the Nautical Almanac; he hence advocated the use of monochromatic images for making such determinations, and replied to some objections of S. Respighi, who, on repeating these experiments, agreed with the almanac.—M. Ledieu's paper on thermo-dynamics was continued.—On a new method of condensing liquefiable bodies held in suspension in gases, by MM. Pelouze and Audouin.—On different forms of curves of the fourth order, by M. H. G. Zeuthen.—On the respiration of submerged aquatic vegetables, by MM. P. Schutzenberger and F. Quinquaud.—On the structure of the cerebral ganglia of *Zonites algirus*, by M. H. Sicard.—On the planet Mars, by M. C. Flammarion.—On a new system of pneumatic telegraphy, by MM. D. Tommasi and R. F. Michel.

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THURSDAY, AUGUST 14, 1873

THE ENDOWMENT OF RESEARCH

V.

ALTHOUGH it is not within the purpose of these articles to propose an elaborate scheme in which the Endowment of Research in all its branches may be completely provided for, yet it may be reasonably expected that some suggestions should be now put forward to serve as an answer to those who urge the hopeless impracticability of the attempt, and as a foundation upon which a definite plan may be constructed, by the help of criticism, from those who can speak with authority in their own particular subjects.

In the first place, it is above everything important that the need of a systematic organisation of a central character with entire freedom of action should be at once recognised. It is absurd to suppose that the lack of pecuniary means can be the main difficulty which has hitherto, in the richest country in the world, hindered original investigation in the Sciences. The natural harvest of scientific discoveries which England ought annually to reap has rather been checked by the irregularity with which the labourers have been rewarded, and the comparative indignity with which they have been treated. For a certain class of scientific investigations of a strikingly practical character the public will always be willing to sanction large parliamentary grants; but for the permanent Endowment of Research, and the continuous support in a worthy position of the researchers, not only the aid of the nation at large, but the wealth and the prestige of our ancient Universities are required. There is, of course, no reason for any interference with the valuable work at present accomplished by the London Societies, but their work is of a different character. The new organisation would not grow into a monopoly, but would naturally take to itself those departments of knowledge which are least cared for, and in which the benefits of endowments will be most felt. Its wealth would enable it to be liberal, and its public position would impose just that amount of responsibility which should protect it from those dangers to which its wealth might render it exposed.

It is impossible to give a precise account of the actual manner in which the endowment should be distributed. To advance a crude scheme would be disadvantageous to the cause at heart, and to descend into detail would be to offer an unnecessary advantage to the enemy. Much must be left for the future to develope, and much must be left to the men to whom the administration is entrusted. If a scheme were to be worked out in detail in accordance with the demands of Science as understood at the present day, and if strict rules were to be adopted for its application, it might very well happen that before many years have gone our new Foundation would become an obstruction rather than a help to the advancement of Science. That a system may be vague, and yet eminently useful, and that its managers may safely be trusted with powers almost irresponsible, may be learnt from the example of the Smithsonian Institution in the United States. It is there found that to the Secretary of that institution, who at present is Professor Henry, may be confided the management of about 8,000*l.* a year, subject only to the nominal control of a board of American politicians, upon the trust to further "the advancement and diffusion of knowledge." Many incidental lessons may be gathered from the manner in which the funds of this Institution are applied. There are no professors, and no oral instruction of any kind: money is advanced to individual investigators, not to support them while engaged in their scientific labours, but merely to provide the apparatus and the materials necessary for their researches; but the largest part of the funds would appear to be devoted to the publication of the work which they have encouraged, and which under the title of "Smithsonian Contributions to Knowledge," are well known all over Europe. In this case, therefore, Research is indirectly endowed by means of a moderate pecuniary assistance to the investigators, whereas in Germany it is indirectly endowed through the professoriate; but our proposal is that nothing but a direct endowment will satisfy the peculiar wants of this country.

There is yet a further reason why any plan now put forward should be purposely indefinite and incomplete. The funds which the colleges will ultimately yield can only fall in very gradually. It is, according to the modern practice, quite impossible to make anything out of the present holders of fellowships, who are in most cases young men, who may retain their appointments if they choose up to the limit of their lives. It would also, for manifest reasons, be inexpedient to divert each several fellowship as it becomes vacant from its present destination. The machinery of the University organisation is so delicate that the occasions for introducing changes into it must of necessity be left to those who are best acquainted with the manner in which it works. Many years must elapse before that portion of the College revenues to which original research is now putting in a claim can be handed over to this account. In the meantime it is the duty of all those who support this claim not to dispute about details, but to force a hearing for that principle which they advocate in common, and which, when once publicly recognised, will render easy the remainder of the task.

It is not, however, difficult to point out roughly the lines in which the endowment will have to proceed, and so to meet by anticipation the apparent objections which are certain to be alleged. The form the endowment should take, the persons who are to be entrusted with the distribution, and the guarantee that the appointments shall not degenerate into sinecures, are all matters which require explanation. With regard to the first question, it is necessary to clear away a prevalent misunderstanding, which would seem to be based upon the existing system of Fellowships. It is not an essential part of the new scheme that a given number of Research Fellowships should be forthwith founded, to be awarded to young students who have passed successful examinations in Science. The very opposite course is the one which would commend itself to those who are aware of the evils of the present practice. The number of the new appointments should not be fixed; at first it should be small, but capable of increase as the suitable candidates come forward: and above all, the principle of selection should be other than that of competitive examination. The man with the peculiar talents and proved industry which are wanted for the post must be carefully sought for, and the place must be made for him, rather than the man manufactured for the place. The managing body must be allowed perfect liberty either to found a new Fellowship for the particular man, or to refuse to fill up a vacant appointment. All our Research Fellows will be, according to the German system, in extraordinary posts. From this it will follow that direct endowment of this kind, though the ultimate aim of our efforts, and by far the principal part of our scheme, is not the manner in which a beginning should be made. This form of endowment, so far as can at present be foreseen, must be comparatively exceptional, and therefore, when the right man is found, his position should be made one of handsome emolument, and it ought to be rendered impossible that he should be negligently passed over.

The other ways in which research should be endowed may be regarded in the ultimate scheme as chiefly subsidiary to this, but in the order of time they must come first. The funds of the Colleges which are not wanted for teaching purposes, may at once be utilised for our object in an infinite number of indirect ways. They ought to be regarded as an abundant reservoir, from which may be continually drawn generous encouragement and ready help for those who happen to be carrying on some special investigation in any branch of Science. The Colleges should take the place which was occupied in England some century ago by those noble and wealthy patrons to whom Science, Art, and Literature all owe so much. They should give in no grudging spirit, for they may be assured that an apparent waste in one direction will be amply compensated by the unlooked-for returns which they will reap in another. By throwing open their libraries, by building museums and laboratories, by supplying instruments or needful materials, by paying for laborious calculations or expensive publications, as well as by subsidising any particular investigation, they would breed up, so far as any artificial means can, that race of men from whom the selection must afterwards be made for their new Fellowships. To those who have had unfortunate experience of the management of college business, and of the sort of matters which come before a college meeting, such a reform as has been sketched out will doubtless appear as a visionary ideal; yet it might be realised with very little trouble if the richest Colleges would transfer some of the attention which they now bestow upon ecclesiastical and educational interests, to the cause of original research, and when realised, the result would be more nearly akin than the present, to that which the original statutes contemplated.

To answer the two other questions proposed need not take long, for an implicit reply to them has already been given. Fortunately, modern Science has taken such definite shape, and is pursued in such full publicity, that each branch has even now, at its head, certain acknowledged leaders, to whose judgments and recommendations in their special subjects, all deference is due. Until the Universities and the Colleges become sufficiently penetrated with the new scientific spirit, it will be natural that they should endow research under the guidance of the scientific societies, and of course it will be always necessary that they should be fully conscious of their responsibilities to the public for the appointments they confer upon the candidates, however selected. The analogy of the Smithsonian Institution will here again come in, for its assistance is never given in any case unless after a favourable report from a Commission of scientific men, who are experts in the particular matter submitted to them.

With regard to the objection that the plan will inevitably tend to the foundation of a new store of sinecures, it is not incumbent to say more than that scientific posts, where the duty itself is of absorbing pleasure, are the least likely to degenerate in the way suggested, and that the insinuation comes with an ill grace from those who are the present recipients of benefactions which they do so little to deserve. C.

ON LOSCHMIDT'S EXPERIMENTS ON DIFFUSION IN RELATION TO THE KINETIC THEORY OF GASES

THE kinetic theory asserts that a gas consists of separate molecules, each moving with a velocity amounting, in the case of hydrogen, to 1,800 metres per second. This velocity, however, by no means determines the rate at which a group of molecules set at liberty in one part of a vessel full of the gas will make their way into other parts. In spite of the great velocity of the molecules, the direction of their course is so often altered and reversed by collision with other molecules, that the process of diffusion is comparatively a slow one.

The first experiments from which a rough estimate of the rate of diffusion of one gas through another can be deduced are those of Graham.* Professor Loschmidt, of Vienna, has recently† made a series of most valuable and accurate experiments on the interdiffusion of gases in a vertical tube, from which he has deduced the coefficient of diffusion of ten pairs of gases. These results I consider to be the most valuable hitherto obtained as data for the construction of a molecular theory of gases.

There are two other kinds of diffusion capable of experimental investigation, and from which the same data may be derived, but in both cases the experimental methods are exposed to much greater risk of error than in the case of diffusion. The first of these is the diffusion of momentum, or the lateral communication of sensible motion from one stratum of a gas to another. This is the explanation, on the kinetic theory, of the viscosity or internal friction of gases. The investigation of the viscosity of gases requires experiments of great delicacy, and involving very considerable corrections before the true coefficient of viscosity is obtained. Thus the numbers obtained by myself in 1865 are nearly double of those calculated by Prof. Stokes from the experiments of Baily on pendulums, but not much more than half those deduced by O. E. Meyer from his own experiments. The other kind of diffusion is that of the energy of agitation of the molecules. This is called the conduction of heat. The experimental investigation of this subject is confessedly so difficult, that it is only recently that Prof. Stefan of Vienna,* by means of a very ingenious method, has obtained the first experimental determination of the conductivity of air. This result is, as he says, in striking agreement with the kinetic theory of gases.

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* *Brande's Journal* for 1829, pt. ii., p. 74. "On the Mobility of Gases," Phil. Trans., 1863.

† Sitzb. d. k. Akad. d. Wissensch., 10 März 1870.

The experiments on the interdiffusion of gases, as conducted by Prof. Loschmidt and his pupils, appear to be far more independent of disturbing causes than any experiments on viscosity or conductivity. The interdiffusing gases are left to themselves in a vertical cylindrical vessel, the heavier gas being underneath. No disturbing effect due to currents seems to exist, and the results of different experiments with the same pair of gases appear to be very consistent with each other.

They prove conclusively that the co-efficient of diffusion varies inversely as the pressure, a result in accordance with the kinetic theory, whatever hypothesis we adopt as to the nature of the mutual action of the molecules during their encounters.

They also show that the co-efficient of diffusion increases as the temperature rises, but the range of temperature in the experiments appears to be too small to enable us to decide whether it varies as T^2, as it should be according to the theory of a force inversely as the fifth power of the distance adopted in my paper in the Phil. Trans. 1866, or as $T^{\frac{3}{2}}$ as it should do according to the theory of elastic spherical molecules, which was the hypothesis originally developed by Clausius, by myself in the Phil. Mag. 1860, and by O. E. Meyer.

In comparing the co-efficients of diffusion of different pairs of gases, Prof. Loschmidt has made use of a formula according to which the co-efficient of diffusion should vary inversely as the geometric mean of the atomic weights of the two gases. I am unable to see any ground for this hypothesis in the kinetic theory, which in fact leads to a different result, involving the diameters of the molecules, as well as their masses. The numerical results obtained by Prof Loschmidt do not agree with his formula in a manner corresponding to the accuracy of his experiments. They agree in a very remarkable manner with the formula derived from the kinetic theory.

I have recently been revising the theory of gases founded on that of the collisions of elastic spheres, using, however, the methods of my paper on the dynamical theory of gases (Phil. Trans. 1866) rather than those of my first paper in the Phil. Mag., 1860, which are more difficult of application, and which led me into great confusion, especially in treating of the diffusion of gases.

The co-efficient of interdiffusion of two gases, according to this theory, is—

$$D_{12} = \frac{1}{2\sqrt{6\pi}} \frac{V}{N} \sqrt{\frac{1}{w_1}+\frac{1}{w_2}} \; \frac{1}{s^2_{12}} \qquad (1)$$

where w_1 and w_2 are the molecular weights of the two gases, that of hydrogen being unity.

s_{12} is the distance between the centres of the molecules at collision in centimetres.

V is the "velocity of mean square" of a molecule of hydrogen at 0° C.

$$V = \sqrt{\frac{3p}{\rho}} = 185{,}900 \text{ centimetres per second.}$$

N is the number of molecules in a cubic centimetre at 0° $C.$ and 76 cm. $B.$ (the same for all gases).

D_{12} is the co-efficient of interdiffusion of the two gases in $\frac{(\text{centimetre})^2}{\text{second}}$ measure.

We may simplify this expression by writing—

$$a^2 = \frac{1}{2\sqrt{6\pi}} \frac{V}{N}, \quad \sigma_{12}^2 = \frac{1}{D_{12}} \sqrt{\frac{1}{w_1} + \frac{1}{w_2}} \qquad (2)$$

Here a is a quantity the same for all gases, but involving the unknown number N.

σ is a quantity which may be deduced from the corresponding experiment of M. Loschmidt. We have thus—

$$s_{12} = a\,\sigma_{12} \qquad (3)$$

or the distance between the centres of the molecules at collision is proportional to the quantity σ, which may be deduced from experiment.

If d_1 and d_2 are the diameters of the two molecules.

$$s_{12} = \tfrac{1}{2}(d_1 + d_2).$$

$$\text{Hence if } d = a\,\delta \; . \; . \; . \; \sigma_{12} = \tfrac{1}{2}(\delta_1 + \delta_2). \qquad (4)$$

Now M. Loschmidt has determined D for the six pairs of gases which can be formed from Hydrogen, Oxygen, Carbonic Oxide, and Carbonic Acid. The six values of σ deduced from these experiments ought not to be independent, since they may be deduced from the four values of δ belonging to the two gases. Accordingly we find, by assuming

TABLE I.

δ(H) = 1·739
δ(O) = 2·283
δ(CO) = 2·461
δ(CO_2) = 2·775

σ_{12}	Calculated $\frac{1}{2}(\delta_1 + \delta_2)$	Observed $\sqrt{\frac{1}{D}\sqrt{\frac{1}{w_1}+\frac{1}{w_2}}}$
For H and O	2·011	1·992
For H and CO	2·100	2·116
For H and CO_2	2·257	2·260
For O and CO	2·372	2·375
For O and CO_2	2·529	2·545
For CO and CO_2	2·618	2·599

NOTE.—These numbers must be multiplied by 0·6 to reduce them to (centimetre-second) measure from the (metre-hour) measure employed by Loschmidt.

The agreement of these numbers furnishes, I think, evidence of considerable strength in favour of this form of the kinetic theory, and if it should be confirmed by the comparison of results obtained from a greater number of pairs of gases it will be greatly strengthened.

Evidence, however, of a higher order may be furnished by a comparison between the results of experiments of entirely different kinds, as for instance, the coefficients of diffusion and those of viscosity. If μ denotes the coefficient of viscosity, and ρ the density of a gas at 0° C. and 760 mm. B, the theory gives—

$$\frac{\mu}{\rho} = a^2 \sqrt{\frac{2}{w}} \; \frac{1}{d^2} \qquad (5)$$

so that the following relation exists between the viscosities of two gases and their coefficient of interdiffusion—

$$D_{12} = \frac{1}{2}\left(\frac{\mu_1}{\rho_1} + \frac{\mu_2}{\rho_2}\right) \qquad (6)$$

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* Sitzb. d. k. Akad., Feb. 22, 1872.

Calculating from the data of Table I., the viscosities of the gases, and comparing them with those found by O. E. Meyer and by myself, and reducing all to centimetre, gramme, second measure, and to 0° C.—

TABLE II.

Coefficient of Viscosity

Gas.	Loschmidt.	O. E. Meyer.	Maxwell.
H	0·000116	0·000134	0·000097
O	0·000270	0·000306	
CO	0·000217	0·000266	
CO_2	0·000214	0·000231	0·000161

The numbers given by Meyer are greater than those derived from Loschmidt. Mine, on the other hand, are much smaller. I think, however, that of the three, Loschmidt's are to be preferred as an estimate of the absolute value of the quantities, while those of Meyer, derived from Graham's experiments, may possibly give the ratios of the viscosities of different gases more correctly. Loschmidt has also given the coefficients of interdiffusion of four other pairs of gases, but as each of these contains a gas not contained in any other pair, I have made no use of them.

In the form of the theory as developed by Clausius, an important part is played by a quantity called the *mean length of the uninterrupted path of a molecule*, or, more concisely, the *mean path*. Its value, according to my calculations, is

$$l = \frac{1}{\sqrt{2\pi}\, s^3 N} = \frac{\sqrt{12}}{\sqrt{\pi}\, V}\frac{1}{\delta^2}\text{*}. \qquad (7)$$

Its value in tenth-metres (1 metre × 10^{-10}) is

TABLE III.

For Hydrogen . .	965	Tenth-metres at 0° C. and 760 B
For Oxygen . .	560	
For Carbonic Oxide	482	
For Carbonic Acid	430	

(The wave-length of the hydrogen-ray F is 4,861 tenth-metres, or about ten times the mean path of a molecule of carbonic oxide.)

We may now proceed for a few steps on more hazardous ground, and inquire into the actual size of the molecules. Prof. Loschmidt himself, in his paper "Zur Grosse der Luftmolecüle" (Acad. Vienna, Oct. 12, 1865), was the first to make this attempt. Independently of him and of each other, Mr. G. J. Stoney (Phil. Mag. Aug. 1868), and Sir W. Thomson (NATURE, March 31, 1870), have made similar calculations. We shall follow the track of Prof. Loschmidt.

The volume of a spherical molecule is $\frac{\pi}{6} s^3$, where s is its diameter. Hence if N is the number of molecules in unit of volume, the space actually filled by the molecules is $\frac{\pi}{6} Ns^3$.

This, then, would be the volume to which a cubic centimetre of the gas would be reduced if it could be so compressed as to leave no room whatever between the molecules. This, of course, is impossible; but we may, for the sake of clearness, call the quantity—

$$\epsilon = \frac{\pi}{6} Ns^3 \qquad (8)$$

the ideal coefficient of condensation. The actual coefficient of condensation, when the gas is reduced to the liquid or even the solid form, and exposed to the greatest degree of cold and pressure, is of course greater than ϵ.

Multiplying equations 7 and 8, we find—

$$s = 6\sqrt{2}\,\epsilon\, l \qquad (9)$$

where s is the diameter of a molecule, ϵ the coefficient of condensation, and l the mean path of a molecule.

Of these quantities, we know l approximately already, but with respect to ϵ we only know its superior limit. It is only by ascertaining whether calculations of this kind, made with respect to different substances, lead to consistent results, that we can obtain any confidence in our estimates of ϵ.

M. Lorenz Meyer* has compared the "molecular volumes" of different substances, as estimated by Kopp from measurements of the density of these substances and their compounds, with the values of s^3 as deduced from experiments on the viscosity of gases, and has shown that there is a considerable degree of correspondence between the two sets of numbers.

The "molecular volume" of a substance here spoken of is the volume in cubic centimetres of as much of the substance in the liquid state as contains as many molecules as one gramme of hydrogen. Hence if ρ_0 denote the density of hydrogen, and $\mathfrak{b}$ the molecular volume of a substance, the actual coefficient of condensation is—

$$\epsilon' = \rho_0\, \mathfrak{b} \qquad (10)$$

These "molecular volumes" of liquids are estimated at the boiling-points of the liquids, a very arbitrary condition, for this depends on the pressure, and there is no reason in the nature of things for fixing on 760 mm. B. as a standard pressure merely because it roughly represents the ordinary pressure of our atmosphere. What would be better, if it were not impossible to obtain it, would be the volume at − 273° C. and ∞B.

But the volume relations of potassium with its oxide and its hydrated oxide as described by Faraday seem to indicate that we have a good deal yet to learn about the volumes of atoms.

If, however, for our immediate purpose, we assume the smallest molecular volume of oxygen given by Kopp as derived from a comparison of the volume of tin with that of its oxide and put

$$\mathfrak{b}(O = 16) = 27$$

we find for the diameters of the molecules—

TABLE IV.

Hydrogen	5·8 tenth-metres.
Oxygen	7·6
Carbonic Oxide . .	8·3
Carbonic Acid . .	9·3

The mass of a molecule of hydrogen on this assumption is

$$4{\cdot}6 \times 10^{-24} \text{ gramme.}$$

The number of molecules in a cubic centimetre of any gas at 0° C. and 760 mm. B. is

$$N = 19 \times 10^{18}.$$

Hence the side of a cube which, on an average, would contain one molecule would be

$$N^{-\frac{1}{3}} = 37 \text{ tenth-metres.}$$

J. CLERK-MAXWELL

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* The difference between this value and that given by M. Clausius in his paper of 1858, arises from his assuming that all the molecules have equal velocities, while I suppose the velocities to be distributed according to the "law of errors."

* Annalen d. Chemie u Pharmacie V. Supp. bd. 2, Heft (1867).

THE LAST GLACIAL EPOCH

On the Cause, Date, and Duration of the Last Glacial Epoch of Geology, and the Probable Antiquity of Man. With an investigation and description of a new movement of the Earth. By Lieut.-Colonel Drayson, R.A., F.R.A.S. (London: Chapman and Hall, 1873.)

THE author of this work allows the existence of the motion of rotation of the earth on its axis and its revolution round the sun. That motion, however, of the axis of the earth, to which is due the precession of the equinoxes, is to him a great stumbling block. He denies the possibility of this motion as generally accepted, and gives us a theory of his own, which is very novel, and the results of which are startling in the extreme.

Lieut.-Colonel Drayson either knows nothing of dynamics or despises the science; the one key he makes use of to unlock the secrets of astronomy is geometry; he does not believe in the existence of a change in the plane of the ecliptic, and apparently is not aware that the attractions of the other planets on the earth *must* produce periodic changes in the plane of the earth's orbit. In consequence of this he persuades himself that all astronomers teach (and perhaps believe) that while the pole of the earth is describing a circle round the pole of the ecliptic, the obliquity of the ecliptic, which is the angular distance between these poles, is constantly changing. He calls this a geometrical impossibility, and nobody would hesitate to agree with him that it is; but astronomers would at once deny that they either teach or believe anything of the kind. The popular belief is that the pole of the earth describes a circle of radius 23° 28′ round the pole of the ecliptic as a centre, and that the whole circle would be described in something over 25,000 years.

Lieut.-Colonel Drayson tells us that the true motion of the pole of the earth is in a circle whose radius is 29° 25′ 47″, and whose centre is at a distance of 6° from the pole of the ecliptic. He attempts to prove this, and, we believe, has succeeded in persuading himself that he has proved it. He does this by showing that this particular circle will satisfy all the necessary conditions, as he puts them, and also (we assume) as he understands them. The author next proceeds to deduce the consequences of this motion. His circle would be described in 31,840 years, so that at intervals of 15,920 years the obliquity of the ecliptic would vary as much as 12°. The consequence of this would be that about 13,700 B.C., Great Britain would have had during the winter an arctic climate, the sun in lat. 54° not being 1° above the horizon at the winter solstice, and during the summer a tropical climate. This is supposed to have been the last glacial epoch, and the author has such confidence in his theory that he promises us glacial epochs every 31,840 years.

The book, as a whole, we look upon as most unsatisfactory. Had the author mastered the principles of dynamics, he probably would not have been led by a mistaken interpretation of movements which he only partly understood, into the fatal error of attempting to solve one of the most abstruse problems in astronomy by mere geometry. The days of such attempts were, we hoped, past for ever.

The motion of the earth's axis is well illustrated by the motion of a boy's top when it is spinning with its axis inclined to the vertical. Every one has seen a top while spinning on its own axis, revolve round the vertical with approximately constant speed, while its axis remained inclined to the vertical at an approximately constant angle: but who has seen a top spinning so that its axis revolved with constant speed round a line inclined to the vertical at an angle of 6°, or any other angle? Till Lieut.-Colonel Drayson produces a top which will do this, thereby proving experimentally that such a motion is possible, or till he demonstrates by analysis the possibility of such a motion, we shall feel confident in rejecting his theory of the earth's motion, as the theory of a paradoxer, and in regarding the cause of the last glacial epoch as a secret still unknown.

DR. SMITH ON FOODS

Foods. By Edward Smith, M.D., F.R.S. (Henry S. King and Co.)

THE tendency during the last thirty years or so to the equalisation, throughout the country, of the prices of the several articles employed as food, has done much to make the subject of Foods one of much greater interest to a larger class of the community than heretofore. The products of a district being now scarcely, if at all, cheaper than those that can be obtained from a considerable distance, a knowledge of the relative nutritive value of foods becomes essential to a larger number. We therefore look with great interest to the results of Dr. Edward Smith's considerable experience, especially with regard to some of the articles of more modern introduction.

The classification adopted is the following. Foods are first divided into solid, liquid, and gaseous, an arrangement which has the disadvantage of separating closely-allied substances from one another, milk having to be considered removed from cheese and butter. The solid foods are then divided into animal and vegetable, and each of these are subdivided into nitrogenous and non-nitrogenous. The source, composition, and alimentary properties of each article are then discussed in detail. The analyses are mainly those of Fresenius, Frankland, Wanklyn, and other well-known chemists. The author in most cases is able to introduce the results of his own observations on the physiological action of each substance, which are also to be found in the Transactions of the Royal Society. Taking arrowroot as a fair example of the manner in which the subject is treated, after a short account of its origin we find that "the proximate elements in 100 parts are water 18·0, and starch 82·0; so that it is or should be free from nitrogen. There are 2,555 grains of carbon in 1 lb. . . . Ten grains of arrowroot when thoroughly consumed in the body produce heat sufficient to raise 10·06 lbs. of water 1° F., which is equal to lifting 7,766 lbs. one foot high." The author observes that when eaten alone on an empty stomach it gives no sense of satisfaction, but one of malaise. Eating 500 grains increased the emission of carbonic acid 0·154 grains per minute. The rate of respiration was somewhat lessened, and the pulse was increased four beats per second (*sic*). As each subject is similarly described, it is evident that there is a large amount of needless repetition, for the estimation of the heat of combustion is a simple calculation, which might have been made once for all with reference to each proximate principle, especially since the bare facts, as they are put, convey but little idea to the general reader. The chemistry of foods is very superficially and imperfectly treated, not nearly so full as it deserves; and the botany would have been better if a more thorough study of materia medica had been undertaken. There is one sentence we have in vain attempted to understand. When speaking of the sweet chestnut, the author, after remarking that at present it may be regarded as a luxury, says, "The first step to a great extension of its use would be to make the ordinary horse-chestnut a safe and agreeable food, since it grows in our climate, and could be obtained in large quantities." How this can be, seems extremely difficult to understand; as is well known, the two fruits having nothing whatever to do with one another.

The descriptions of the various methods that have been proposed for the preservation of meats which have to travel long distances and through hot climates is very complete and clear. The preference is given to the method of heating, and that adopted by Mr. Jones, in which the meat is heated in vacuo, to 280° F., in the cans, is fully described. It is shown, however, that by this process the meat is stewed, and over-stewed, not roasted nor boiled. In this, and all similar processes, it is found impossible to expel all the air without over-cooking the meat.

Another subject of particular interest which is discussed is the preservation of milk. Two methods, it appears, are adopted in America, one in which the milk is simply evaporated to one-fourth its original volume, when it will often keep for a month, and another in which sugar is added; by the latter process it remains good for an indefinite time, and contains about one-third of its weight of sugar. The author agrees sufficiently with Dr. Daly in his condemnation of the employment of this preserved milk for infants, to quote an article by him which appeared last year in the *Lancet.*

Extract of meat, especially Liebig's, occupies the greater part of one chapter, and we think the author has done good service in setting in a clear and unmistakable light the true value of that expensive luxury. He shows that its chief value depends on the meaty flavour it is capable of imparting, and that its nutritive value is *nil.* He remarks—"Its proper position in dietetics is somewhat more than that of a meat-flavourer, but all that is required for nutrition should be added to it. . . . Used alone for beef-tea it is a delusion." That this is correct is evident from a consideration of the method by which it is prepared, for "during the process, all the fat and as much of the gelatin and albumen as can be extracted are removed from the solution of flesh, whilst the fibrin, being insoluble, is necessarily left behind. Hence there remain water, salts, osmazone, and the extractives of flesh, or, in general terms, the flavouring matters and the salts of meat—thus leaving out all that is popularly (and correctly) regarded as nutritious."

Many tables are given to show the effects of different substances on the respiration, pulse, exhalation of carbonic anhydride and aqueous vapour. There seems to be a want of association between the great mass of facts, which must have been the result of long and continuous labour; and they are undoubtedly put forward in a way which is not best suited to convince the scientific student. For example, the effects on the pulse, &c., of tea dissolved in water is given in full, but under the head of water no mention is made of its physiological action, though decidedly, by itself it changes the pulse rate, if nothing else.

Several recipes of the fourteenth century are quoted from "Cury," a copy of ancient manuscript recipes of the master cook of Richard the Second. There are also many scriptural references, and a very inappropriate abstract of an incident which occurred at the Worship Street Police Court.

OUR BOOK SHELF

A Manual of Metallurgy. By George Hogarth Makins, M.R.C.S., F.C.S., &c. (Ellis and White, 1873.)

THE present edition of this work presents a marked improvement over those which have preceded it, but it is still far from being all that even a small manual might be. In the preface the author expresses a hope that the volume, "in which the leading points connected with the principal metals are set forth, may be found useful," and as there are singularly few metallurgical works in the English language, we have but little doubt that this hope will be realised. Mr. Makins has long enjoyed the reputation of being a most accurate assayer, and the descriptions of the processes of assaying gold and silver are careful and valuable. The portion of the work which is the least satisfactory is that devoted to iron.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Huemul

THE Huemul of Chili and Patagonia, referred to in NATURE, v. pp. 253 and 263, was *first* recognised in modern scientific literature by MM. Gay and Gervais, who in the *Annales des Sciences Naturelles* for 1846 (v., p. 91), showed that the so-called *Equus bisulcus* of Molina, was a species of Deer (*Cervus*), which they proposed to call *Cervus chilensis.*

In Gay's "Fauna Chilena" (plates 10 and 11), the female and skull are figured. Concerning the nomenclature of species, I have published some remarks in the last volume of the "Annals of Natural History" (ser. 4, vol. vi. p. 213), to which I beg leave to refer such of your readers as are desirous of further information on this subject.

P. L. SCLATER

11, Hanover Square, W., Aug. 6

Perception and Instinct in the Lower Animals

IN answer to Mr. George J. Romanes (NATURE, August 7) I beg to say that I particularly inquired of my friend whether he had been to or near his old house on the day the dog returned, or shortly before, and he assured me that "he had never been near it since he left." I ought to have stated this in my account of the circumstance.

I shall make no further remarks on the subject, because I believe that nothing satisfactory can be arrived at till experiments of the nature indicated in my last letter have been systematically carried out.

ALFRED R. WALLACE

Collective Instinct

THE writer of one of the books on Indian sport relates how he saw a herd of antelopes, driven backwards and forwards by four wolves, which surrounded the herd, each guarding a different side, until at length the antelopes passed over a ditch in which a fifth wolf lay concealed. This wolf, jumping up as the antelopes crossed, secured one of them, upon which his four companions joined him, and assisted in making a meal of the captured animal.

A civilian of the N.W.P.* told me that he witnessed a very similar occurrence in Oudh. He saw two wolves standing together, and shortly after noticing them was surprised to see one of them lie down in a ditch, and the other walk away over the open plain. He watched the latter, which deliberately went to the far side of a herd of antelopes standing in the plain, and drove them, as a sheep dog would a flock of sheep, to the very spot where his companion lay in ambush. As the antelopes crossed the ditch, the concealed wolf jumped up, as in the former case, seized a doe, and was joined by his colleague.

Here are two well-authenticated instances of an action or series of actions requiring the exercise of combined sagacity of a high degree on the part of two or more individual animals, being performed in exactly the same way by different members of the same species. Was the method employed by the wolves to secure their food, which they could not have caught single-handed, the result of separate experience or of inherited habit? The identical character of the stratagem employed in the two cases points to the latter.

I have noticed some similar instances of collective action on the part of other animals which I believe to be as much inherited as the habitual actions of individual animals. I have constantly seen a flock of pelicans when on the feed form a line across a lake, and drive the fish before them up its whole length, just as fishermen would with a net. The capture of the fish is rendered doubly easy by this method. I have witnessed exactly a similar plan pursued by a large number of Ganges crocodiles which had been lying or swimming about all day in front of my tent, at the mouth of a small stream which led from some large inland lakes to the Ganges. Towards dusk, at the same moment, every one of them left the bank on which they were lying, or the deep water in which they were swimming, and formed line across the stream, which was about twenty yards wide. They had to form a double line, as there was not room for all in a single line. They then swam slowly up the shallow stream, driving the fish before them, and I saw two or three fish caught before they disappeared.

Where a large number of individuals constantly repeat in continuation the same action, it is possible that the younger members may merely copy the older members of the species, and so carry on the habit generation after generation. This is less likely where few are concerned, as in the case of the wolves. A pair of wolves are probably of the same age. It is a marked habit of some species of birds to hunt in pairs, and assist each other in the capture of their prey. The *wokkab*, or common eagles of the Indian plains, hunt in this way. When one of the pair misses in its swoop, the other descends on the victim before it has time to make a fresh attempt to escape. The circumstance that some species of birds of prey are in the habit of combining for the capture of their food, while others hunt singly, would tend to prove that the combined habit is as much inherited as the habits of individuals are known to be.

Gregarious actions, which require combination of purpose on the part of two or more individuals, entail the exercise, if not of a higher degree of intelligence, at any rate of a greater number of intelligent qualities than the isolated actions of single individuals. This class of actions possesses, therefore, a special interest. Those instances in which different individuals perform totally different acts for the attainment of the same end, as in the case of the wolves, are the most interesting, as requiring the most intelligent qualities. I should be glad to learn if any of your readers have ever witnessed or heard of the stratagem described above being employed by wolves for the capture of their prey.

Allahabad, June 29 E. C. BUCK

Ants and "the Taint of the Hand"

IN NATURE, July 24, Mr. James D. Hague, writing on the habits of ants, attributes their dislike to the place across which a finger has been drawn to "the taint of the hand."

Now, Sir, I have frequently drawn a line with a piece of chalk across the track of ants, and observed in them the same symptoms of dislike as Mr. Hague's ants showed to the finger-mark.

I have also drawn a small circle with chalk round one or more ants, who will seek a spot untouched by the chalk through which to make their escape; but should there be no such opening, they will presently cross the circle. If, however, this enclosure be made upon a perpendicular wall, &c., they will frequently drop to the ground rather than walk across the line.

Now, as I have never observed this same dislike—exhibited by dropping—of the "taint" when ants have been running over my hands, and as the chalk-line has the same effect as the finger-mark, may it not be something else than the "taint of the hand" to which the ants object when their usual track is interfered with?

Stamford, Aug. 8 G. E. C.

Venomous Caterpillars

WITH reference to a paper published by Mr. Murray in NATURE, vol. viii. p. 7, on Venomous Caterpillars, I wish, in corroboration, to add my testimony from personal experience, that a species of caterpillar has the power of inflicting a very painful sensation (I will not say wound, as such was not visible) by its sting.

In 1868, when travelling in company with Capt. Street in the Burmese forests on a botanical trip, and whilst in the act of detaching a specimen plant of *Dendrobium farmerii*, from the naked branch of a tree, I felt a severe and painful sting on my thumb. On examination I noticed I had seized hold of a large caterpillar lodged amongst the roots of this orchid. It was about two inches long, clothed with erect hairs; its colour was a reddish brown, the lower part of the abdomen being darker, with well-developed legs.

My thumb continued painful for three days; it was considerably swollen, the skin having a drawn glazed appearance.

The Burmese told me that this kind of caterpillar was exceedingly venomous, and one fellow was particularly consoling by informing me that unless the pain subsided in three days the sting might prove fatal. I am inclined to think that the caterpillar for self-protection has the power of detaching these hairs; whether any propelling force is present at the time of detachment it would be difficult to prove.

I found steeping my thumb in Eau de Cologne gave me the greatest relief.

Whether these hairy caterpillars have a special venom or otherwise I do not feel qualified to express my opinion either one way or the other; but I lean towards the conclusion that the irritation is set up by the mechanical action of the spine during its penetration of the skin, and my reason for inclining towards this opinion is because we have a somewhat parallel case in the irritation caused by the hairs of the prickly pear.

I was present when an officer was thrown off his horse into a prickly pear hedge; he suffered the greatest pain, and could not bear the parts, where these minute spines had penetrated the skin, to be touched. On his being placed in a warm bath the relief was almost immediate, especially to those parts capable of total immersion, and this I attribute to the prickles or hairs floating and becoming removed from the skin by the oscillatory motion of the water.

Madras, July R. BENSON

Abnormal Ox-eye Daisy

IN 1868 I gathered among the ruins of Pompeii a very curious monstrosity of the common ox-eye daisy. The flower and flower-stalk were confounded into a strap-shaped mass which was fringed with the florets. I showed it to Prof. Wyville Thomson, who told me it was an instance abnormal in this species, of the form of inflorescence which is normal in the coxcomb.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Aug. 1

Canarese Snakes

FAM. *Erycidæ*, Gen. *Gongylophis*? Sp.?—Captured in Mangalore, December 2. Gape wide; fangs in sup. and inf. maxillaris.

Body moderate, tail short, obtuse scales, smooth, 48;—ventrals narrow, 197, terminating with three rows of scales between last ventral and anal; latter entire. Subcaudals single, 24, last forming conical point.

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* Mr. Elliott, B.C.S., now Secretary to Government, N.W.P.

Head flat, not very distinct from neck, scaled, with following exceptions :—Rostril, anterior frontals; nasals (double, with the nostril between); mental; upper (12) and lower labials.

Gular depression; small groove anterior to orbit; orbit surrounded by scales; eye small, pupil vertical, iris silver grey, with dark longitudinal streak.

Rudimentary hind limbs, scales small, greatly increasing in size as they approach ventrals; colour above greyish brown, vertebral series of dark brown irregular spots, confluent towards neck; lateral series of dark brown spots. Belly whitish, mottled with dark brown; post orbital dark brown streak.

Length of specimen 24 in. A sand snake of sluggish disposition, especially during day-time.—Did not attempt to bite when handled.

Fam. *Elapidæ*. Gen. *Ophiophagus*. *O. Elaps*.—The Hamadryad, a male specimen caught by snake charmers at Agumbi, Western Ghauts, South Canara. Since dead, the skin having been secured by a member of the Basil Mission. I measured the snake when alive, and found it to be 10 ft. 6 in. but it was probably more, as it strongly resisted being stretched out. Colour brownish black, with about thirty bands on fore part of body, formed by dull yellowish interstitial skin. A yellow V mark with the apex towards head on upper part of hood: dark band beneath hood.

The Canarese call the snake "Kallinga havre," and state it to be common in the jungles along the Ghauts. I hope before long to procure a live specimen.

Fam. *Crotalidæ*. Gen. *Trimeresurus*. Sp.—Scales 21, ventrals 153, subcaudals 58. Head scales strongly keeled. Colour dark reddish brown, irregularly marked with pale reddish brown, forming pale centred lateral ocelli. A series of pale yellow irregular dots arranged in a lateral stripe. This specimen has been forwarded to Dr. J. Shortt, F.L.S.

A specimen of the *Daboia elegans*, the Tic Polongo of Southern India and Ceylon, was lately brought me having the belly pure white, unmarked with the usual brown spots.

A Tahsildár in a Northern Talug reports the occurrence of a large venomous snake, black above and red beneath. This I think will prove to be *Callophis* (*Elaps*) *nigrescens*.

Mangalore. H. H. PRINGLE

BRITISH MEDICAL ASSOCIATION.—ABSTRACT OF DR. SANDERSON'S ADDRESS ON PHYSIOLOGY

IN his address on Physiology before the British Medical Association, Dr. Sanderson gave a *résumé* of the most important physiological work that has been done during the past year. Commencing with the circulation of the blood, he considered it to be resolved into several constituent processes, such as arterial pressure, velocity of blood-current, and contraction or relaxation of muscular fibre. He referred to a very elegant method adopted by Dr. Marey of Paris, and illustrated by him to the members of the Association, by which the influence of arterial resistance on the heart's rapidity may be demonstrated on the excised heart of the tortoise, the number of pulsations being proved to vary inversely as the *resistance* and not as the blood pressure, a fact previously known, but not before so clearly illustrated. He then referred to the observations of Mr. Dewar and Dr. M'Kendrick, in which they have shown that the normal electromotive force in the optic nerve is reduced in intensity when it is receiving the impression of light, a "negative variation" of the current being the result. Dr. Jackson's and Dr. Ferrier's pathological and physiological studies as to the localisation of the sources whence originate some of the voluntary movements in certain parts of the surface of the brain were shown to have a very important bearing on the progress of cerebral physiology; Dr. Ferrier having arrived at a method by which one at least of the highest functions of the nervous system can be brought under the control of experimental investigation. With reference to the part played by Bacteria in the living organism, Dr. Sanderson remarked that observations respecting them were, though very numerous, not sufficiently connected to allow of a ready summary; the facts added during the year being, first, that in certain persons apparently healthy, and in many animals, organisms belonging to this class are always found in the blood; secondly, that in all acute inflammations which are attended with the destruction of living tissue, Bacteria are to be found in the exudation liquids; and thirdly, that in relapsing fever living beings are present in the blood, which exhibit characteristic forms.

Dr. Sanderson in the latter part of his address gave many reasons in favour of the combination of the study of medicine with that of physiology. It has been said that theoretical physiology has led to injurious medical treatment, *e.g.*, to the over-feeding and over-stimulating treatment of disease; to the unreasonable disuse of venesection; to the neglect of antimony and other so-called antiphlogistics, and to the purgative treatment of cholera. But are the theories on which these changes of treatment have been based, physiological in the proper sense? Decidedly not. Taking the action of mercury as an example. It has been proved to have no influence in increasing the secretion of the liver; nevertheless, blue-pill is of undoubted value in certain well-defined disturbances of the digestive organs. From these facts, however, it is not right to assume that mercurial remedies are useless, or that they act beneficially by exciting the secretion of bile; such inferences are not physiological, but result from the manner in which practical men throw undeserved discredit upon Science by attempting to apply its facts without any sufficient knowledge of their bearing. Therefore it is highly desirable for the welfare of both Medicine and Physiology that a distinct line of demarcation should be drawn between them.

The speaker then entered upon subjects of a more purely medical nature, giving an excellent *résumé* of the present position of our knowledge respecting the nature of fever and pyrexia generally.

LAKES WITH TWO OUTFALLS

SOME years ago a discussion took place concerning the possible or actual existence of lakes possessing outlets into two distinct watersheds, so as to render one watershed continuous with the other. If even one such lake could be shown to exist, the question would of course be resolved in the affirmative. I have frequently heard mentioned as an instance a certain lake at the summit of the Romsdal in Norway, and having lately spent a day or two at each end of this lake, I have taken advantage of the opportunity to examine each of the outlets with care. I have thus convinced myself that it ought not to be quoted as a proof of the natural existence of such lakes.

The piece of water in question is called the Læsöskougens Vaud, or sometimes the Lesje Værks Vaud; it lies between the posting stations of Mölmen and Lesje Jernværks, at an elevation of 1,992 Norwegian feet, or 2,050 English feet above the sea level, occupying, for a length of about seven miles, the highest part of the great valley which in its south-eastern part is known as the Gudbrandsdal, and in its north-western part as the Romsdal. There can be no doubt that from the eastern extremity of the lake flows a small stream, which forms one of the sources of the Laagan or Logen River, while from the western extremity descends a much larger stream, which is the principal source of the river Rauma. Since the Logen, after passing through Miösen Lake, becomes a part of the great river Glommen, and thus falls into the Skaggerat at Frederichshald, while the Rauma reaches the sea through the Romsdal Fjord, it follows that the whole of the south-western part of Norway is encircled by water.

On examining the eastern exit of the lake, however, it soon becomes apparent that the outflow is artificially regulated. The water is retained at this end by a great barrier of boulders, gravel, and sand, which has doubtless been heaped up by glacial action. At the north-eastern extremity this barrier is narrowed until it resembles an artificial embankment, and at this point a channel has apparently been cut for the purpose of supplying water power to the works situated immediately below. The actual stream of water forming the first source of the river Logen had a depth at the time of my visit of three feet, with a width of about six feet; it flowed through a rectangular channel, paved at the bottom and sides with large boulders, and sustained by timbers. Although these timbers are now nearly rotted away, it is evident that the channel had at some time or other been carefully formed. The water power is at present used for a saw-mill, but it was, no doubt, originally employed to furnish the blast for an old iron furnace, which has given the name of Lesje Jernværk to this place. The furnace has been abandoned, as I was informed, for the last eighty years, and from the dates upon the ironwork of a neighbouring house I think it likely that the works were erected at least 150 years ago, a length of time which would perhaps be sufficient to account for the natural appearance of the stream below the works.

I also examined the western exit of the lake with care. The first break in the level of the water occurs at a wooden bridge which slightly restrains the outflow. The stream flows strongly here, with a width in all of about 45 ft., a maximum depth of about 2 ft. 9 in. at the time of my visit, and an average depth of about 2 ft. After falling about 9 in. at this point, the river flows in a steady deep stream through a perfectly natural channel for about an English mile, with a very slight fall, after which its descent becomes gradually accelerated. I have no doubt that this considerable stream forms the natural outlet of the lake, but that a lowering of the water in the lake to the extent of three or four feet would stop this outflow altogether.

Now when we speak of a lake with two outfalls, I presume we mean one with two natural and permanent outfalls, and in this sense the Læsöskougens Vaud cannot be adduced as an instance at the present day. It is just possible that the lake had a natural outlet at Lesje Værks before the artificial channel was cut, but it is highly improbable, and we should require good traditional or documentary evidence to that effect before we could assume it to be so. Such evidence would probably be very difficult to obtain, and could only be obtained by some person intimate with the Norsk language. Moreover, I judge from the nature of the outfall at this end, that if it were not looked to from time to time, the stream would eventually widen and deepen the channel through the barrier of loose sand and gravel, and finally lower the level of the water by many feet, so as to destroy the outflow into the river Rauma.

I write the above without having previously entered into the subject, and without being able to refer to any information about it. On *à priori* grounds it seems very unlikely that there should exist any lake with two distinct outflows. For in order that such a state of things should exist permanently, either there must be no erosion of the channels whatever, or the erosion must proceed with exact equality, otherwise one stream will augment at the expense of the other, and its eroding power being thus increased, it will more and more tend to sap the supplies of the other stream. The condition of things would, in fact, be that of unstable equilibrium, which could not long continue to exist.

Colonel George Greenwood, who is, I presume, the same as the former active correspondent about this subject, visited this lake last summer, as appears from the entry of his name in the day books. I am not aware that he has since published any opinion, but the lake seems, so far as I can judge, to support his view of the matter.

W. STANLEY JEVONS

THE NEW BIRD OF PARADISE

AT the last scientific meeting of the Zoological Society of London for the past session, I had the pleasure of exhibiting and describing specimens of a new Bird of Paradise recently discovered by Signor Luigi Maria D'Albertis, in New Guinea. As it will be some time before the part of the Society's "Proceedings" containing the record of the business transacted at the meeting on June 17 can be issued, and as I am informed that some knowledge of the existence of this singular bird has been obtained in another quarter, I am anxious to secure to Signor D'Albertis the honour of his discovery by a somewhat earlier publication of such a description and figure as will enable the bird to be recognised by other naturalists.

*Drepanornis** *albertisi*, as I have proposed to call this fine bird, in honour of its energetic discoverer, belongs to the long-billed or Epimachine section of the Paradiseæ, and is, perhaps, more nearly allied to *Epimachus* than to any other described form. But it is very distinct from *Epimachus* as regards its long, thin, and much curved bill, shorter legs, and shorter, squarer tail, not to speak of the peculiar tufts of feathers which are characteristic of the male sex only. The general colour of the plumage of the male *Drepanornis* is brown above, and lavender-grey below. The naked rim round the eye, and a bare space at the back of them on each side, are of a bright blue. On each side of the front before the eye rises a short tuft of bright, coppery, metallic green feathers. A large patch of similar scaly feathers covers the chin and throat. Two large tufts of feathers spring from each side of the breast, and form conspicuous ornaments when erected. The upper pair of these peculiar tufts have a mass of brilliant coppery red at the base of their feathers, terminated by a dark band. This metallic colour is only exposed when the plumes are raised. The lower pair of tufts, which are much lengthened, and in a state of repose reach beyond the lower third of the tail, are margined by a splendid purple band. The lower part of the breast is likewise crossed by a narrow band of bright green. The middle of the belly and vent are white, the tail of a nearly uniform pale chestnut.

The above description will give some idea of the special peculiarities of the male *Drepanornis* in full plumage. The female, as is the case in all the true Paradiseæ, is very different in colour, though alike in form. Her plumage is above of a nearly uniform bright brown or rufous, below paler, and crossed on the throat, breast, and sides of the belly, by numerous small irregular black wide cross-bars. The naked space round and behind the eye is coloured bright blue, as in the full-plumaged male. The beak, in the single specimen sent, is still longer than in the male, but this may be an individual peculiarity. The whole length of the male *Drepanornis*, from the tip of the bill to the end of the tail, is about 14 in., that of the wing, from the carpal joint, 6 in., of the tail, from the base, 5½ in., the outer tail feathers being about 1 in. shorter than the middle pair. The bill measures 3½ in. from the front along the curvature, the tarsus 1¼ in.

The figure of the *Drepanornis* herewith given is reduced from the lithograph prepared for the "Proceedings" of the Zoological Society, which will form the 47th plate of the volume for 1873, and will be published as soon as the second part is ready.

Signor D'Albertis obtained his examples of this remarkable bird during his recent excursion into the interior of New Guinea, at a place called Atam, which is situated at an elevation of about 3,500 feet above the sea-level in the Arfak mountains. In an account of his journey recently published in the *Sydney Mail*, he speaks thus of the present species:—

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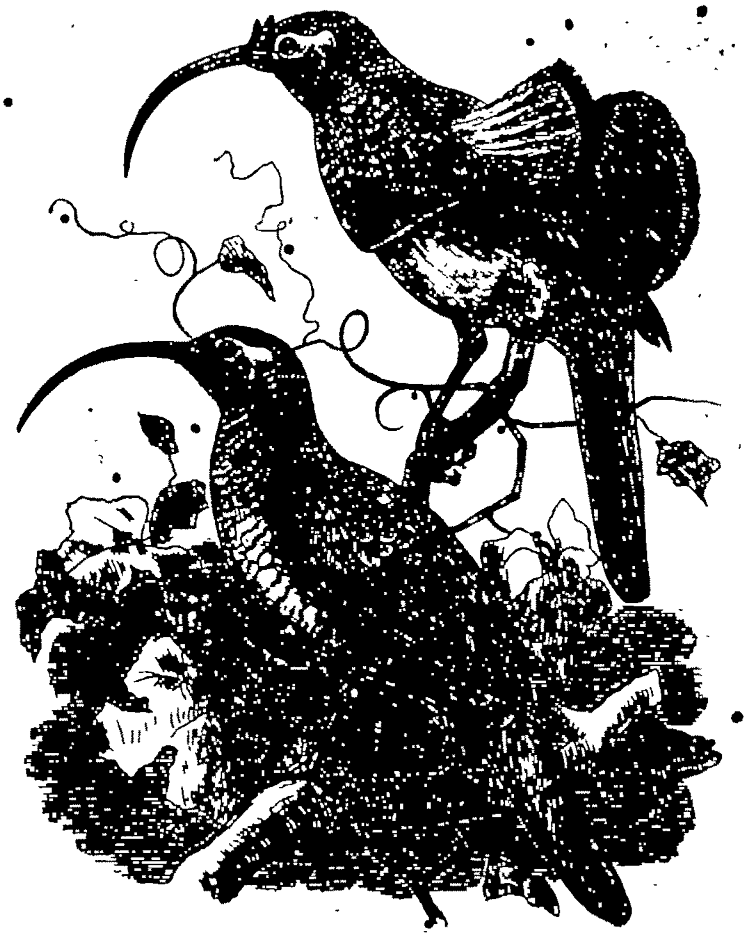
* The name originally given at the Zoological Society's meeting of June 17 was *Drepanephorus* ([illegible]) *falcem gerens*. (See NATURE, vol. viii. p. 195.) But this term having been previously applied by Sir Philip Egerton to a genus of fossil fishes, I proposed (NATURE, viii. p. 192) to convert the bird's name into *Drepanornis* ([illegible] *falx et* [illegible] *avis*).—P.L.S.

"Among other birds obtained at Atam, I may mention a new species of Bird of Paradise-bird which perhaps may even prove to be of a new genus. I secured only a male and female, which have been transmitted to the Zoological Society of London by the last April mail steamer, and they are unique specimens. It is evidently a very rare bird, for many of the natives did not know it, but others called it *Quama*. The peculiarity of this bird consists in the formation of the bill, and the softness of the plumage. At first it does not appear to have the beauty usually seen in the birds of this group, but when more closely observed, and under a strong light, the plumage is seen to be both rich and brilliant. The feathers that arise from the base of the bill are of a metallic green and of a reddish copper-colour; the feathers of the breast, when laid quite smooth, are of a violet-grey, but when raised, form a semicircle round the body, reflecting a rich golden colour. Other violet-grey feathers arise from the flanks, edged by a rich metallic violet tint; but when the plumage is entirely expanded, the bird appears as if it had formed two semicircles around itself, and is certainly a very handsome bird. Above the tail and wings the feathers are yellowish, underneath they are of a darker shade. The head is barely covered with small round feathers, which are rather deficient behind the ears; the shoulders are of a tobacco-colour, and underneath the throat of a black blending into olive colour; the feathers of the breast are violet-grey, banded by a line of olive, and those of the vent white. The bill is black, eyes chestnut, and the feet of a dark leaden colour. The food of this bird is not yet known, nothing having been found in the stomachs of those I prepared but clear water."



The new Bird of Paradise, *Drepanornis Albertisi*. Upper figure, Male; lower figure, Female.

Besides this Paradise-bird, M. D'Albertis procured from the natives, in the vicinity of Orangeri Bay, on the western coast of New Guinea, opposite to Salawatty, two imperfect skins of a second apparently new species. This is a true Paradisea, nearly allied to the Greater and Lesser Birds of Paradise (*P. apoda* and *P. papuana*), but having the long lateral plumes more of an orange-red, as in *P. rubra*. These skins were likewise exhibited at the Zoological Society's meeting on June 17 last, and the species, in accordance with M. D'Albertis' wishes, was proposed to be called *Paradisea raggiana*, after the Marquis Raggi.

As the collection of birds which contained these two new Paradise-birds only reached me on the morning of the same day as the meeting of the Society, it was not possible to make an accurate examination of all of them before the meeting, and the two Paradise-birds, being the most remarkable among the novelties, were alone described. But I have now had time to examine the whole series carefully, and find that it contains 70 specimens referable to 53 species. Twelve of these (besides the two Paradise-birds) appear to be new to Science, and will be described and named at the first meeting of the Zoological Society in the autumnal session. Besides these novelties there are examples of several other birds recently described by Dr. Schlegel from Rosenberg's collections, and of other rare species.

P. L. SCLATER

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE *

II.

IT has already been mentioned that the gravitation or weight of bodies varies with their density, and the density of the medium in which they are placed. In order to ascertain the true relative weight, as well as the actual weight of standard weights differing in density when they are weighed in air, it is necessary to allow for the weight of air displaced by each. It thus becomes necessary to reduce these weighings to a vacuum, *by deducting from the apparent weight in air the weight of the volume of air displaced by each standard.*

But the weight of a given volume of air is necessarily more or less according to its temperature, the pressure of the atmosphere, and other conditions affecting it; and the following data are requisite for ascertaining the weight of air displaced by each standard.

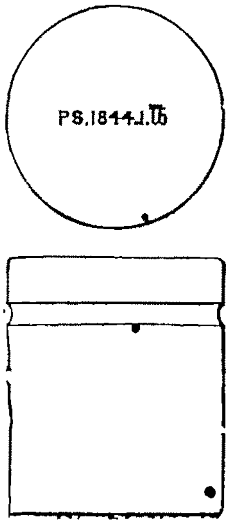


FIG. 1—Imperial Standard Pound of Platinum. Diameter = 1·15 inch. Δ = 21·1572. Displaces 0·403 grains of air.

Upper Surface of Platinum Standard Pound shown.

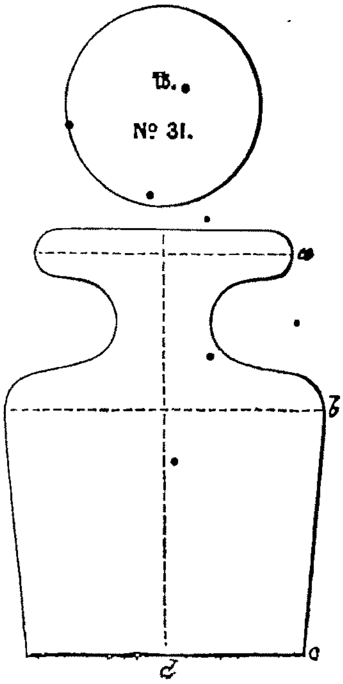


FIG. 2.—Official Standard Pound, Gilt Gun Metal. No. 31.
Size: Diameter at *a* = 1·25 inch.
" *b* = 1·65 "
" base *c* = 1·47 "
Height *d* = 2·2 inches.
Δ = 8·5144
Displaces 1·001 grain of air.

Upper Surface of Gilt Gun Metal Standard Pound shown.

1. The mean temperature of the air during the weighings.
2. The mean barometric pressure reduced to 32° Fahr. and corrected for the pressure of vapour and of carbonic acid gas in the air.
3. The density of the metal of which each standard weight is composed.
4. The co-efficients of expansion of the metals and of air.
5. The relative weight of each standard.

From data 1 and 2 the ratio of the density of the air to the maximum density of water must be ascertained. This ratio is also affected by the height above the mean level of the sea, and the latitude of the place where the comparison is made, as the force of gravity differs accordingly. But in practice the determination of the weight of air displaced in weighing is easily and quickly effected, either by the more accurate mode of making the computations from the above-mentioned data, with the aid of a logarithmetical formula and tables for reduction of weighings, or approximately by special tables showing the mean weight of ordinary air displaced by standards of various densities. The mean ordinary air taken as the standard air in this country is of the normal temperature of 62° Fahr., the barometer being at 30 inches, with the mercury reduced by computation to the temperature of 32° Fahr., the amount of aqueous vapour in the air being assumed to be two-thirds of the quantity in saturated air, and the amount of carbonic acid contained in it being taken at 0·0004 of its volume.

The actual mode of ascertaining the weight of air displaced by standard weights when compared by weighings in air, will be described more at length afterwards. But some illustrations may here be given of the effect of the difference of density in standard weights, upon their weight in ordinary air. The following 1lb. avoirdupois weights are of the actual form and size:—

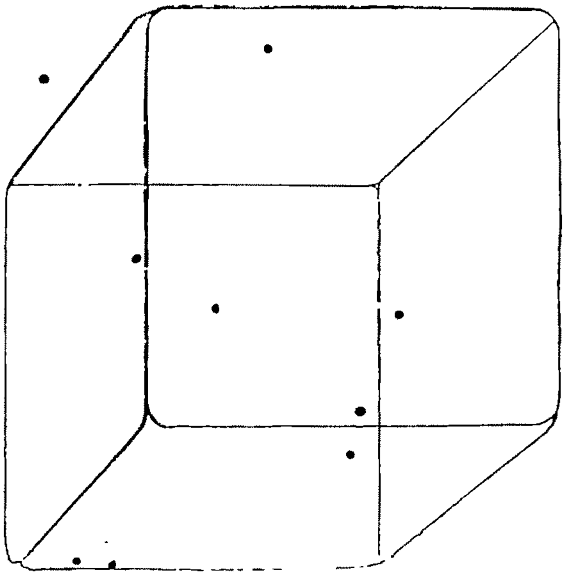


FIG. 3.—Quartz Pound in Standards Department, bearing no mark. Size = 2·17 inches cube, edges rounded. Δ = 2·6505. Displaces 3·216 grains of air.

It may here be seen that the difference of air displaced by the imperial standard lb. P.S. (Fig. 1), and the gilt gun metal lb. No. 31 (Fig. 2), is 0·598 gr.; and if they were equal in weight when in a vacuum, No. 31 would be 0·598 gr. lighter in air of the given density. No. 31 is one of the gilt gun-metal secondary standard weights, intended to regulate the weighings in air of all commercial weights. As the primary platinum standard P.S. from its greater density displaced so much less air than ordinary brass and iron weights—the density of cast-iron being about 7·408, and a cast-iron lb. displacing about 1·150 gr. of air—the weight of all the gilt gun-metal lbs., of which No. 31 was one, was referred by Prof. Miller to a theoretical commercial standard lb. of brass of the average density of brass and bronze weights (Δ = 8. 143), and thus displacing 1·047 gr. of standard air. This commercial standard lb. denoted as W was assumed to be of the same weight in a vacuum as P.S., and consequently in standard air P.S. was 0·644 gr. heavier than W.

The standard pound of quartz (Fig. 3) displaces 3·217 grains of air. It was constructed as an auxiliary standard on account of the invariability of quartz, and its apparent

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* Continued from p. 270.

weight in air was made intermediate between that of a pound of platinum and a pound of brass, being 0·401 gr. lighter than P.S., and 0·232 gr. heavier than W. in standard air.

As the determination of the density of bodies has thus been referred to the maximum density of an equal volume of water, it was evidently necessary to determine the absolute weight of a normal measure of water at its maximum density, in order to determine the true weight in air of a given volume of any substance, the density of which has been ascertained. It is claimed to be one of the important advantages of the decimal metric system, that this relation may be at once ascertained from the circumstance of the unit of weight, the kilogram, having been determined by its being the weight of a cubic decimetre of pure water at its maximum density. Thus the volume of any body expressed in cubic decimetres, or the measure of capacity of liquids expressed in litres, the litre being the measure of a vessel holding a cubic decimetre of water at its maximum density, when multiplied by its density, at once gives the weight in kilograms; or, if expressed in centimetres, the weight will be given in grammes. There is not the same simple relation between the unit of weight and of volume or capacity in the imperial system, the same definite ratio not being established between the unit of cubic capacity derived from the unit of length and the unit of weight, which is found in the metric system. This relation has therefore been determined experimentally in England from ascertaining the weight of a cubic inch of pure water, and the determination by Sir George Shuckburgh in 1798 was accepted by scientific men in this country, and has been legalised by Statute, by which a cubic inch of water at the temperature of 62° F. weighed in air of the same temperature, with the barometer at 30 inches, weighs, 252·458 grains of brass. From this ratio, the cubic capacity of the standard gallon, containing 10 lbs. weight of water, is declared to be 277·274 inches, and a cubic foot of water is declared to weigh 62·321 lbs. avoirdupois. But this ratio does not agree with that adopted in France, nor indeed with other and different ratios adopted in Sweden, Austria, and Russia respectively, as determined from separate experiments made in each of these countries. As respects the metric system, even assuming the weight of a cubic decimetre of water to be exactly a kilogram according to its theoretical definition, as to which doubts exist, it is only equal to this weight when the water is at the temperature of about 39° F. or 4° C, and when weighed in a vacuum. When of the ordinary temperature (say, 62° F.) and weighed against brass weights in ordinary air (say, the barometer at 30 inches), it would weigh not a kilogram or 1,000 grammes, but about 999·012 grammes, the difference being the loss of weight by the weight of air displaced by a cubic decimetre of water. According to the English ratio, the cubic decimetre of water would weigh in air 999·515 grammes. And if the French ratio were applied to our imperial measures a cubic inch of water would weigh 252·336 grains, the capacity of the gallon would be 277·141 inches, and the cubic foot of water would weigh 62·291 lbs. But in point of fact, a new and authoritative international determination of the weight of a standard unit of water is very much needed, in order that its true weight may be satisfactorily ascertained and uniformly adopted in all countries.

II.—*Standards of Imperial Weight and Measure*]

The English standard units of weight and length, the pound and the yard, have come down to us from the Saxons. The Mint pound of the Tower of London, which continued to be the legal unit of weight up to the time of Henry VIII., was the old pound of the Saxon Moneyers in use before the Conquest; whilst the earliest recorded standard of length in this country was the yard or *gird* of the Saxon kings, kept at Winchester. King Edgar is recorded to have decreed, with the consent of his Wites, the standard." No change was made by the Normans in the system of weights and measures established in England, and by a statute of William the Conqueror it was ordained that the measures and weights should be true and stamped in all parts of the country, as had before been established by law.

The old Tower pound was the ancient pound sterling of silver, containing 20*s*., each of 12*d*. or pennyweights. It was also divided into 12 ounces, and was thus used as the apothecaries' weight. The Tower pound was less than the Troy pound by 15 dwt., and contained 5,400 Troy grains. It was discontinued by law in the 24th year of Henry VIII., the Troy pound, which appears to have been first introduced into this country from France at the close of the reign of Edward III., being substituted for it. The mark of 8 ounces was ⅔ of the Tower pound, and was identical in weight with the ancient unit of money weight in Germany, known as the Cologne Mark. The Tower pound was also nearly identical in weight with the ancient Alexandrian pound, the 125th part of the Great Talent of the Ptolemies, from which it was probably derived. The Troy pound is said to have owed its origin to the Arab *roth* or pound of the Caliph Almamoun, of very nearly equivalent weight, sent as a present to Charlemagne.

The earliest English weight for heavy goods was the merchants' pound, declared in a Statute of Henry III. to be equal to 25*s*., or one-fourth more than the Tower pound. It must thus have been equal to 6,750 Troy grs. Another ancient authority declared the merchants' pound to have contained 15 ounces, and if these were Troy ounces this merchants' pound must have contained 7,200 Troy grs. The merchants' pound seems to have merged insensibly into the avoirdupois pound of 7,000 Troy grs., not later than the time of Edward III. It is certain that commercial pounds nearly equivalent to each of the three weights here specified were largely used in different parts of France and Germany. Our existing avoirdupois pound can be distinctly traced to the time of Edward III.; and there is good ground for believing that no substantial difference has occurred in its weight, or that of the Troy pound, since either of them was first established as a standard in this country.

There can also be little doubt that the length of the English yard has continued unchanged from the earliest times. The standard yard of Henry VII., which is still preserved in the Standards Department, is hardly $\frac{1}{100}$th of an inch shorter than the imperial standard yard, and being an end-standard, it must have lost a little of its original length. The standard weights and measures made in the eleventh year of Henry VII., which are the earliest English standards now known to exist, are all declared to have been taken from the older standards of the Exchequer, as were also the later standards of Queen Elizabeth, which continued to be the legal standards of the country up to the year 1824. Although there is no direct evidence of the origin of the Saxon yard, it is highly probable, from its length agreeing very nearly with that of double the natural cubit (of about 18 English inches) and from its third part, the foot, being very nearly identical with the ancient Egyptian and Greek foot, that these two English unit measures of length owe their origin to the cubit of a man, the earliest known standard measure of length recorded in ancient history.

The Troy pound was the standard unit of weight in this country from the time of Henry VIII. up to the year 1855, when the imperial pound avoirdupois was made the legal standard of weight. The actual primary units of imperial weight and measure are now the standard pound avoirdupois and the standard yard in the custody of the Warden of the Standards, and deposited at the Standards Department, Old Palace Yard, Westminster. They were constructed under the superintendence of the Standards Commission, appointed in 1843 for the restoration of the standards of weight and measure which had been or Council, that "the measure of Winchester should be placed in the custody of the Clerk of the House of Commons, and were destroyed by the burning of the Houses of Parliament on October 11, 1834. The members of this Standards Commission had previously given their services as a preliminary committee, having been appointed in 1838 to consider the steps to be taken for restoring the standards, the Act of 1824 (5 Geo. IV. c. 74), under the authority of which the lost standards had been legalised, having directed that, in the event of their loss or destruction, new standards should be constructed in accordance with provisions contained in the Act, by reference to an invariable natural standard.

These provisions were as follows:—In regard to the Standard of Weight, it was recited in § 5 of the Act, that a cubic inch of distilled water, weighed in air against brass weights, at the temperature of 62° Fahr. the barometer being at 30 inches, had been determined by scientific men to be equal to 252·458 grains, of which the Standard Troy pound contained 5,760; and if this Standard were lost or destroyed, a new Standard Troy pound was to be constructed, bearing the same proportion to the weight of a cubic inch of water, as the Standard pound bore to such cubic inch of water.

It will thus be seen that the new unit of weight was declared to be dependent upon the new unit of length, it being based upon the capacity of the cubic inch, or the cube of the thirty-sixth part of the Standard yard.

With respect to the Standard unit of length, § 3 of the Act recited that the Imperial Standard yard, when compared with a pendulum vibrating seconds of mean time in the latitude of London, in a vacuum at the level of the sea, had also been determined to be in the proportion of 36 inches to 39·1393 inches, and it was provided that if lost or destroyed, a new Standard yard should be constructed bearing the same proportion to such pendulum, as the Imperial Standard yard then bore to it.

After long deliberation, the Committee made a very full Report, dated December 21, 1841, and declared their opinion that the several elements of reduction of the pendulum experiments referred to in the Act of 1824, were doubtful or erroneous. It was evident, therefore, that the course prescribed by the Act would not necessarily reproduce the Standard yard. It appeared also that the determination of the weight of a cubic inch of water was still doubtful, differences being found between the best English, French, Austrian, Swedish and Russian determinations amounting to about $\frac{1}{1500}$ of the whole weight, whereas the results of the mere operation of weighing might be determined within $\frac{1}{1000000}$ of the whole weight. The Committee were fully persuaded that with reasonable precautions, it would always be possible to provide for the accurate restoration of Standards by means of material copies which had been compared with them. And they had ascertained that several measures existed which had been most carefully compared with the former Standard yard; and several weights, which had been most accurately compared with the lost Standard pound; and by the use of these, the values of the original standards could be restored without sensible error.

They recommended that no change should be made in the values of the primary units of the weights and measures of the kingdom, or in the meaning of the names by which they were commonly denoted; that the construction of the Standards be entrusted to a Committee of scientific men, under certain instructions contained in the Report, and by comparison with the most carefully selected specimens; that the Parliamentary standard of length be one yard, there appearing no sufficient reason for departing from the length hitherto adopted for the standard; and that the Avoirdupois pound be adopted instead of the Troy pound as the Parliamentary standard of weight, the avoirdupois pound being invariably known and generally used, and the Troy pound being wholly unknown to the great mass of the British population, and comparatively useless. They also recommended that no new specific standard of capacity be established, the unit of capacity, the gallon, being continued to be defined by its containing 10 lbs. weight of distilled water, as specified in the Act of 1824.

Many other important recommendations were also made by the Committee in relation to the official Secondary Standards, and the verification and legalising of local Standards for the use of Inspectors of Weights and Measures throughout the country, and for the Colonies, in order to secure the requisite uniformity in commercial weights and measures, and their accordance with the scientifically constructed primary standards.

For more effectually carrying out these recommendations for the construction of the new Standards, the Standards Commission was appointed on June 20, 1843, and continued their labours until 1854, their definitive Report being dated on March 28 in that year.

The preliminary Committee was composed of the following scientific men:—G. B. Airy, Astronomer Royal, Chairman (now Sir G. B. Airy, K.C.B., and President of the Royal Society); F. Baily, V.P.R.S.; J. E. D. Bethune; Davies Gilbert, V.P.R.S.; J. G. S. Lefevre (now Sir J. G. S. Lefevre, K.C.B.); J. W. Lubbock (afterwards Sir J. W. Lubbock, Bart.); Rev. G. Peacock, F.R.S. Dean of Ely and Lowdian Professor of Astronomy; Rev. R. Sheepshanks, F.R.S.; Sir J. F. H. Herschel, Bart. With the exception of Mr. Davies Gilbert, who died in the meantime, all these scientific men continued their services as members of the Commission for constructing the new Standards. The Marquis of Northampton, P.R.S., Lord Wrottesley, F.R.S., and Prof. W. H. Miller were also appointed members of the Commission. On the death of the Marquis of Northampton, the name of the Earl of Rosse, his successor as President of the Royal Society, was added.

H. W. CHISHOLM

OREODON REMAINS IN THE WOODWARDIAN MUSEUM, CAMBRIDGE

In addition to the valuable collection of recent skeletons lately given by Lord Walsingham to the University of Cambridge, he also presented a series of mammalian remains from the Miocene deposits of the Mauvaises Terres in Nebraska. These were, fortunately, for the most part brought to England in masses of the original rock, and have therefore had the great advantage of Mr. H. Keeping's care and skill in developing them from the matrix. His long-continued labour has resulted in the most interesting collection of fossils referred to in this notice, and now deposited in the Woodwardian Museum. Professor Hughes has entrusted me with the examination and determination of the remains, and has afforded me every possible assistance. The species revealed, some of which may possibly require the establishment of a new genus, at any rate appear to be new to science, and much larger than any hitherto described in America. We have thought that, pending the preparation of a complete description, your readers would be interested in a general account of the fossils; and especially it has been thought desirable that an account of the skull and dentition should be given in as simple a form as possible; for I have not yet seen any description of the skull other than the complete one of Prof. Leidy. At any rate, fresh interest will be excited in the Oreodontidæ now that so splendid a series of remains can be seen in an English Museum.

A summary of our fossils may be thus given:—

1. A large nearly complete skull, with lower jaw attached; the zygomatic arches being, however, almost destroyed.

2. The greater portion of a large skull preserving very completely one zygomatic arch with posterior crest.

3. Another skull of the same species showing the part anterior to the bifurcation of the sagittal crest.

4. Another large skull of the same species, wanting the greater part of the face.

5. A nearly complete skull of another species.

6. The greater part of two skulls of *Oreodon Culbertsoni* (the original and typical species), smaller than any of the above.

7. Half of the frontal region of an individual larger than any of the others.

8. Casts of the brain of a large and of a small species, with determinable parts of bones attached.

9. Many pieces, more or less complete, chiefly parts of upper and lower jaws with teeth, including a number which show the canine and incisor teeth.

10. Portions of limb bones, and a number of vertebræ.

Besides these, the collection includes Carnivorous, Rodent, and other very interesting remains.

"The deposits of the Mauvaises Terres," says Prof. Leidy, "are remarkable for the great quantity of fossil remains of mammals and turtles they have yielded without further exploration than picking them up from the surface of the country. Detached from the neighbouring soft and readily disintegrating rocks, the fossils lie strewn about, and have often attracted the attention of the least curious of those who have traversed the district. Many of the loose fossils have gradually been collected by travellers and others, so that few of a conspicuous character, I am told, now remain. Of those collected, by far the greater part have been submitted to my investigation, and these have amounted to the enormous quantity of between three and four tons in weight." The first description of fossils from the Mauvaises Terres, was by Dr. Prout, who, in 1846 and 1847, described a jaw of a large animal supposed to be a Palæotherium, in the *American Journal of Science and Art*. Gradually specimens came to light, many of which were described by Prof. Leidy, who collected and completed his descriptions in 1852, when he published, in the Smithsonian Contributions, "The Ancient Fauna of Nebraska," of 126 pages, and 24 splendid plates. In succeeding years the Mauvaises Terres were further explored by Dr. David Dale Owen, Dr. John Evans, and Dr. F. V. Hayden, who brought to Philadelphia large collections of fossils. Altogether Prof. Leidy supposes that he has seen entire skulls or portions of skulls of about 500 individual Oreodonts, a very large proportion of which belong to one species, *Oreodon Culbertsoni*. In 1869 the results of his twenty years' labour were published as the seventh volume of the second series of the "Journal of the Academy of Natural Sciences of Philadelphia," under the title of the "Extinct Mammalian Fauna of Dakotah and Nebraska," 472 pages, and 29 plates, large quarto. This great work includes also a synopsis of the entire mammalian remains of North America, with the most complete references and the author's valuable critical opinions. The interest is not merely in the artiodactyle ungulates, but also in the perissodactyles, including the famous Hipparion and Anchitherium, as well as the Rhinoceros, Machairodus, Mastodon, and Edentate remains. Quite recently Prof. Marsh has described a new medium-sized species of Oreodon in the current number of the *American Journal of Science and Art*.

The family Oreodontidæ is characterised by the possession of an elongated massive skull, of which the portion in front of the articulation of the lower jaw constitutes more than three-fourths. The upper surface slopes gradually from behind forwards. Posteriorly is a high sagittal crest (1½ in. at the greatest height in large species), reaching far back, so as to project on a level considerably behind that of the occipital condyles. The crest is flanked by large and wide temporal fossæ, their floor being chiefly formed by the squamous bone, which is internally strongly convex, and bears a blunt ridge proceeding from behind forwards, downwards, and outwards. The sagittal crest bifurcates anteriorly to form the postero-lateral sides of a nearly flat lozenge-shaped frontal region, whose lateral angles overarch the completed bony orbit. The upper surface of the face is terminated by elongated convex nasals, which extend, I think, quite to the level of the front of the premaxilla, and project further in the middle line than at the sides. The nasal cavities are very large, high at the anterior opening, and do not open laterally on the face near the orbit. They have complicated turbinals. The frontal region is alternately gently convex and concave, being more convex near the lateral angles. The frontals have, near the middle line on each side, a considerable supra-orbital foramen, appearing at about the level of the posterior boundary of the orbit.

On the lateral aspect of the skull there is first to be noticed the lateral occipital crest which extends outwards and backwards, as the outer margin of the post-occipital fossa, which varies in size. It then bifurcates, giving an inferior branch continuing the margin of this fossa, and a lateral branch which passes far outwards, bounding the great temporal fossa. This ridge rises higher as it recedes from the occipital region, and external to the articulation of the lower jaw developes into a curved crest, which is remarkably large and thick in one specimen. Further forward this crest does not exist. The widest part of the skull is just in front of this; in one of our species the width at this point is twice as great as the distance from the occipital to the orbit. The zygomatic process of the squamosal comes forward to the under part of the orbit, and is received into a long concavity of the malar. The latter passes above this process, to join the post-orbital process of the frontal, and bound the large oval or circular orbit. The malar is often of great vertical depth, and joins a prominence of the maxillæ above the alveoli of the posterior molars. Inside and above this elevation, the lachrymal occupies a considerable space on the face, and has an antorbital fossa of varying size. Anteriorly the face continues comparatively high, generally convex, and nearly vertical.

The base of the skull presents the occipital condyles, which have their anterior and posterior portions obliquely bent upon each other at an acute angle; they approach very close to one another in the median line below. The basi-occipital has a strong raised median ridge, which gradually dies away on the basi-sphenoid. The basi-cranial axis is set at an angle of about 40° to the palatine axis. Externally there is a large nipple-shaped post-glenoid process of the squamosal (the transverse diameter being the greater). Immediately on its inner side is a large auditory bulla, somewhat compressed; and applied to its external surface, and at the same time nearly touching the post-glenoid process is a long and strong paroccipital. The external meatus opens obliquely upward in front of the paroccipital.

Between the teeth, the palate is of almost uniform width, is regularly concave, and smooth. It extends for some distance behind the molar teeth, being narrowed; and has a concave posterior margin of different form in the various species. The pterygoid continues the lateral part of the concavity to the alisphenoid region.

The horizontal ramus of the mandible is of moderate height, each half being separated slightly from the other in the specimens. The symphysis is considerable, and shows serrated sutures. The anterior end of the mandible is very little diminished in height, has less of the spatulate form than ordinary ruminants, and is somewhat expanded in consequence of the size of the canines. The rami are very nearly parallel throughout their whole extent. The ascending ramus is high, with a small coronoid process, and a transversely elongated condyle.

The dental formula is—

$$i. \frac{3-3}{3-3} \; c. \frac{1-1}{1-1} \; p.m. \frac{4-4}{4-4} \; m. \frac{3-3}{3-3} = 44.$$

In the middle line above there are six small somewhat chisel-shaped incisors, increasing in size from within outwards. Next succeeds a large curved conical canine, flattened on its external aspect, and bearing a slight median longitudinal groove. There are seven teeth in the molar series, of which the first four appear to be premolars. These teeth present characters common to most ruminant genera, the premolars showing one double crescent, and the true molars two double crescents; the convexity of the crescents being turned inwards as in the upper jaw of all ruminants. They are very square in general shape, and the crescents are very convex. The junction of the anterior and posterior crescents externally is raised into a strong column, and a similar column projects as a third lobe on the posterior molar.

In the lower jaw eight teeth appear in front; the six middle ones of about the same size as the incisors of the upper jaw, but more cylindrical. The extreme tooth on each side, homologically a canine, is considerably larger and more chisel-shaped. The upper canine bites immediately behind this tooth; and behind this again is a long curved caniniform tooth similar to the canine of the upper jaw. Three premolars and three true molars succeed. They are generally similar to those of the upper jaw, but have the convexities of the crescents turned outwards. Throughout the series of teeth there is no diastema, except just as much as will allow the canine teeth to fit compactly above and below.

The following are, roughly, the dimensions of the large skull No. 1.—Length on upper surface, 13½ or 14 inches; height posteriorly 8¼ inches; anteriorly, nearly 6 inches; length of lower jaw, 10½ inches; length of molar series of upper jaw, 6 inches.

A brief comparison with some other skulls will assist in giving an idea of the affinities of the Oreodonts. The Peccary presents perhaps the greatest number of resemblances. The sagittal ridge and frontal surface are somewhat alike, but the sagittal ridge is much longer and higher in Oreodon. The part of the squamosal (with the high crest) posterior to the glenoid cavity is similar, but not nearly so elevated or so widely diverging from the middle line. The supra-orbital foramen is on the level of the anterior, and not the posterior of the orbit. The post-occipital fossa and the condyles are very much alike; so is the narrowing of the palate behind the molars; but the palate is wider and not so long proportionally in Oreodon. The posterior edge of the mandible is similar.

But the differences between Oreodon and the Peccary are many and important; the characters of the teeth are very different: the Peccary has a large diastema; the mandibular rami are not parallel, the nasal cavities are smaller in proportion; there is no lachrymal fossa: the orbit is incomplete; there is scarcely any post-glenoid process of the squamosal.

The pig exhibits somewhat more likeness to Oreodon in the relations and size of the par-occipital and the auditory bullæ; but differs still more importantly in the wide separation of the two temporal fossæ by the intervening flat parietals.

The Camel agrees with Oreodon in the large size and close proximity of its temporal fossæ, which are separated by a sagittal crest, but the latter is low, and the floor of the temporal fossa is exceedingly convex. There are vast differences in the face, teeth, mandible, and auditory bullæ.

In the ordinary Ruminant, as the sheep, it is the face which presents most resemblances to our specimens. These consist in the shape of the nasals, the nearly vertical maxillæ, the complete orbits, the antorbital fossa of the lachrymal, the Ruminant molars, and the form of the palate between the molars. But the posterior part of the skull is very unlike. Even in the molar teeth, while the type is the same there are considerable differences which will be hereafter fully described.

The Llama is much less like Oreodon than the camel is.

The casts of brains and the limb and trunk-bones and vertebræ promise to afford very interesting matter, but I have not yet made a careful examination of them.

G. T. BETTANY

ASTRONOMICAL ALMANACS,

A COMPARATIVE HISTORY OF THE "CONNAISSANCE DES TEMPS," THE "NAUTICAL ALMANAC," AND THE "JAHRBUCH" OF BERLIN.*

I.—*The "Connaissance des Temps" of Picard and Lefebvre.*

IN 1666 a celebrated bookseller of Paris, Jean de la Caille, at the sign of the "Fontaine d'or," in the Rue Jacob, published, at his own expense, the "Astronomical Ephemerides" of Hecker, the Astronomer of Dantzig. These Ephemerides were calculated on the observations of Tycho Brahe and Kepler, according to the rules given in the Rudolphine tables—tables constructed at the expense of Rudolph II., Emperor of Germany, by Tycho Brahe, Kepler and himself. Their title was, "Johannis Heckeri Motuum Cælestium Ephemerides, ad anno 1676, ad annum 1680, ex observationibus correctis nobilissimorum Tychonis Brahei et Johannis Kepleri. Hypothesibus Physicis, tabulisque Rudolphinis ad meridianum Uraniburgicum in freto Cymbrico."

These tables gave for the meridian of Uranibourg (island of Heven, between Copenhagen and Elsinore)—which derived considerable importance from the immortal observations of Tycho Brahe—and for each day the longitudes and latitudes of the sun, of the moon, of Mercury, Venus, Mars, Jupiter, and Saturn; the longitudes in degrees and minutes for the planets and the sun, in degrees, minutes, and seconds for the moon; the latitudes in degrees. They contained, moreover, an announcement of the eclipses of the sun and of the moon for the whole period indicated, and a table of geographical co-ordinates (latitude and longitude reckoned from Uranibourg) of the principal towns.

These Ephemerides, the best that then existed, stopping at the year 1680, Picard, the creator of exact astronomy, resolved to continue them. But on account of a voyage which King Louis XIV. was about to undertake, and during which the work which Picard proposed might be useful, the French astronomer decided to advance by a year the date of his publication, and to commence with the year 1679.

The Ephemerides of Picard are thus titled:—"La Connaissance des Temps ou Calendrier et Ephémérides de lever et coucher du soleil, de la lune et des autres planètes, avec les éclipses, pour l'année 1679, calculées sur Paris, et la manère de s'en servir pour les autres élévations†; avec plusieurs autres tables et traités d'astronomie et de physique, et des Ephéméndes de toutes les planétes en figures."

This work contains the following information:—1. The time, almost to the minute, of the rising and setting of the sun and moon at Paris, for every day of the year. 2. The time of the rising and setting of the sun (every fortnight) and of the moon (every ten days) for Calais, Paris, Lyon, and Marseille. From these tables the preceding time could be calculated for every point of France. 3. Announcement of eclipses of the sun and moon. 4. The time of the passage of the moon across the meridian and the right ascension of the sun for every day of the year. We have thus the time of the tide. Besides, the solar dials could be used to obtain the hour during the night by the shadow of the moon; and indeed the time at night could be obtained by observation of the fixed stars. The same table contains the value of the equation of clocks and pendulums, what we now call the "equation of time." 5. A summary of the movements of all the planets for the year, containing little but an indication of the epochs when they were visible and of the constellations through which they passed. 6. A plate in which the preceding data were graphically traced. 7. A table of the latitudes and longitudes (adjusted to the meridian of Paris) of the principal cities of France. 8. An appendix, relating to physical questions, containing an account of the winds which prevailed in Paris for every day of the preceding year, and an exact account of barometric indications for the same period.

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* Translated from *La Revue Scientifique*, July 19.

† The word *élévation* is synonymous with *latitude*.

In 1680, Picard completed his volume by the following additions:—A note on the inquiry into longitudes (*recherches des longitudes*) by means of clocks and pendulums; a table of lengths of the pendulum corresponding to an increasing number of vibrations per second, and intended for the regulation of clocks; a table of declinations of the sun for each day (by degrees and minutes); and lastly, a table indicating the weights of the unit of volume (a cubic foot) of different substances.

These Ephemerides, although less complete, so far as pure astronomy is concerned, than those of Hecker, were, however, superior to them from a practical point of view, by the substitution of the right ascension of the sun and moon for the longitude and latitude of these bodies; it is, in fact, the right ascension and declination which are directly useful to astronomers.

Picard, who published the "Connaissance des Temps" at his own expense and his own risk, was naturally interested in the success of his work. Thus, after having sought to satisfy the wants of astronomers and mariners, he added to this publication a list of the days on which the posts to the various towns of France set out from Paris. The custom of adding to the astronomical tables physical or statistical data altogether foreign to astronomy, has been continued to the present time in the "Annuaire du Bureau des Longitudes."

Still the great labour required in editing these Ephemerides soon tired the Abbé Picard, who tried to find a successor. There was then at the college of Lisieux, at Paris, a professor of rhetoric named Pierre, who was a good astronomer, and on that account was intimate with all the astronomers of his time. The learned Abbé asked him one day if he knew any one capable of assisting him, and afterwards of carrying on the "Connaissance des Temps;" Pierre proposed Jean Lefebvre, weaver at Lisieux, who, in the intervals of leisure which his work allowed him, amused himself by reading some books on astronomy, and was familiar enough with that science to be known to Pierre, originally of the same town: he had sent the latter, among other things, calculations of eclipses which quite agreed with observation. Pierre and Picard then asked Lefebvre to calculate a table of the passage of the moon across the meridian, and this having been accurately performed, they offered him an academician's annuity to come to Paris and continue the "Connaissance des Temps." We owe to his calculations the volumes from 1684 to 1702. Profiting by the new tables of the equation of the sun of Picard and Cassini, he was able to calculate the "Connaissance des Temps" with more accuracy than had ever been done before.

To Lefebvre also are due several additions and modifications. Thus in 1686 he added a table of the exact positions of the planets, the sun, and the moon for every ten days; in 1690 he gave the immersions and emergences of the first satellite of Jupiter; in 1691 maxims in reference to the movement of a ship, a list of ports and coasts, &c. In 1692 he added a table of refractions from 0° to 90° of apparent height, calculated to a minute up to 48° and to a second from 48° to 90°, as well as a value of the declination of the needle according to the observation of La Hire.

In 1693 Lefebvre, having left Paris to take part in the geodetic operations of Picard, one of his colleagues of the Academy, Lieutaud, edited the *Connaissance des Temps* in 1693 and 1694; but on his return he resumed the editorship, and continued it without interruption till 1702.

At that time, in consequence of an incident curious enough to bear relation, the publication of the *Connaissance des Temps* was taken up by the Academy of Sciences.

The son of De la Hire, a very popular academician, who had considerable influence among his colleagues, published, for 1701, a collection of Ephemerides intended to rival those of Lefebvre, in which he said, "I hope, at least, that there will not be found here errors (*éloignements*) of calculation so great as are seen in certain popular and much praised Ephemerides," &c. Wounded to the quick by such a reproach, altogether untrue, Lefebvre wrote in the preface of the *Connaissance*, for 1701, "I cannot avoid replying to the invectives of a certain small novice [De la Hire *fils*], supposed author of an annual Ephemerides published a short time ago. This new author, filled with a spirit of vanity, presumption, and falsehood . . . We reply to this youthful novice . . ."

De la Hire, himself, was not spared. At this uncouth reply the enemy's camp winced, and resolved on revenge; success was easy, for Lefebvre was by no means a general favourite. Little by little the meetings of the Academy were rendered insupportable to him, and when he had absented himself for a certain number of meetings, his name was struck out of the lists of that body. Deprived of his Academician's pension, Lefebvre could no longer continue the *Connaissance des Temps*. The Academy then took possession of the publication, which became a public undertaking; so that the volume of 1702, instead of being, like the previous ones, dedicated to the king, is published "by order of the Academy of Sciences." The old title is changed, and it is simply called "Connaissance des Temps, pour le Méridien de Paris."

(*To be continued.*)

NOTES

In reference to the meeting of the British Association at Bradford, the Reception Room will be opened on Monday, September 15, at 1 P.M., and on the following days at 8 A.M., for the issue of tickets to members, associates, and ladies, and for supplying lists and prices of lodgings, and other information, to strangers on their arrival. No tickets will be issued after 6 P.M. On and after Monday, September 15, members, and persons desirous of becoming members or associates, or of obtaining ladies' tickets, are requested to make application in this room. In the Reception Room there will be offices for supplying information regarding the proceedings of the meeting. The "Journal," containing announcements of the arrangements for each day, will be laid on the table on Wednesday, September 17, and the following mornings, at 8 A.M., for gratuitous distribution. Lists of members present will be issued as soon as possible after the meeting, and will be placed in the same room for distribution. The first general meeting will be held on Wednesday, September 17, at 8 P.M. precisely, when Dr. Carpenter, LL.D., F.R.S., &c., will resign the chair, and the President Elect will assume the presidency, and deliver an address. On Thursday evening, September 18, at 8 P.M., a Soirée; on Friday evening, September 19, at 8.30 P.M., a Discourse; on Monday evening, September 22, at 8.30 P.M., a Discourse; on Tuesday evening, September 23, at 8 P.M., a Soirée;

on Wednesday, September 24, the concluding General Meeting will be held at 2.30 P.M. We omitted to mention in last week's number that the President of Section D, Biology, is Prof. Allmann, M.D., F.R.S.

SIR HENRY RAWLINSON has received a letter dated Khartoom, July 2, from Sir Samuel Baker. Sir Samuel expresses a hope that he will be in England in September. In reference to the oneness of Lakes Tanganyika and Albert Nyanza, he says:—"The envoys sent by M'tése all assured me that the Tanganyika is the M'wootan N'zizé (Albert Nyanza) and that Ujiji is on the eastern border; that you can travel by boat from Ujiji to the north end of the Albert Lake; but you must have a guide, as some portions are very narrow and intricate. From my experience of the high-water grass, I should expect islands and floating vegetation in the narrow passes described. I am by no means fond of geographical theories, but the natives' descriptions were so clear that I accepted as a fact that the Tanganyika and Albert Lakes are one sheet of water, with marshy narrow straits overgrown with water grass, through which you require a guide."

THE Session of the British Medical Association in London during the last week seems in all respects to have been most successful: a great many papers were read, and a great quantity of pleasuring hurried through. Many of the papers were valuable from a medical point of view, and some of importance even from a general scientific standpoint. This week we give a short abstract of Dr. Sanderson's address.

AT the annual general meeting of the Royal Botanical Society, on Monday, the Council congratulated the Fellows on the fact that since the last anniversary meeting the progress which had characterised the operations of the society during the last few years had been maintained. The number of new Fellows elected during the year was 114, being an increase of ten above that of last year; few resignations had occurred. The total number of Fellows and members at the present time was 2,502, the largest on the books of the society since its commencement. The total amount received in subscriptions was 250*l*. in excess of that of last year, and considerably above the average of the last few seasons. From the auditor's report it appeared that the total receipts for the year, including the balance of 529*l*. from the previous year, amounted to 13,434*l*. 6*s*. 11*d*., and the payments, exclusive of the balance in hand, 2,170*l*. 9*s*. 4*d*., to 11,263*l*. 17*s*. 7*d*. The report of the secretary was also read, and was equally satisfactory with the other reports. The Council for the next year was elected by ballot.

PROF. G. SCHWEIZER, Director of the Moscow Observatory, died on July 5, after a long illness.

THE death of Sir Francis Ronalds in his 86th year, at Battle, in Sussex, has just been announced. Sir Francis was well known, many years ago, for his experiments in electricity. In 1823 he published a pamphlet containing an account of some of his experiments, and explaining, with the help of illustrations, his plan of an electric telegraph. He had erected in his own garden, first at Highbury and then at Hammersmith, a number of poles supporting eight miles of wire, and through this wire he sent his messages. Each message was read at the further end by means of two needles moving on a dial plate, a plan much the same as that which afterwards came into general use. The spark in his telegraph system was however created by an electrical machine, and not, as in existing systems, by a galvanic battery. In recognition of the value of his discovery, the Government bestowed on him the honour of knighthood in 1870, when the same mark of appreciation had been conferred on Sir Charles Wheatstone for his improvement of the telegraph. Sir F. Ronalds superintended for a short time the Meteorological Observatory at Kew on behalf of the British Association, and the Government conferred upon him a small pension for his services to Science. For some years he lived in the north of Italy, studying the works of Italian writers on electricity. Lately he was engaged in his home at Battle in preparing a catalogue of the published books and papers on electrical science, which we believe is quite ready for print, and will be of great value to students.

To the notice which appeared some few weeks back stating that the large female Octopus had deposited a quantity of spawn on the rock-work of her tank, we have now to add the still more interesting intelligence of the successful development and escape of the perfected embryos. It will be remembered that the first of these eggs were deposited on June 19, and as the earliest arrivals of the young Octopods into the outer waters of their tanks took place on Friday the 8th inst., we have just eight weeks as the period of incubation. Mr. Saville Kent, having personally witnessed the congress of the two sexes in April last, we are also in a position to record an almost similar period occupied during the process of gestation, and which together constitute an important addition to our previous knowledge of the habits of the Cephalopoda. Since Mr. Saville Kent's resignation of the Curatorship, the Brighton Aquarium has unfortunately lost the older and tamer example of the two porpoises, commented upon by that gentleman in NATURE for July 17, as also the unique specimens of the Sturgeon and John Dorée, which have likewise received a share of attention from the same pen in the pages of this journal.

THE Lords of the Committee of Council on Education are about to appoint a keeper of the Natural History Department of the Edinburgh Museum of Science and Art. The salary will be 350*l*., rising to 450*l*. per annum. Candidates should apply to the Secretary, Science and Art Department, South Kensington.

THE German African Exploration Society has received a despatch, dated July 1, announcing the arrival of Professors Bastian and Goeschen at Cabinna Clougd, for which place Dr. Guesfeldt had started on June 28 from Sierra Leone. Dr. Falkenstein, Dr. Anatoin, physician, and Herr Linder, engineer, are hourly awaiting, at Berlin, further intelligence, on receipt of which they leave to join the expedition.

Xanthorrœa australis, one of the grass gum trees of Australia, is coming into flower for the first time in Europe, in the succulent-house at Kew. There is also a fine plant of *Agave jacquiniana*, removed to the palm-house for the sake of space, which is now in full flower.

DR. PETERMANN has sent us advanced sheets of some of the articles to appear in the forthcoming number of his *Mittheilungen*. One of these gives an account of the *Polaris* Arctic Expedition under the unfortunate Capt. Hall, and points out the main scientific results, which Dr. Petermann rightly regards as of the highest importance. He animadverts with considerable severity on the conduct of the English for the last nine years with regard to Arctic exploration; we, he says, having during that time endeavoured to depreciate the efforts of others, while we ourselves have done nothing. Even the expedition of the daring Hall, he declares, we sneered at when it set out, and since its fate was known, have spoken slightingly of the results. We must acknowledge that Dr. Petermann's taunt as to our inaction during the last nine years in the direction of Arctic exploration is to some extent justified by facts; that inaction, however, is not due to the apathy of English men of Science but to the parsimony of the British Government. We have done much in the way of private effort for discovery, but no amount of private effort is equal to the fitting out of an adequate Polar Expedition. It is, we believe, the earnest desire of all classes that Government should provide the means of enabling this country to take that foremost part in Arctic exploration which was formerly hers without dispute, by fitting out a thoroughly equipped expedition, an expedition which should have for one of its aims the finding of the Pole. As to Captain Hall's expedition so far as we are aware, the high value of its results has been everywhere in this country gratefully acknowledged, as well as the indomitable bravery and enthusiasm and high intelligence of the leader; one of its most important results, for which all men of Science must be thankful, is that it has left the most practicable path to the Pole no longer questionable. That the *Polaris*, however, was ill suited for ice-navigation, and that there was a want of that thorough discipline on board, without which no expedition of the kind can hope to be perfectly successful, we still maintain is borne out by what was elicited during the official investigation. We sincerely hope with Dr. Petermann that the magnanimity and liberality of the American Government will be the means of putting an end to the "mere talk of Englishmen," and of inducing our Government at last to set about organising on the most liberal scale an expedition to leave our shores in the spring of 1874. Other papers in the forthcoming number are "With the Russian Army against Khiva," being two letters to Dr. Petermann from Lieut. Hugo Stumm, of the Westphalian Hussar Regiment, and a paper by Dr. D. Sievers, dated Tiflis, May 7, full of geographical information of great importance. The same number will contain the conclusion of Baron von Richthofen's account of his travels from Pekin to Sz'-tshwan.

THE last issued number (vii.) of Petermann's *Mittheilungen* contains the conclusion of Ernest Marno's Travels in High Sennaar; the Results of the Observations made during the voyage of the *Albert* in November and December last, by Prof. Mohn, Director of the Norwegian Meteorological Institute; and a well-constructed map of the Chinese Province of Kuang Tung, from native and foreign authorities, by Dr. Hirth, with accompanying description.

PROF. AGASSIZ, in his address to the students, at the opening of the American School of Natural History, on Penikese Island, said:—"Our chief work will be to watch the aquarium. I want you to study principally marine animals. The only way to do that properly, is to have them alive by your side. In a very few days I shall place at your disposal a series of these appliances. I have ordered one for every person admitted to the school, so that each of you will have means to make these investigations. I have never had, in my own laboratory, better opportunities for work than I place at your disposal. Our way of studying will be somewhat different from the instruction generally given in schools. I want to make it so very different, that it may appear that there is something left to be done in the system adopted in our public schools. I think that pupils are made too much to turn their attention to books, and the teacher is left a simple machine of study. That should be done away with among us. I shall never make you repeat what you have been told, but constantly ask you what you have seen yourselves." The following men of science will, it is said, assist Prof. Agassiz in the conduct of his new charge:—Dr. Burt G. Wilder, of Cornell; Dr. A. S. Packard, of Peabody Academy of Science, Salem; Count Pourtales, of the Coast Survey; Prof. Waterhouse Hawkins, of England; Paulus Roetter, artist of the Museum at Cambridge; Prof. Mitchell, of the Coast Survey; Prof. Joseph S. Lovering, of Harvard University; Prof. F. W. Putnam, of Peabody Academy of Science, Salem; Prof. N. S. Shaler, of Harvard; Prof. Arnold Guyot, of Princeton, N. J.; Prof. Brown-Séquard.

ACCORDING to the *Melbourne Argus*, H.M.S. *Basilisk*, Capt. Moresby, while cruising in Torres Straits and neighbourhood for the suppression of the Polynesian labour traffic, has added a valuable fact to the knowledge we possessed of the geography of New Guinea by the discovery of a new port and harbour in lat. 9° 30′ S., lon. 147° 10′ E., about 38 miles east of Redscar Bay, on the south-eastern coast. The discovery was made in February, when Captain Moresby, while searching for a river supposed to flow into the sea east of Redscar Bay, entered an inlet which proved to be the entrance to a magnificent harbour, with an outer and inner anchorage, to which the names of Port Moresby and Fairfax Harbour have been given. The natives are much lighter complexioned than those of the opposite coast, and are evidently of a much more friendly disposition.

A GREAT earthquake occurred at Valparaiso early on the morning of July 8. There were six shocks in succession. Many families took refuge in the streets, the damage to private houses as well as to the public buildings being considerable; and many deaths were reported. A statue lately erected to Lord Cochrane was wheeled half round on its pedestal. The earthquake was observed to come from the east, and was felt as far south as Curico.

THE *Telegraphic Journal* intends to offer to its students from time to time prizes for the best and most carefully considered paper on a given subject. The first of these students' prizes is one of 25*l.* to be awarded to the author of the best paper on "The Evidence of the Theory of Correlation of Physical Forces as applied to Electricity and Magnetism," received by the editor of the journal on or before January 1st, 1874 The funds for this prize have been kindly given by Mr. Edward Sabine, C.E. The prize paper will be printed in the columns of the *Telegraphic Journal*.

WE understand that 1,000*l.* has been generously presented to the Oldham School of Science and Art, by Mrs. Platt, widow of the late John Platt, M.P., who was its founder in 1865, and life-president. Since the opening, its artisan students have gained four Whitworth Scholarships of 100*l.* each for three years (two have been awarded this year); two Whitworth Fellowships of 25*l.* each; one Studentship at the Royal School of Mines; three gold, six silver, and five bronze Queen's Medals (the Medallists of 1873 are not yet announced). Twenty-four artisan students were examined by the Department last May, in Inorganic Chemistry—eighteen passed (nine first class, nine second class)—and twelve in Laboratory Practice. The Committee have granted funds to enlarge the Chemical Laboratory, also to establish one for practical work in Heat, Steam, Light, and Acoustics. Mr. J. T. Hibbert, M.P. for Oldham, has given a Local Scholarship of 25*l.* for the coming session. We have received a well-arranged time-table of Classes under the direction of Mr. Phythian, C.E., and Mr. Philip, M.A.

IN accordance with the resolution passed at the meeting, noted in last week's NATURE, for the promotion of technical education, at which H.R.H. the Prince of Wales presided, the Haberdasher's Company have sent to Lord Lawrence, for distribution by the London School Board, the sum of 20*l.* as their contribution towards the purchase of tickets of admission to the International Exhibition.

DURING the month of October, we learn from the *Journal of the Society of Arts*, notwithstanding the Anarchical State of Spain, an exhibition is to be held at Madrid, of national products and manufactures, of agriculture, mines, chemicals, industries, and graphic arts. Foreign products will be received by the executive at Madrid if carriage paid. Goods will be sold by the executive on a small commission charge. This is to be the first of a proposed series of Spanish exhibitions.

PROF. COPE sends us, as No. 14 of his "Palæontological Bulletins," the description of two new mammals from the tertiary "of the plains." One, *Ælurodon mustelinus*, is only known from some teeth of the molar series; the other, *Aceratherium megalodus*, is represented by a perfect cranium with dentition of both jaws nearly complete, with other bones of other specimens. The wording of the description is intricate and short.

A PAPER entitled "A Study of North American Noctuidæ, by A. R. Grote, was read on July 2 before the Buffalo Society of Natural Sciences, declaring that six new genera (Ufeus, Ablepharon, Ommatostola, Argillophora, Harveya, Spiloloma) and twenty-seven hitherto undescribed species (Agrotis, 7; Ufeus, 2; Mamestra, 1; Dianthœcia, 1; Oncocnemis, 3; Hadena, 1; Ommatostola, 1; Cucullia, 1; Xylina, 1; Heliothis, 6; Argillophora, 1; Harveya, 1; Spiloloma, 1), occur in the N. American Insect Fauna.

SIR HENRY RAWLINSON'S presidential address at the last anniversary meeting of the Geographical Society has been published in a separate form by Messrs. Clowes and Sons. We are glad to see it reproduced in a handy and well-printed form, for it contains a masterly summary of the progress of geographical knowledge during the past year.

WE have received the prospectus of what promises to be a handsome and valuable work, "The Fenland, Past and Present: its History, Geography, Geology, Natural History, Scenery, Antiquities, Climatology, Drainage, Agricultural Produce, and Sanitary Condition; illustrated with Wood Engravings, Maps, and Diagrams; by Samuel H. Miller, F.R.A.S., Fellow of the Meteorological Society; and Sydney B. J. Skertchley, F.G.S., H.M. Geological Survey." It will be published by Leach and Son, Wisbech; and Longmans, Green, and Co. London. Under the head "Fenland," the authors include that area of low, once marshy lands, in which the rivers Witham, Welland, Nene, and Ouse interlaced, including nearly 2,000 square miles, and roughly bounded by a line drawn from Lincoln by Bourn and Peterborough to Cambridge on the west; from Lincoln to Skegness on the north; from Cambridge and St. Ives to Brandon on the south; and from Brandon to Lynn on the east (thus including Boston, Sleaford, Spalding, Croyland, Thorney, Wisbech, March, Huntingdon, Ely, besides the border towns)

A VERY deserving institution has recently been established in Cincinnati, under the title of the Cincinnati Acclimatisation Society, its object being to effect the introduction of such foreign birds as are worthy of note for their song or their services to the farmer or horticulturist. The society announces that during last spring it expended 5,000 dols. in introducing fifteen additional species of birds, and that it had already successfully accomplished the acclimatisation of the European sky-lark, which is stated to be now a prominent feature of the summer landscape in the vicinity of Cincinnati. Among the species which it is proposed to introduce is the European titmouse, considered abroad as one of the most successful foes of insects injurous to vegetation.

THE addditions to the Zoological Society's Gardens during the past week include a Harnessed Anntelope (*Tragelaphus scriptus*), a Double crested Pigeon (*Lopholæmus antarcticus*), two Senegal Touracous (*Corythaix persa*), two Chilian Tinamous (*Rhynchotus perdicarius*), a White-fronted Dove (*Leptoptila jamaicensis*), a Glossy Ibis (*Ibis falcinellus*), a Mauge's Dasyure (*Dasyurus maugei*), a Barbary Ape (*Macacus inuus*), and others.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie Neue Riche, Band xci. Heft 2 und 3, June 14. This number begins with communication No. 83 from the Griefswald Laboratory, the subject of which is Phenathren, by M. Hayduck. The author describes several of the compounds of this body.—From the same laboratory we have a notice on the compound $C_{14}H_8S_2$, by C. Pauly.—B. Rathke contributes a paper on the chloro-sulphides of carbon, and another on the compounds of the amides with that body. One of these chloro-sulphides has the formula $CSCl_4$—perchloromethylmercaptan, another the formula $CSCl_2$, several of their compounds are described. The same author also contributes a short paper on the changes nitro-compounds undergo in sulpho-acids.—Messrs. Maunder and Tollens communicate a paper on β Bibromopropionic acid, in which they give an exhaustive account of this body and its compounds.—Messrs. Caspary and Tollens have converted β Bibromopropionic acid into acrylic acid and give an account of the process, and of the salts of acrylic acid.—Mr. B. Tollens communicates a paper on the constitution of the allyl and acryl derivatives.—Prof. Max. von Pettenkofer has a paper on "Nourishment in general, and on flesh extract as an essential portion of human nutriment in particular."—Messrs. Lieben and Paterné have a paper on the dry distillation of calcic formate.—J. Wislicenus communicates a paper on the optically active lactic acid of flesh extract, and on paralactic acid. The same author also communicates some observations on ethyl-lactic acid. The next paper is by C. E. Groves on the formation of naphthaquinone by the direct oxidation of naphthalene, which has already appeared in the March number of the Chemical Society's Journal. Messrs. Hlasiwetz and Kachler, in a postscript to their paper on a new derivative of sulpho-[illegible]minic acid, mention the discovery of the body in question by Zeise in 1842. H. Ranke finishes the number with some experimental proofs of the possibility of the spontaneous combustion of hay.

Reale Instituto Lombardo di Scienze e Lettere Rendiconti, serie ii. vol. vi. Fascicoli x.—We notice papers on *Pelobates fuscus*, by Prof. Emelio Cornalia; on the Italian earthquake of March 12, by A. Serpieri; on some geological theories, by G. Cantoni; on the inversion of currents in electromotors, by A. Ferrini. Besides these there are papers on Manzoni and on Kant's philosophy, the first by A. Buccellati, and the second by C. Cantoni. Fascicolo XI. contains only social papers, none of scientific interest. In Fascicolo XII., S. A. Lemoigne contributes a paper on the mechanism of rumination, and J. A. Serpieri one on the earthquake of March 12; S. A. Cantoni has a paper on the molecular movements of gases. The rest of the number is devoted to the section of moral and political science.

IN the *Annali di Chimica applicata alla Medicina* for June there is a paper on the cremation of the dead, which practice is strongly advocated. The author, who is anonymous, states that in Belgium 7,500 hectares (1 hectare = 2·47 acres) are unproductive of food, through being used as cemeteries. He estimates the value of this land at from 38 to 40 millions (lire?).

SOCIETIES AND ACADEMIES

LONDON

Royal Horticultural Society, July 16.—Scientific Committee.—Dr. M. T. Masters, F.R.S., in the chair.—A letter was read from the locomotive superintendent of the Brighton Railway stating the results of the company's experience in using a mixture of chalk with coal for fuel. It was found that used for any other purpose than that of saving the fire-bars from Welsh coal (for which it is admirably suited) or for reducing the area of heating surface it increases the ordinary consumption of fuel considerably.—The Rev. M. J. Berkeley showed female flowers of *[illegible] dumma*, in which the calyx was reduced by arrest of development to a mere rim.

August 6.—General Meeting.—W. B. Kellock in the chair.—The Rev. M. J. Berkeley commented upon the fruits and vegetables exhibited. He mentioned the remarkable improvement in the quality of W. Indian pines owing to the introduction from England of the better cultivated kinds.—Prof. Thiselton Dyer pointed out that a curious cucurbit which had lately been introduced, rather as a curiosity than for any useful purpose, under the name of Sooly Qua, was a form of *Luffa ægyptiaca*, the common washing gourd. Another cucurbit known as the Tonng Qua appeared to be identical with *Benincasa cerifera*. A new method of propagating Ipecacuanha had been devised in India by Mr. Jaffray, and promised to be of great importance. It simply consisted in striking the leaves upright in pots. These produced roots and the most superficial of these eventually produced buds.—As an interesting fact bearing upon the distribution of plants, an extract of a letter from Mr. Moseley, naturalist on board H.M.S. *Challenger*, was read. A vessel laden with grapes was wrecked on the coast of Bermuda a short time ago.

The boxes of grapes were washed ashore, and the seeds germinated in abundance, so that the governor was able to collect plants for his garden.

BERLIN

German Chemical Society, July 28.—O. Liebreich, vice-president, in the chair.—A. Laderburg described a simple way of obtaining zinc-methyl and its action on silicic ether. The result is a liquid boiling at 150° of the formula $SiCH_3(OC_2H_5)_3$, to which he gives the name ortho-silico-acetic ether. The same chemist, conjointly with Demole, has transformed chlorhydrine into acetochlorhydrine of glycol. The latter by treating oxide of ethylene with aniline has obtained a single base of the formula of phenylated mono-oxyethylene-amine $CH_2OHCH_2NHC_6H_5$.—O. Jacobsen has been able to investigate human bile obtained from a fistula of a strong and healthy man. It contained no taurocholic acid, while other human biles obtained from patients contained both glyocholic and taurocholic acids in variable proportions.—A. Faust has transformed monochlorinated phenol into resorcin (and not, as Petersen communicated lately, into hydrochinon.—H. Limpricht has compared sulpho-ortho-toluidinic acid and many of its derivatives, with those of sulpho-pseudo-toluidinic acid.—Thomas Dykes Barry described several derivatives of propiophenone $C_6H_5COC_2H_5$: viz., two isomeric mononitro-propiophenones, amido-propiophenone, and secondary propyl-benzol-alcohol $C_6H_5CH.OH.C_2H_5$.—G. Goldschmiedt, in treating benzol and bromal with sulphuric acid obtained diphenyl-tribrom-ethane $(C_6H_5)_2CH.CBr_3$. This treated with potash yields diphenyl-dibrom-ethylene $(C_6H_5)_2C_2Br_2$, and heated with zinc powder, it is transformed into stilbene $C_{14}H_{12}$.—P. Liechto has determined the atomic weight of molybdenum = 95·86, and describes the following chlorides:—$MoCl_2$, $MoCl_3$, $MoCl_4$, $MoCl_5$, and $MoO(OH)_2Cl_2$.—A. Michaëlis and G. Köthe find that iodide of lead treated with sulphite of sodium yields sulphite of lead and iodide of sodium, and that the salt formerly described by Zinero $I_2SO_2(ONa)_2$ does not exist.—A. Michaelis and O. Schifferdecker describe the following compounds of sulphur:—SCl_4, existing only at temperatures below −20°, $S_2O_3Cl_4$ (a solid body obtained by treating SO_3HCl with SCl_4), and its product of decomposition by moist air $S_2O_5Cl_2$.—A. Mitscherlich described a new method of organic analysis. He replaced oxide of copper by that of mercury, weighs the reduced mercury, CO_2 and H_2O in the ordinary way, and thus determines the oxygen contained in the substance, as well as the Cl, I, Br retained by the mercury or the sulphur and phosphorus transformed into sulphate and phosphate of mercury.—A. Borodin in treating valeric aldehyde with solid caustic potash at 0° obtained alcoholic products of condensation of the following formula: $C_{10}H_{18}O$ $C_{20}H_{38}O_2$. The former left for three years with diluted soda yielded crystals of the composition

$$C_{20}H_{42}O_3 = \underset{(polymeric\ valeral)}{(C_{10}H_{20}O_2)} + H_2O.$$

C. Engles, by treating monochlorinated acetonitrile $NC.CH_2Cl$ with aniline replaced Cl by NHC_6H_5, thus obtaining a base, anilido-acetonitril.—A. Emmerling and C. Engles have obtained from acetophenone the corresponding pinacone and secondary alcohol.—E. Baumann, by treating cyanamide with sulphuric acid and water, has obtained a body of the composition of urea, but hygroscopic giving a nitrate of a different crystalline form, and a double salt with chloride of platinum, in fact differences that seem to indicate that this body is a new compound isomeric with urea.—E. Mulder described several derivations of uric acid and of urea.—C. Tiemann compared two methods for determining nitric acid in water. The wells of Berlin yield water containing great quantities of nitric acid, viz. 17 in 100,000 instead of 0·4, which is generally admitted to be the maximum quantity allowed for drinking purposes It should be known, however, that the water-works supply the town with river water of good quality.—C. Biedermann showed beautifully coloured salts of mononitrophenol with alkalies and alkaline earths.—W. H. Pike, of London, has succeeded in obtaining some of the higher homologues of oxaluric acid by heating a molecular mixture of urea or sulpho-carbamide with an anhydride of a dibasic acid. The acids already obtained are succincarbaminic acid $NH_2—CO—NH—CO—C_2H_4—COOH$, succin-sulpho-carbaminic acid $OH_2—CS—NH—CO—C_2H_4—COOH$, and citracon-sulpho-carbaminic acid $NH_2—CS—NH—CO—C_3H_4—COOH$.—The next meeting of the society will take place the 13th of October.

PARIS

Academy of Sciences, Aug. 4.—M. Bertrand, president, in the chair.—The following papers were read:—A further portion of M. Hermites' paper on the exponential function.—A reply to M. Vicaire's theory of the sun, by M. Faye. The author controverted the statement that the sun is a cold mass of combustible matter burning at the surface only, in an atmosphere of oxygen.—On the determination of the wave-lengths of the lines in the ultra-violet, and also in the ultra-red parts of the spectrum by means of phosphorescence, by M. Ed. Becquerel.—On the action of armatures applied to compound magnets, by M. Jamin.—On the reciprocal displacements between the hydracids, by M. Berthelot. The author has been investigating the heat phenomena produced by these reactions.—Note on the cubic capacity and on the volume of air requisite to insure the healthfulness of inhabited places, by General Morin. The general gives the results of observations on barracks and hospitals. As regards the former, he thinks that 16—20 cubic metres of space are required per man, equal to 565—706 cubic feet.—The fourth part of M. A. Ledieu's paper on thermodynamics was then read.—An analysis of Dewalquite from Salm Chateau, Belgium, by M. F. Pisani.—On the Cocuyos of Cuba, by Señor de dos Hermanas. The cocuyo is a luminous insect, said by M. Blanchard, at the conclusion of the paper, to belong to the genus *pyrophorous*, to which also a Mexican insect of the same name belongs.—Memoir on cerebral localisations, and on the functions of the brain by Dr. Fournie.—On polychromic photography, by M. L. Vidal. This was a description of a recently patented method of obtaining coloured prints by the use of various pigments, as in carbon printing.—M. Lichtenstein communicated a paper on the present state of the Phylloxera question, and M. Signoret one on the evolution of the Phylloxera.—Fourth note on the maximum resistance of magnetic coils, by M. T. du Moncel.—On electric condensation, by M. Neyreneuf.—Studies on nitrification, II., by M. Schloesing.—On the corundum of North Carolina, Georgia, and Montana, by Mr. Laurence Smith.—On Roman essence of chamomile, by M. E. Demarçay.—On the characteristics of the true polyatomic alcohols, by M. Lorin.—On the variation in the amount of urea excreted under normal nourishment, and under the influence of tea and coffee, by M. E. Roux. The author found that these substances very largely increase the amount of both urea and chlorine voided in the urine, if they be taken after abstinence from them, but that when continuously used, the quantity gradually returns to its normal amount. Hence he regards this action as that of the washing out of accumulated urea.—On the uniformity of the action of the heart when that organ is free from external nervous influences, by M. Marey.—On some effects produced by lightning at Troyes, on July 26, 1873, by M. E. Parent.

PAMPHLETS RECEIVED

ENGLISH.—Improved Method of Recording Telegrams: Richard Herring.—Report of the Radcliffe Observer to the Board of Trustees, read at their meeting at Oxford

FOREIGN.—Medizinische Jahrbücher herausgeben von der K. K. Gesellschaft der Aerzte, redigirt von S. Stricker, Jahrgang 1873, Heft I and II. (W Braunmuller, Wien).—Ofversigt af Kongl. Vetenskaps Akademiens Förhandlingar, Trettionde Argangen, 1873, Nos. 2, 3, 4 (Stockholm).—Bulletins de la Société d'Anthropologie de Paris, Fasc. 1, Jan. et Feb., 1873.

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THURSDAY, AUGUST 21, 1873

THE REPORT OF THE SCIENCE COMMISSION ON THE OLD UNIVERSITIES

I.

ONE of the two Royal Commissions appointed to inquire into University matters has just issued its Report, and it comes in the very nick of time; for while on the one hand the question of University reform is day by day attracting a larger share of public attention, on the other the Financial Commission may be expected to report shortly and make us acquainted with the actual resources available for fundamental reforms which all acknowledge must be made, though opinions differ as to the precise direction they should take.

When we state that the Report to which we refer has been drawn up by a Commission, the Chairman of which—the Duke of Devonshire—is the Chancellor of Cambridge University, and that to it are appended the names of Stokes, H. J. S. Smith, Sharpey, Huxley, Lubbock, the Marquis of Lansdowne, and Mr. B. Samuelson as Commissioners, the importance of the document becomes manifest. Nor is it lessened by the way in which the Report at its outset refers to "all those parts of human knowledge and culture which are not usually regarded as having any scientific character;" adding, "Least of all should we wish to imply that there is any antagonism between the literary and scientific branches of education and research; it is rather our conviction that neither branch can be neglected without grave detriment to the other; and that an University in which the Mathematician, the Experimental Philosopher, and the Biologist are actively engaged in the endeavour to advance human knowledge in their own provinces, is not on that account less likely to be productive of original labours in the fields of Literature and Learning."

The subjects are dealt with in the following order:—

I. The Courses of Study and the Examinations.
II. The Professoriate.
III. The Scientific Institutions within the Universities.
IV. The Colleges.
V. The Relation of the Universities to Technical Education, and to Education for Scientific Professions.
VI. The Duty of the Universities and the Colleges with regard to the Advancement of Science.

Under the first head an examination on leaving school equivalent to the German *abiturienten examen*, to be controlled by the Universities, is proposed, "so that the scientific student who had shown the requisite literary proficiency in the 'Leaving Examination' would find himself absolutely free, except so far as the examination in Divinity is concerned, from the first moment of his entrance to the University, to devote his whole time and energy to his scientific studies." The Commissioners adding their opinion that "any system which does not concede, from the first, this freedom to those students of Science who have given proofs of sufficient literary acquirements, involves an interference with their course of study which in many cases is prejudicial."

The opinion is also expressed that, in addition to the College Scholarships, University Scholarships in Natural Science should be founded at both Universities; scholarships comparable to those which already exist for various branches of classical learning, and, at Oxford, of Mathematical Science.

Under the heading of the Professoriate, lists of the Professorial and Collegiate teachers at Oxford and Cambridge are given and compared with similar lists for Berlin, with the remark that "it is impossible not to be impressed with the evidence which the list affords of the abundance and variety of the scientific teaching given in the University of Berlin by professors of great eminence. We would particularly call attention to the fact that the list includes not merely general courses adapted to the requirements of those students who are interested in Science only as a part of a liberal education, but also special courses on subjects taken from some of the newest and most interesting fields of scientific inquiry; so that instruction of the kind most likely to develop a scientific spirit in the mind of the learner, and given by the most competent teachers, is put within the reach of every student."

With regard to the proposed additions to the Scientific Professoriate, without attempting to decide what should be the ultimate organisation of the Scientific Faculty in Oxford, the Commissioners are of opinion that arrangements should be made at the earliest possible opportunity for the establishment of two Professorships in Physics, and two in Chemistry, in addition to those already existing; for the redistribution of the biological subjects (exclusive of those assigned to the Faculty of Medicine) in such a manner as to secure their being represented by five independent professors; and for the addition of two chairs, one in Pure Mathematics and one in Mathematical Physics. Lastly, they are disposed to recommend the establishment of a Chair of Applied Mechanics and Engineering.

Somewhat similar additions are proposed in regard to Cambridge.

So far we have dealt with professors of the first order, so to speak, but the appointment of adjoint professors, demonstrators, and assistants is also proposed in the following words:—

"Although the witnesses have been unanimous as to the necessity of strengthening the professorial staff, they do not entirely agree as to the way in which this should be done. Mr. Pattison would increase the number of independent Chairs of Science to twenty or even to thirty. On the other hand, there appears to be a feeling that the principal subjects should not be too much divided although it is admitted that at present they are too much grouped together.

"It must not be forgotten that an increase in the number of independent Chairs would render it necessary for the Universities to provide increased accommodation in laboratories, and additional apparatus. With the view of utilising to the utmost the existing appliances of this sort, some of the witnesses have suggested that the increase of the professoriate should, as far as possible, be provided for by an abundant supply of skilled assistants, of demonstrators, and of assistant professors, rather than by increased numbers of independent lecturers.

"The necessity for skilled assistants and for demonstrators of course made itself felt at a very early period, and though a certain number of such assistants and demonstrators have been supplied, yet the need for an increase in the number of these subordinate offices has already become apparent. It may be mentioned, for example, that at neither University is any assistance of this kind at present afforded to the Chair of Geology, or to that of Botany.

"A Natural Science Professor should have, in the first place, sufficient skilled assistance to relieve him from all mere drudgery in the preparation of his lectures. In the second place, he should have such further assistance as may be necessary to enable him to carry on original researches. And, thirdly, although no professor would wish to hand over the superintendence of the practical teaching in his laboratories entirely to others, he should be enabled to discharge this duty of superintendence without an undue sacrifice of time. The work should be done under the professor's eye, but its details should be entrusted to competent demonstrators, appointed by and responsible to him.

"So far there is a general agreement; but the question whether assistant professors should be appointed at all, and if so, how far the dependence of the assistant professor upon the principal professor of the subject should be carried, has given rise to some divergence of opinion. We have already stated that we regard as indispensable the establishment of a certain number of new Chairs, to be independent of, and to take equal rank with, the existing Chairs. If the Universities are to become great schools of Science, it is of the first importance to secure for them the permanent services of a very considerable number of scientific men of established reputation; and we cannot perceive how this object is to be attained otherwise than by offering to such men, without any reservation whatever, the same academical *status* which has hitherto been enjoyed by the University Professors. We consider, therefore, that in any extension of the Professoriate, this is, without doubt, the first point to be attended to. But we are also disposed to attach great weight to the suggestion that, in addition to the Professorships representing the great divisions of Natural Science, University Teachers, who might be termed Adjoint Professors or Readers, should be appointed to undertake the instruction in special branches. It would be undesirable to place an Adjoint-Professor in a position of complete subordination to the Principal Professor of the subject; and it would probably be very difficult to arrange any plan of partial subordination which could work satisfactorily. We are, therefore, of opinion that the Adjoint Professors should not be regarded as assistants to the Professors, but should be responsible for the due discharge of the duties assigned to them to a Board or Council, appointed by the University, and not to any individual Professor.

"It is important that the Universities should be able to secure the services of men who have shown their ability to promote Science, and to become successful teachers of it, by offering them places, such as the Adjoint Professorships, which would give them an opportunity of distinguishing themselves; and, with this view, it is very desirable that as much independence as possible should be allowed to the Adjoint Professors, in order to make the appointments attractive to the best men. On the other hand, as it is obvious that the perfection of the means and system of instruction in the Universities is of primary importance, an organisation of, and control over, the courses of instruction would be necessary, as otherwise there might be an excess of lectures in some subjects, and a deficiency in others. We are of opinion that these difficulties might be overcome, and a sufficient amount of liberty combined with systematic organisation, if, as we shall presently recommend, a Central Board, or Council, should be formed, representing the Scientific Faculty, and having definite functions with regard to the scientific teaching within the Universities.

"We may observe that the financial argument in favour of extending the Professoriate (at least in the first instance) by the institution of offices not intended to take equal rank with the existing Chairs, rather than by increasing the number of the Principal Professorships, will probably lose some of its force when a careful estimate is made of the difference which the adoption of the one plan or the other would make in the charge to be laid upon the funds of the Universities. It is quite true that the emoluments of an Adjoint Professor need not be so great as those of one of the Principal Professors; and that to this extent there would be a saving. But whether an additional professor of any subject be termed an Adjoint Professor, or whether his Chair be regarded as co-ordinate with the existing Chairs, the difficulty would always remain that if he is to be of any use at all he must be furnished with the necessary apparatus; he must have a room to lecture in, a room or rooms to work in, and the classification of the students will also probably require additional space. Laboratories of chemistry, physics, and physiology have been already provided; it would, therefore, not be necessary to create a large establishment for any new professor. But it is certain that the only way in which the Universities can increase the usefulness, at the same time that they increase the number of the professors, is by being ready to make, from time to time, such moderate additions as may be necessary to the buildings which they appropriate to Science."

Under the heading "Duties of Professors," we have the following:—

"It has been suggested that, in the case of certain professorships at both Universities, the functions of Original Research might be separated from direct instruction. To a professor the duty of teaching is a matter of daily routine; whereas, original research is a duty which belongs to no day in particular, and which is, therefore, very likely to be neglected in comparison with the other. Nevertheless, we cannot see any just and sufficient reason, in the case of the professorships, for a total separation of the two functions; and even Sir Benjamin Brodie, who has supported the view that some distinction should be made between offices appropriated to teaching and those appropriated to original research, would not have the separation absolute, and would consider it of importance that even a professor whose chair was founded chiefly with the latter view, should be called upon to produce, from time to time, in the form of lectures, the results of investigations in new departments of Science. Lecturing is not the only mode in which scientific instruction may be imparted. A professor who should undertake the direction of a laboratory in which advanced students were to be trained in the methods of scientific research, would be very far from holding a sinecure office, and would be rendering the highest, as well as the most direct, service to scientific education.

"We have no doubt that for a professor the duty of teaching is indispensable, but we agree with the witnesses whom we have examined that original research is a no less important part of his functions. The object of an university is to promote and to maintain learning and science, and scientific teaching of the highest kind can only be successfully carried on by persons who are themselves engaged in original research. If once a teacher ceases to be a learner it is difficult for him to maintain any freshness of interest in the subject which he has to teach; and nothing is so likely to awaken the love of scientific inquiry in the mind of the student as the example of a teacher who shows his value for knowledge by making the advancement of it the principal business of his life.

"It has been, to a certain extent, a complaint against the School of Natural Science in Oxford that hitherto it has produced but very few original workers. The complaint (if well founded) may, perhaps, be accounted for by the circumstance that the school has not been long in existence; but there can be no question that it is of the utmost importance to impress upon teachers and learners alike that one, and perhaps the chief criterion of success in the teaching of Science is its leading to new discoveries. To promote this end the Universities probably can do nothing more useful than to increase the number of persons employed, under whatever name, in the teaching of Science, taking care at the same time that, while such duties are assigned to them as may prevent their offices from being sinecures, they shall be left with time and energy enough to carry on original work. We consider this to be a point of great importance, and we should regret to see any scientific office whatever established in either of the Universities without its being understood that it is expected from the holder that he shall do what is within his power, not only for the diffusion, but also for the increase of scientific knowledge.

"It has been stated in some parts of the evidence which we have taken, that the duties of lecturing and teaching which are required from the professors are such as seriously to interfere with their leisure for original investigation, and a wish has therefore been expressed that the provisions of the Professorial Statutes as to the number of lectures to be given should be relaxed.[*] We cannot concur with this suggestion. In estimating the amount of teaching and lecturing which can properly be required from a professor, we do not forget that he is expected to keep himself well acquainted with all the latest advances in some very wide department of knowledge, a task which, at the present rate of scientific productiveness, is no light one. But, on the other hand, we cannot leave out of sight that the University duties of a professor last for only six months, and that he has thus the invaluable privilege of being master of his own time for fully one half of the year. It is, therefore, only reasonable that during the University Terms he should devote a fair proportion of his time to the work of teaching. And we feel it to be our duty to say that, in recommending, as we have done, the foundation of a considerable number of new Scientific Professorships, our intention is that duties of a very substantial kind should be attached to each of these offices, with a view to the establishment of an efficient and complete course of instruction."

From the limited scope of the functions of the various existing administrative bodies, as well as from the constitution of one of them, the Commissioners consider that they cannot be regarded as representing, in any adequate manner, the Scientific Faculty of the University. They then add, "We are of opinion that the best mode of providing for this important object would be to replace them by a Single Administrative Body, representing every department of Science, and having wider but still definite powers entrusted to it. Without attaching any importance to the name, we shall, for the purposes of the present Report, designate this proposed administrative body as 'the University Council of Science.'

"The duties of the Council would, we conceive, be twofold—educational and financial."

(*To be continued.*)

HARMONIC ECHOES

ACCORDING to Dr. Brewer[*] "The harmonic echo repeats in a different tone or key the direct sound. The harmonic is generally either the third, fifth, or tenth of the tonic. . . . On the river Nahe, near Bergen, and not far from Coblentz, is an echo thus described by Barthius:—It makes seventeen repetitions at unequal intervals. Sometimes the echo seems to approach the listener, sometimes to be retreating from him; sometimes it is very distinct, at others extremely feeble; at one time it is heard at the right, and the next at the left; now in unison with the direct sound, and presently a third, fifth, or tenth of the fundamental. Occasionally it seems to combine two or more voices in harmony, but more frequently it resembles the voice of a single mimic.

"At Paisley, in Scotland, there is a somewhat similar echo in the burying-place of Lord Paisley, Marquis of Abercorn. Musical notes rise softly, swell till the several echoes have reverberated the sound either in unison or harmony, and then die away in gentle cadence.

"At the Lake of Killarney, in Ireland, is a very celebrated harmonic echo, which renders an excellent *second* to any simple air played on a bugle.[†]

"There was formerly, according to the authority of Dr. Birch, an harmonic echo no less remarkable, seventeen miles above Glasgow, near a mansion called Rosneath. If a trumpeter played eight or ten notes, the echo would repeat them correctly a third lower. After a short silence another repetition was heard, still lower than the former; and after a similar pause the same notes were repeated a third time, in a lower key and feebler tone, but nevertheless, with the same undeviating fidelity. This echo no longer exists."

It is difficult to believe that these descriptions are accurate, but that they have a basis of truth there can be little doubt. My attention was first drawn to the subject by an echo at Bedgebury Park, the country residence of Mr. Beresford Hope. The sound of a woman's voice was returned from a plantation of firs, situated across a valley, with the pitch *raised an octave*. The phenomenon was unmistakeable, although the original sound required to be loud and rather high. With a man's voice we did not succeed in obtaining the effect.

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[*] "Brewer on Sound and its Phenomena." (1864.) P 305.

[†] This must be a near connection of the equally celebrated Irish echo, which in reply to "How do you do?" answers, "Very well, thank you."—R. Or of that celebrated echo at Shoreditch Station, illustrated by poor Leech in *Punch*, where, to the old gentleman's call of "Porter," is replied "Don't you wish you may get him"—ED.

At the time I had no idea that such an alteration of pitch had ever been observed, or was possible; but it soon occurred to me that the explanation was similar to that which I had given of the blue of the sky a year or two previously (Philos. Mag., Feb. 1871). Strange to say, at the very time of the observation I had in my portfolio a mathematical investigation* of the problem of the disturbance of the waves of sound by obstacles which are small in all their dimensions relatively to the length of the sound waves. In such a case (precisely as in the parallel problem for sight) it appears that the reflecting, or rather diverting, power of the obstacle varies inversely as the fourth power of the wave-length. When a composite note, such as that proceeding from the human throat, impinges on the obstacle, its components are diverted in very different proportions. A group of small obstacles will return the first harmonic, or octave, sixteen times more powerfully than the fundamental. After this, it is not hard to understand how a wood, which may be considered to be made up of a great number of obstacles, many of which, in two or three of their dimensions, are small in comparison with the wave-length, returns a sound which appears to be raised an octave.

The increased reflection is, of course, at the expense of the direct sound. If we conceive a group of small obstacles to act on a train of plane waves of sound, the effect will be a *diffused* echo, which may be heard on all sides, appearing to proceed from the group, and the direct waves which maintain their direction. If the original sound be composite, the diffused echo contains the higher elements in excessive proportion, and for the same reason the direct wave, being shorn of these higher elements, will appear duller than the original sound. It is well known that pure tones are liable to be estimated an octave too low, and thus it may be possible that a note in losing its harmonies may appear to fall an octave.

What is here called the direct sound may itself be converted into an echo by *regular* reflection. For example, if a plane wall were covered with small projections, there would be a diffused echo, due to the projections in which the higher elements preponderated, and an ordinary echo, obeying the law of reflection, in which the wave elements would preponderate.

I shall be much obliged if any one under whose observations echoes of this description may happen to fall, would communicate particulars of them to NATURE.

RAYLEIGH

LEITH-ADAMS' "FIELD AND FOREST RAMBLES"

Field and Forest Rambles. By A. Leith-Adams, F.R.S. (Henry S. King & Co.)

ONCE, on our expressing surprise to a friend at the fact of his having forsaken his usual line of study for another of a very different character, he remarked, "Well, you see it does not matter much what I take up, for whatever it may be, I am sure to make some discovery of value." The reply was sufficient to enable anyone to form an idea of the results that might be expected. He was an assiduous and earnest worker, but there was a certain deficiency in the quality of all he produced.

Mr. Leith-Adams is an assiduous and earnest worker; his opportunities in connection with his military avocations, have been considerable, and he has used them well. He has already given us the results of his experience in India and elsewhere in his "Wanderings of a Naturalist in India," as well as in the "Natural History and Archæology of the Nile Valley and Maltese Islands," and in the work before us he takes us to New Brunswick, vividly portraying the beauties of its short summers and the discomforts of its dreary winters. An intense love for natural history has led him to make careful and prolonged observations as to the habits of most of the animals inhabiting the province of which he treats, together with the dates and direction of migration of the numerous migratory birds which are there met with. He has also paid considerable attention to the fish, and the geology of the district.

Our author, in endeavouring to obtain an accurate account of the past history of the native Indians of New Brunswick, found the task of more than ordinary difficulty, "inasmuch as, even apart from their persistent indifference to treat on any subject connected with their past history or present condition, there would seem to be an absolute incapacity to comprehend the meaning of such inquisitiveness on the part of the interrogator." Drink is the ruination of the remnant of this doomed race, a race so little advanced in the scale of humanity, that when it has disappeared, there will not be left a trace even of written or monumental record; "indeed, were it not for implements of the chase picked up occasionally, we should have few other data to establish the existence of the human inhabitants of the region, previous to the arrival of the first European travellers." The European colonist, as long as he is the possessor of the *mens sanus in corpore sano*, however, stands a better chance of surviving; nevertheless leprosy produces painful ravages among the original French settlers, on the north-east frontier of the province.

No explanation is attempted of the fact quoted from Dr. Gilpin, that many of the wild animals, as the bear, racoon, and beaver, which were driven from their haunts on the clearing of the forests, are again returning to the same districts, "to cultivated fields instead of primitive forests, to corn and maize, instead of wild fruits and berries." We cannot help thinking that this does not say much for the present assiduity of the farmers.

Albinism and Melanism, the tendency for certain individuals of a species to be white or black, is one of Mr. Adams' favourite subjects, and he gives it as his opinion that the reason why they in the wild state do not continue to propagate their peculiarity is because "the very decided difference as regards outward appearance would be sufficient to forbid intercourse between them and the typical individuals."

There is a want of point in many of the author's attempts at explanation of the various phenomena which excite his curiosity. In considering the fact that the Cat-bird (*Mimus carolinensis*) has a strongly marked antipathy against the animal whose name it bears, he says, "I have often wondered if this inherited distrust of the cat could be explained in any way with reference to the imitative peculiarities of the bird. In other words, is it possible that some ancestor began to mew like a cat whenever it saw the wild cat in his haunts, and that in process of time it came to be an established habit?" Again, the answer given to the question, why such migratory birds as the ruby-throated Hummer (*Trochilus colubris*) are not content with the eternal summer of the south? is equally inconclusive: "All that we can say is that some inherited instinct is at work, perhaps to them as precious as is the longing for the holidays to the schoolboy, full of pleasant reminiscences, which of course would grow by experience." And we do not feel any nearer the truth as to the reason why the peculiarity of the beak of the Crossbill is so well marked, when we know that in the bird's attempts to extract the seeds from the red spruce and other cones, "the bill, which is not so strong and conical as that of the pine bullfinch, became curved, until at length the condition became hereditary and transmissible."

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* Since communicated in an amplified form to the Mathematical Society.

An interesting remark is made, which illustrates how very susceptible the animal body is to the influence of slowly-acting external circumstances. For it is the popular belief in New Brunswick that the severity of an ensuing winter may be predicted by the amount of fat present on the intestines and omenta of animals, whether wild or domesticated; and as the coldness of the winter must depend on the previous climatic condition, that may reasonably be supposed to affect the constitution in a manner favourable to the individual.

In conclusion, we think that both sportsmen and naturalists will find this work replete with anecdote and carefully recorded observation, which will entertain them; at the same time they will not put down the book without feeling that they have acquired much new information on the physical geography and natural history of New Brunswick.

HOEFER'S "HISTORY OF PHYSICS AND CHEMISTRY"

Histoire de la Physique et de la Chimie. Par Ferdinand Hoefer. (Paris: Hachette, 1872.)

MORE than twenty years ago M. Hoefer published a History of Chemistry, the first which had appeared since the publication of Dr. Thomas Thomson's History. M. Hoefer has since been known to us as the author of the biographies of various scientific men in the *Nouvelle Biographie Générale*, and of a small work entitled *La chimie enseignée par la biographie de ses Fondateurs.* The volume before us is one of a series which treats of universal history, and is published under the direction of M. V. Duruy. The works which it comprises are intended to be used in colleges and schools, and M. Hoefer's volume has no doubt been included, because the promoters of the series have wisely considered that the history of matter, and of motion, are as worthy the attention of the rising generation as the history of languages numbers, peoples, faiths.

Out of the 553 pages which the work contains, no less than 314 are devoted to the history of Physics, while the remainder contain in a condensed form the substance of M. Hoefer's larger *Histoire de la Chimie.* The History of Physics is divided into two books, entitled respectively "Matter" and "Motion," the former including—1. The immediate properties of matter (weight, volume, density, elasticity, compressibility); 2. The terrestrial atmosphere; 3. Liquefaction and solidification of gases; 4. Hygrometry; 5. Acoustics.

The second book on Motion includes—1. Gravity; 2. Heat; 3. Light; 4 Electricity and Magnetism.

We feel bound to take exception to this arrangement, which is both immature and ill-considered. For why has M. Hoefer classed *weight* with *matter*, and *gravity* with *motion?* and why *liquefaction and solidification of gases* with *matter*, when they are operations distinctly connected with motion? But, worse than all, why has he classed *acoustics* with *matter?* Again, he has omitted all mention of certain sciences which were among the earliest—Statics, Dynamics, Hydrostatics, Hydrodynamics. These sciences, from their antiquity, lend themselves with great facility to the apt illustration of the various phases of the history of science. Archimedes has received an altogether insufficient amount of notice: we may not forget that several of our sciences actually owe their origin to him; and how M. Hoefer, with Peynard's fine edition of the works of Archimedes in his own language, can have overlooked him, we are quite at a loss to understand Then the Archimedian screw, the pumps of Ctesibius, the *Δυναμεις* of Hero of Alexandria, should all have full mention in the work. And if it be urged that space did not permit mention of these things, we would reply that they are of far more importance than Hygrometry, which finds mention in the book. Also such sections as "Pèse-liqueur d'Hypatie," "Manomètre," "Hygromètre condenseur," "Porte-voix," "Clavecin et carillon electrique," "La beatification de Bose," might all have been replaced with advantage by more important matters.

We notice with regret a tendency to attribute discoveries to men who were not first in the field. Thus, although Boyle discovered his law of the compression of gases, no less than *fourteen years* before Mariotte, it is called *Loi de Mariotte.* Again, M. Hoefer says, "Gassendi paraît s'être le premier occupé de la question de la vitesse du son, sans préciser les résultats auxquels il était parvenu." But if M. Hoefer will read Lord Bacon's *Historia Soni et Auditus*, he will find a good deal of valuable and suggestive matter, among other things, a suggestion for determining the velocity of sound.

Let us turn to the comprehensive little treatise on the history of chemistry, beginning with Hermes Trismegistus, nay, with Moses, and ending with Wurtz, Williamson, Frankland, and Kōlbe. This part of the work, as derived from M. Hoefer's larger treatise, is altogether more matured than the preceding; yet it is not without evidence of hasty selection and ill-considered statements. We cannot agrée with M. Hoefer when he tells us that the word chemistry was used in the fourth century, and that we are to trace it to *χυλὸν* and *χέω.* Neither, for various reasons, which we have stated elsewhere, can we accept the Greek MSS. attributed to Zosimus, Pelagius, Olympiodorus, Democritus, Mary the Jewess, and Synesius, as exact evidences of date or knowledge. In regard to more modern matters we regret to find no account of Robert Hooke's important theory of combustion. We are glad to observe that M. Hoefer does not echo the Wurtzian aphorism: "La chimie est une science Française, elle fut instituée par Lavoisier d'immortelle memoire." More liberally our author says, "Tout en suivant chacun une route différente, trois chimistes ont fondé, vers la fin du dix-huitième siècle, la chimie moderne: Priestley, Scheele, et Lavoisier, un Anglais, un Suédois, et un Français."

We should be glad to see in our own country the history of matter and of motion studied side by side with the history of languages and of numbers. Prof. Kopp lectures on the History of Chemistry in the University of Heidelberg, and no doubt his example is followed in other of the German universities. M. Hoefer's work is in many ways suitable for use as a text-book; it is cheap, it is anything but dull, and whatever the errors of arrangement may be, it contains a great deal of information.

G. F. RODWELL

OUR BOOK SHELF

An Essay on the Physiology of the Eye. By S. H. Salom. (Published by the Author.)

THAT the study of formal logic is not in itself conducive to sound reasoning will be acknowledged by many, but it is seldom that the truth of the statement is so fully illustrated as in the short work before us. The author has studied the writings of Hamilton, Mill, Bain, and others, and with a creditable enthusiasm endeavours to employ the new powers he thinks he has thereby acquired, in developing a hypothesis of his own to account for the phenomenon of vision more satisfactorily than those already accepted. An outline of the arrangement, which is partly disguised at first sight by the many technicalities and circumlocutions employed, will be almost, if not quite, sufficient for most of our readers. Commencing with a notion broached by Erasmus Darwin, that visual perception ensues from retinal motion derived through the motile force of light, the author hopes, "by turning the light of modern histological discovery on Darwin's theory of involuntary animal action, to succeed in convincing associational psychologists that this theory must henceforth be included in the creed of *à posteriori* thinkers." With this as a basis, the doctrine promulgated may be thus summarised. The eyeball being in a constant state of reflex action on account of the light acting dynamically on the retina, the motion thus produced exerts in the muscles surrounding the eye feelings of muscularity similar to those excited when we voluntarily determine ocular direction, and consequently without any voluntary effort, we are constantly aware of visual space properties. To prove this novel hypothesis the structure of the retina has to be fully entered into, and in a most ingenious manner solid fact is distorted to satisfy unsubstantial theory. Taking a single example of the reasoning employed, we find that it is necessary for the theory that the fovea centralis of the retina should be elastic; that it is so is evident from the following considerations:—"In the copious index of that exhaustive anatomical work, 'Quain's Anatomy,' under the heading 'yellow,' we find, in addition to 'yellow spot,' four substances *only*, namely—

Yellow cartilage,
„ fibres of areolar tissue,
„ ligaments of the vertebræ,
„ tissue.

And on referring to the pages of the book in which these subjects are treated, we discover that *they have the common property of being elastic*." From this on one of Newton's rules for philosophising "we are bound to frame the following physiological induction,—*all yellow anatomical substance is elastic*." We can hardly think that the author is not attempting to fool us.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Atoms and Ether

ATTEMPTS to dispense, in physics, with the ideas of direct attraction and repulsion, however interesting, lead generally to a *petitio principii*, and I fear Prof. Challis's view, to which attention is called in NATURE of August 7, cannot be received as an exception.

For an ether of which the density can be varied is a substance that can be compressed and expanded, and what idea is in our minds when we speak of compression and expansion in a really continuous substance? Continuity implies space, and space that is full. Can space be more than full? When we say that a fluid is compressible and elastic, do we mean anything else than that it is made of parts which can be pushed closer together, and which, being so pushed, will push each other back? But this is repulsion and action at a distance. We do not alter the fact by calling the substance ether, and relieving it from the influence of gravitation.

Is a continuous substance, which is capable of compression, conceivable? I think not; or if it is, the conception is at once more difficult and more opposed to sensible experience than that of attraction and repulsion.

The substance of a bar of iron is not continuous. If I draw one end of it towards me, why does the other end follow? What can be the relation between the movement of my end of the bar and the ethereal vibrations which must propel the other end and all intermediate parts in the same direction?

Liverpool, Aug. 9 ALBERT J. MOTT

Instinct

Sense of Direction

THE perusal of the correspondence published in the February and March numbers of NATURE now to hand, and also your article on "Perception and Instinct in the Lower Animals," in the number of March 20, has induced a belief in my mind, that I may perhaps be able to contribute some evidence bearing upon the question at issue; and also that it may have some value from having been obtained from a field of observation not generally accessible, and from the fact that cattle and horses in Australia are subject to very different conditions to those obtaining in England.

I may commence by stating that the question, whether animals have or have not a peculiar power of finding their way from place to place, suggested itself to my mind very shortly after I first went into the Australian bush, now more than twenty years ago. It was not long before I satisfied myself that in many horses this faculty was strongly developed, but yet unequally in different individuals. I afterwards ascertained that it also existed in cattle.

Not only did I find that horses had extensive memories for places, being enabled to recollect a track they had followed some time previously, but also to remember the way from one place to another where no track existed. I found that not only had horses this exact memory, but that they possessed another gift which at first appeared to me inexplicable. This was, that when ridden through the bush, many horses would never, for a moment, as it were, lose the recollection of home, but "bear away" in its direction. I remarked this not only in a district with which the horse might be acquainted from grazing in it, but also when travelling and absent for the day from my camp, and from the other horse or horses, the "mates" of the one I rode.

Further than this, I also found that as regarded myself, I never lost the distinct perception of the direction in which my home, or camp, or starting-point for the day was situated, and in endeavouring to trace out and analyse this feeling, I at last came to the clear perception that it depended upon an unconscious action of the memory thus recording the alterations of the courses I had followed, and which by an effort of the memory I could recall. On this point I feel quite clear, for from the practice of paying especial attention during constant explorations to the course travelled, both for the purpose of keeping a correct dead reckoning, as also for the delineation of the features of the country passed over, I have found the faculty intensified, and the process more evident to myself. I may say that during the course of those twenty years' experience, I have never found the faculty at fault.

I believe in this lies the explanation of the power possessed by cattle and horses of finding their way from one place to the other irrespective of the road they may have gone.

I now propose to record some instances showing how cattle and horses in this district have endeavoured to reach the places where they were reared, and the truth of which I do not in the least question. To show how frequently such cases are met with here, where horses and cattle are bred in a half wild state at large in the bush, I may note that on determining to make this communication, I spoke to the first persons I met with who were likely from their pursuits to have noticed instances of the nature I required; of these persons, four at once gave me the particulars I am about to relate. But before doing so I must further remark, as bearing perhaps not remotely upon the question, that I have not met with aboriginal natives, either as savages or as "tame blacks," who possessed any power of finding their way from place to place differing in its nature, though perhaps in its degree, from that to be found in every good "bushman" among the whites. Their knowledge of country is entirely local—special as regards the district belonging to their tribe or family—general as regards the country of the neighbouring tribes. They know it thoroughly because they have been born in it and have roamed over it ever since. Out of their own locality I have found them to be inferior to a good white "bushman," in so far that they are unable to reason out any problem relating to the features of the country, and my experience has shown that out of their local knowledge I could never rely upon one of them in preference to my own judgment. I have remarked also that very few could, even in their own districts, travel straight from one place to another, say at twenty miles' distance. I now refer especially to the aborigines of that part of the interior of the continent lying on each side, north and south, of Sturt's Desert and including Cooper's Creek. As a rule they would "give and take" some 30° on each side of the course, correcting the direction from time to time as they recognised the "lay of the country" from rising ground.

In order that the instances I shall now quote may be more clear, it will be necessary to say in the first place that all the localities mentioned below will be found named in the maps published by the Surveyor-General of Victoria, and no doubt also in others. The only exception is Deadcock Creek, which is however shown as a small stream falling into the Mitchell River, on the west side below Cobbannah Creek All cattle and horses brought down from the Maneroo table-land in New South Wales to the Gippsland market, travel by one road *viâ* the Black Mountain, Buchan, Bruthen, Bairnsdale, and Stratford; the distance from the centre of Maneroo district, say where the 149th meridian crosses the Snowy River, to Stratford, is about 180 travelled miles, and the number of cattle brought down annually may be about 12,000; of these a certain percentage escape and make their way back to the place where they were bred unless recovered on the way or hindered by natural obstacles. There is no other way from Maneroo into Gippsland excepting the one mentioned, and the country northward between that road and the Great Dividing Range is occupied by high and rugged mountains, dense forests, and thick scrubs. The road from Maneroo crosses the rivers flowing from the Great Dividing Range.

1. About four months ago a mob of cattle was brought down from O'Rourke's Station, the Black Mountain, Snowy River, and sold at Stratford. After being two months on the Bushy Park run near Stratford, fourteen bullocks escaped from the paddocks, and on search being made were recovered at the junction of Deadcock Creek with the Mitchell River. The line they had taken if carried out would go near the Black Mountain.

2. A horse bred by Mr. Sheen of Omeo was taken down *via* Bruthen, Bairnsdale, and Stratford, and sold; was broken into harness, and worked by Mr. McFarlane, a contractor; was lost near Stratford, and on search being made was found at the junction of the Wentworth and Mitchell rivers. The line taken in this case is direct for Omeo.

3. Mr. Dougald McMillan of Stratford some little time back bought a mare from a Maneroo "mob." About a month ago she was lost from near Stratford in hobbles and was seen a day or two afterwards crossing "Iguana Creek still in hobbles and as fast as she could go." The people from the Glenalladale Station (Iguana Creek) being then engaged gathering some wild horses at Deadcock Creek, found her with them. This line taken was the usual one, and if carried out would cross the centre of Maneroo.

These three cases were related to me by the stock-keeper at Glenalladale Station.

4. A year or two back Mr. Kreymborg of Bairnsdale purchased a mob of horses from O'Rourke Station, Black Mountain, and sold one, a black mare, broken to lead to a person named Gee, living at Cobbannah Creek. The mare remained with Gee's horses for some time, but was then missed from Lower Cobbannah Creek and next heard of at Tabberabberah, and was recovered on Pettersen's Station, at the foot of Mount Baldhead.

This line bears a little away from the Black Mountain, but the nature of the country is such that the Mount Baldhead and Notch Hill tier of mountains form the end of a *cul de sac*, of which the open country at the junction of the Wentworth and Mitchell rivers is the mouth. This tract of forest country fenced in by mountains a few years ago swarmed with stray cattle and their progeny; three hundred bulls were shot by the then proprietors in, I believe, about two years.

5. Mr. Freitag, who follows the occupation of packing up goods to the Crooked River gold-workings, tells me that he is in the habit of buying Maneroo horses at Stratford and breaking them in for use in his pack-train. He finds that for the first few trips they require watching carefully when camped at Iguana Creek, where the road to Crooked River turns northward, as they are very apt to make away at that place. When recovered they are usually found either at Deadcock Creek or up the river towards Tabberabberah, thus conforming strictly to the direction taken by cattle and horses in other instances.

6. Thomas Dowling, employed in the stations of Messrs. Degraves, at Omeo, bought a mare from Mr. McKeachie, of Delegete in Maneroo. The mare was kept in the Hinnomungie paddock (Omeo) for two or three years. Being then taken to Bindi, about twelve miles distant, she escaped, and after being seen at Nannyong, was recovered at Gelantipy, on the Snowy River. Nannyong is a small open piece of country on the summit of the mountain east of Bindi, and the country crossed over, fifty miles, is very difficult, the mountains being some 4,000 to 5,000 ft. in altitude, and almost unknown even now except to stock-men. I came through, last summer, nearly in the line the mare must have taken. It is almost direct for Delegete.

7. A bullock-driver named Richardson purchased a working bullock which had been sent down from Maneroo by the usual road for sale. He sold the bullock at Omeo—going up there with loading—to Mr. Lewis, the manager of Messrs. Degraves' stations. The bullock was kept in the Hinnomungie paddock, but got out two or three times, and in each case made away across country direct for Maneroo, being recovered by the Messrs. Pendergast, of Mt. Leinster, and sent back to Mr. Lewis.

These cases I have obtained from Mr. Lewis, and they are remarkable as showing the length of time during which cattle and horses retain the recollection of their native places, and also as showing, in even a more marked manner than those quoted first, that they return homewards without any regard to the track by which they have reached their place of departure. The cases from Stratford, on the other hand, illustrate the distances from which cattle will start for home.

8. Mr. Mackintosh, of Dargo, informs me that about two years ago, when gathering wild cattle on the Avon River, he got away from his men down that river for many miles before he ascertained that he was astray. Finding, then, that his horse persisted in going in a certain direction, he gave him his head, and the horse went in a straight line to the place where the camp was fixed, a distance of some ten miles through a scrubby country, and without a track.

I could continue quoting examples still further, but I fear that

I have already trespassed too much on the columns of NATURE, and I shall conclude by saying that these instances are not thought extraordinary here, and that the belief that cattle and horses can find their way "straight" is firmly held by all bushmen. I have heard similar instances at Lake Torrens, the Darling River, and Maneroo.

I am aware that they do not affect the question as to how a cat finds her way home when conveyed shut up in a bag, but I conceive that they bear out the view suggested by Mr. Darwin, and with which my own experience coincides.

A. W. HOWITT

Bairnsdale, Gippsland, Victoria, May 21

Ingenuity in a Pigeon

THE following facts (having been witnessed by myself) may, perhaps, be considered worthy of insertion in your journal, as bearing on the subject of "Perception and Instinct in the Lower Animals," which has lately been brought into such prominent notice.

On the Richmond road (Surrey), at about a mile from the town, stands an old roadside inn, yclept "The Black Horse," owned by one R. Ketley. Attached to the house are a number of domestic pigeons of various breeds, chiefly "Pouters."

Having occasion to wait for my pony to be harnessed at this inn a few years since, my attention was directed by a gentleman (a resident of the neighbourhood) with whom I was acquainted, to the strange conduct of one of these birds.

A number of them were feeding on a few oats that had been accidentally let fall while fixing the nose-bag on a horse standing at bait. Having finished all the grain at hand, a large "Pouter" rose, and flapping its wings furiously, flew directly at the horse's eyes, causing that animal to toss his head, and in doing so, of course shake out more corn. I saw this several times repeated; in fact, whenever the supply on hand had been exhausted.

I leave it to your readers to consider the train of thought that must have passed through the pigeon's brain before it adopted the clever method above narrated, of stealing the horse's provender.

Was not this, indeed, something more than mere instinct?

RICHARD H. NAPIER

Upton Cottage, Bursledon, Southampton, Aug. 13

The Origin of Nerve Force

I NOTICE in NATURE for July 21 a paper by A. H. Garrod, suggesting that nervous force has its origin in thermo-electric currents due to the difference of temperature between the surface and interior of the body. Without presuming to any opinion from the physiological point of view, I venture to mention one or two obvious difficulties.

Although, as the writer observes,[*] "in cold weather the impulse to act is much more powerfully felt than in summer, when the air is hot, and therefore the temperature of the surface is higher," yet even 98° F. (the internal temperature of a healthy body) is not uncommon for the air in tropical climates, where the natives can undergo great exertions. But, according to the thermo-electric hypothesis, the nerve force must in this case be *nil*. Again, temperatures of 140° to 160° F. are easily sustained for a considerable time in the Turkish Bath. Under these conditions the direction of the current should be reversed; and, even supposing that positive and negative currents both acted in the same sense on the muscles and nervous ganglia, it would seem that there must be an instant of transition when the two should be balanced, and nervous force at zero, and the powers both of sensation and motion lost with it.

The thermo-electric theory is not required to explain the cases to which Mr. Garrod alludes. We have only to consider that the body must be kept at a constant temperature of about 98° F., while heat is being continually evolved internally by nervous and muscular action, to see that the surface of the body must be cooler than the interior in order to get rid of the superfluous heat without consumption of work in increased perspiration and evaporation. At high external temperatures there will naturally be disinclination to muscular exertion; not only because it produces heat which tends to upset the equilibrium of temperature, but because the force that would have been expended in it is consumed in increased action to get rid of the heat. That the exhausting effect of hot water is much greater than that of hot air is accounted for both by its greater conductivity and specific heat, and still more because it checks evaporation, which is one of the most powerful outlets for waste heat. It must be familiar to everyone that rapid exhaustion is produced by immersion not only in hot water but in that of almost any temperature. Taking 70° as an average external temperature, we shall find that immersion in water at 30° would be quite as rapidly destructive of nervous energy as in that of 110°; and that while air of the latter temperature could be sustained by the naked body for long without inconvenience, that of 30° would be rapidly fatal unless the temperature was kept up by violent exercise.

[*] Supposing the brain to be really colder than the blood, I shall be glad if some physiologist will inform me if this is not due to the consumption of heat in building up the complicated and unstable matter of the brain from the comparatively stable and simple constituents of the blood, and in this case, if there is any difference of brain temperature between times of rest and nutrition (sleep) and those of active exertion.

Knowing as we do that chemical action is constantly going on in the body and that electrical disturbance is an almost constant result of such action, it seems hardly necessary to look further for the *source* of nerve force, though we are in almost complete ignorance of the details of its production.

HENRY R. PROCTER

The Flight of Birds

YOUR correspondent, J. Guthrie (vol. viii p. 86), has struck a note which will, I think, echo. The question he raises is one which has exercised more minds than one. It has been present to me individually almost ever since I was able to reason. The opportunities enjoyed by exiles, especially in tropical countries, for the study of the phenomenon of a body, poised in mid-air, with no apparent support, is considerable, owing to the boldness and number of kites and birds of that class. I have watched them from the point of view—figuratively speaking—of your Cape correspondent, scores of times, and sometimes under peculiar conditions: but I am unable to add anything *certain* to the bare statement that birds of prey *can* maintain a position of absolute apparent rest.

It is some fourteen or fifteen years since I first watched an eagle in a telescope, with a view to test an explanation—the same as that suggested by Mr. Guthrie—hazarded as a conceivable possibility by my father, long before. Since that day I have had innumerable opportunities for close watch—some of which I will describe—and never have I seen anything to support it.

Not to go back too far, as I must trust to memory, I was, two or three years ago, on the summit of a long-backed solitary hill, 500 or 600 ft. high, in the Coimbatore plains of Southern India. There was a light breeze blowing, and I saw an eagle stemming it, on the leeward side of the hill, which was steep. Sometimes he was within (say) fifty yards, and having a good glass at hand, I rested it on a stone heap, and watched him. It was frequently possible to see him thus, stationary in a motionless field of view, at an apparent distance of 10 or 12 ft. Not a feather quivered: the head was turned from side to side as he scrutinised the hill-side: occasionally a foot was brought up to the beak: the roll of the eye was perceptible: but otherwise he was *at rest* to all appearance. Of course the tips of the wings came in for a share of my scrutiny. They may have been quivering, but they looked as steady as those of a stuffed specimen. And here I may observe, that for this appearance to be compatible with an unperceived vibration, the position of rest must have existed alternately with successive excursions, and the time occupied by the latter must have been insignificant as compared with the duration of rest. I find it impossible to accept this explanation, even as a first step, and need not inquire how it would produce the supporting effects. The tail, I should mention, was not at rest. It was frequently feeling, as it were, the passing breeze.

It is to be understood that in the course of frequent changes of general position, I had the bird under examination from different directions—not always of course so favourably.

On another occasion I spent a fortnight on the summit of a peculiar hill in this neighbourhood, with nothing to do but recruit as fast as possible. The hill resembles a dish-cover at top, and being the resort of fugitives from the dust and drought and heat of Bangalore in April and May, who occupy every available dwelling on a very restricted space, there is plenty to attract the kites from far and wide, to say nothing of vultures. There are two or three kinds of kite, but for the present subject they are all the same—fine, powerful, bold birds, with a stretch of three or four feet of wing—who will swoop and take meat from a basket on a man's head, any day, or even from his hand. A score or two of these circling about the kitchen and outhouses, may be watched with advantage from the house-top, as is evident. The difficulty is to reproduce, in description, anything definite, from the copiousness of the evidence. I can therefore only express distinctly the conclusions I formed :—(1) that it was utterly incomprehensible ; (2) that there *must* be some unperceived source of motion ; (3) that it *might* be (and probably was), a subtle utilisation of the varying air currents met with or sought for. This conclusion lands one in a new set of perplexities, it is true ; but it is the least opposed to reason, however ill it may accord with some of the facts as interpreted by us.

Vultures are large heavy brutes, with comparatively little wing-power, and their flight is far slower and heavier. They very commonly rest on the ground, doing nothing, and if disturbed, the effort to rise is evidently a toilsome one. Nevertheless, they too possess, and largely exercise the power, of sustaining themselves in mid-air without apparent *action.* Not that they ever rest motionless, but they sweep about in endless paths with hardly ever a beat of the wing except on occasion, in this respect seeming to husband their strength much more than the kites, who are always on the move, and wheel in much sharper curves.

I was a good deal impressed, at one time, with the notion that the secret lay in slants of wind taken advantage of, but the more I see the less I like it. It is impossible to conceive upward currents as commonly strong enough to support a *dead* bird similarly extended. And though I am not prepared to assert that I have ever seen birds floating motionless where there was *no* wind, yet if we are to take the vertically resolved portion of wind, considered as essential, as the supporting agency, what becomes of the horizontal force? Given a sufficient momentum, one could conceive an economic expenditure of it, but not enough to explain the endless wheelings of vultures, much less the long continued poising without forward or backward movement of eagles.

In fine, I can only echo Mr. Guthrie's appeal for further explanation ; but I beg that we may have no nonsense about "bones filled with air." One is tempted to ask in that case if death *solidifies the bones*, to account for the undeniable weight and density of a goose.

J. HERSCHEL

Bangalore, July 6

Earthquakes in the Samoan Islands, South Pacific

ON two former occasions I have contributed to NATURE notices of the earthquakes experienced in these islands. I will now continue my list from the commencement of 1872.

On March 22, at 1.25 P.M., there was a shock from N.E., motion horizontal. Vibration continued 15 seconds. For several seconds before the motion was felt, and during the whole time of vibration, there was a noise like distant thunder.

On April 8, at 3.10 P.M., there was a slight shock, horizontal.

May 11, at 10.20 A.M., we had a double shock. This was rather severe. Motion horizontal; interval between shocks, 15 seconds ; total duration, 25 seconds.

May 28, at 10 30 P.M , a slight horizontal shock.

Sept. 9, at 10.20 P.M., double, horizontal shock from N.E. ; interval 25 seconds. This was a more severe shock than we usually feel here.

Nov. 12, at 5.10 A.M., a slight horizontal shock.

Dec. 3, at 9.20 P.M., a slight horizontal shock.

Jan. 2, 1873, at 7.40 A.M., a shock which, in these islands, is considered very severe. The motion was horizontal. The main undulation was followed by rapid oscillations for 45 seconds, followed by a sea-wave.

I regret that I cannot give full and definite information respecting this earthquake. I was away from home at the time, staying at the inland residence of the British Consul, on the island of Upolu, where I was unable to note with precision any of the accompanying phenomena. The Consul's residence is a wooden building with a ground floor only. It stands due east and west. This shook very severely with the rapid undulations of the earth-waves, apparently, longitudinally from east to west. I at once thought the centre of impulse was to the east of my position. Of this, however, I am by no means certain ; in fact, I have reason to doubt whether my observation on this point was correct. The sea-wave was almost entirely confined to the south coast of the islands of Upolu and Sarau On the island of Tutuila (forty miles to the east of Upolu) it rose equally on the south and north sides. I have at present no information from Manua (three islands about sixty miles east of Tutuila) except that both earthquake and sea-wave were felt there. None of those who saw the sea-wave noticed particularly the time which elapsed between the earthquake and the rolling inland of the sea-wave. All my informants from Sarail (the most westerly island) agree that the one followed the other almost immediately. They felt the earthquake and almost immediately afterwards saw the reef bare, much lower than it is at low tide. The tide was at about half-ebb at the time. Following closely on this efflux came the reflux in a large wave which rolled inland and flooded the sites of villages lying low at the back of deep bays. This wave rose about 6 ft. above high-water mark during spring tides. The rise and fall during spring tides in this group being about 4 ft. 6 in. The first great wave was followed by a number of smaller waves, and the oscillation continued for some time. No efflux of the sea was noticed, as far as I can learn, on Upolu or Tutuila. At the latter island the sea-wave rolled inland more than half-an-hour after the earthquake, and rose about 6 ft. above high-water mark. No damage of importance was done by the wave.

Two days after the above earthquakes, we had three others in rapid succession, and three more have followed them on different days since, viz. :—

On Jan. 4, at 10.45 A.M., we had a heavy horizontal shock, or rather a succession of shocks, two of which were severe. These continued 55 seconds, and were accompanied by great rumbling and a hissing noise.

Four minutes afterwards, viz , at 10.50 A M., we experienced another sharp shock, accompanied by similar noises. The vibrations of this shock continued 15 seconds. We had scarcely recovered our equilibrium and quieted our nerves after this second shock when, at 10.57 A M., we were startled by another, the oscillations of which lasted 20 seconds. This also was accompanied by great rumbling.

No damage was done by these earthquakes. The buildings in these islands are all low, and nearly all are built with wood, so that only a very severe earthquake could do much injury.

On Jan. 8–9, at midnight, another slight horizontal shock was felt.

On Jan. 13, at 8.45 A.M , we had another which was also slight.

On Jan. 14, at 5 24 A.M., there was another slight horizontal shock.

The Samoan Islands owe their existence to volcanoes, as they consist almost entirely of volcanic rock. There has, however, been no eruption for a very long period until in 1867, when, it will be remembered, a submarine volcano burst out between Tau and Olosenga, two of the Manua islands in the eastern end of the archipelago. This subsided after a fortnight's activity. A few months afterwards I was on board H.M.S. *Falcon* when soundings were taken over the spot where the volcano had been. We found a cone 180 feet above the bed of the surrounding ocean: the average depth of the sea around it being 120 fathoms, while the depth on the apex of the cone was only 90 fathoms. There has been no further eruption from this volcano up to the present time. Almost ever since this has been quiet, there has been great activity in the volcano of Nina Foou, in the neighbourhood of the Friendly group of islands.

Samoa, S. Pacific S. J. WHITMEE

THE ARITHMOMETER

MOST of our readers who have anything to do with calculations have heard of the above calculating machine, the invention of M. Thomas de Colmar. A few remarks, therefore, on its construction and operation may be of interest to those who have not seen this really useful calculating machine.

The instrument is of small size, the one which we are about to describe being only 22 in. long, 6¼ in. wide, and 3¼ in. deep.

We can best give an idea of the great saving of time effected by this instrument when we state that with it eight figures (tens of millions) can be multiplied by eight figures in eighteen seconds, sixteen figures be divided by eight figures in twenty seconds, and a square root of sixteen figures be extracted, with the proof, in less than two minutes.

Our illustration shows a top view of an arithmometer the lid of the box being removed. It is constructed chiefly of a brass plate, A, furnished with eight slots, as shown; directly under these slots are mounted eight drums, each having nine elongated cog teeth of successively decreasing length; over each drum, and between it and the slot, is mounted a square shaft, on which slides a pinion wheel so as to catch any number of teeth on the drum. Each of these pinion wheels is moved by a button, *a*, of which there is one in each slot, the figures at the sides of the slots showing the proper position of each button *a*, for any work to be performed by the instrument.

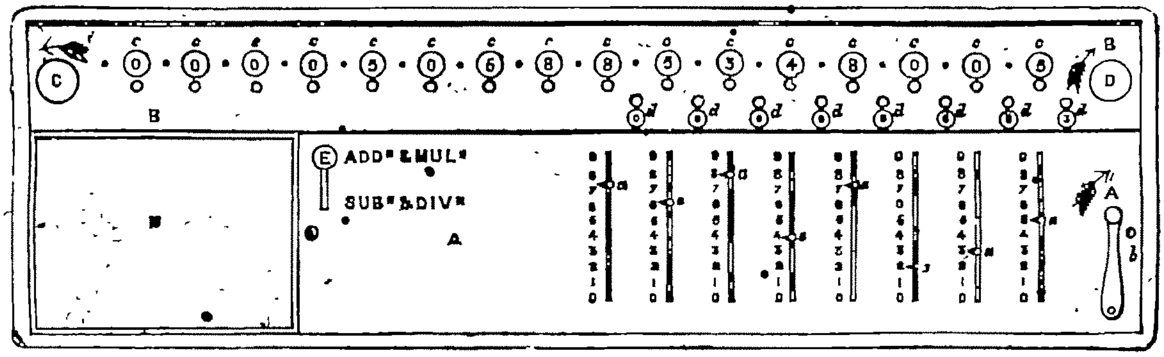
The cogged drums gear by bevil wheels with a long horizontal shaft, which is also in gear with the vertical shaft moved by the handle *b*, by which the instrument is worked. B is a moveable brass plate, which can turn and slide on a round bar-hinge at the back; in this plate there are sixteen holes, *c*, under each of which is a moveable disc numbered from 0 to 9, and arranged so that any one figure of each disc may be brought under its corresponding hole *c*. These discs have bevil wheels which gear with bevil wheels on the before-mentioned square shafts. The moveable plate B is also furnished with the holes *d*, having discs numbered from 0 to 9 underneath, and are for showing the number of turns of the handle, giving by this means the quotient in division, and showing the multiplier in multiplication. The knobs C and D are for bringing the figures under the holes *c* and *d* to zero before commencing an operation, and the knob E is for setting the instrument to work addition and multiplication, or subtraction and division. F is a small slate for memoranda.

Before further describing the working of the machine, we would remark that, if the knob E be placed at Add^{n}, each turn of the handle will carry the figures marked by the buttons *a*, under the indicator-holes *c*, or add them to the figures already under the holes *c*, while if the knob be placed at Sub^{n}, each turn of the handle will subtract from the figures under the holes *c*, the numbers marked by the buttons *a*.

Such being the general construction and principle of the machine, we will now proceed to give an example of its operation for multiplication, the operations for addition and subtraction being sufficiently explained in the preceding paragraph.

Thus, to multiply 76847235
by 6583

Mark the multiplicand on the plate A by the buttons *a*, as shown in the illustration; set the knob E at Add^{n} and Mulp^{n}, then turn the handle *b* three times for the unit figure of the multiplier, and three times the multiplicand, viz. 230541705, will appear under the holes *c* in the moveable plate B; this plate must now be raised, and moved one figure or station to the right, and the handle turned eight times for the second figure of the multiplier, and 6378320505 will appear under the holes *c*; move the plate B again to the right, and turn the handle five times for the third figure of the multiplier, and 44801938005 will be brought under the holes *c*; and finally, by moving the plate B once more to the right, and turning the handle six times for the last figure of the multiplier, the total product, 505885348005, will appear under the holes *c*, and the figures of the multiplier, viz. 6583, will appear in the holes *d*.



Colmar's Arithmometer

In division the operation is as simple as for multiplication, and is performed as follows: thus, to divide 414591904 by 4768, set up the dividend on the plate B, and the divisor on the plate A, commencing with the unit figure in each case to the right hand; place the knob E at Sub^{n} and Div^{n}, and move the plate B to the right until the second figure (from the left) of the dividend is over the first figure (4) of the divisor; turn the handle eight times, and 8 will appear in the quotient holes *d*, and will give the first figure of the quotient, while the dividend will now show 33151904, having been reduced by eight times the divisor, as in ordinary arithmetic; move the plate B one place to the left, and turn the handle six times for the second figure of the quotient, and the dividend will be further reduced by six times the divisor, and will mark 4543904; again move the plate, and turn the handle nine times, and after moving the plate B, and turning the handle five times and three times respectively, the holes *c* will all show noughts, and the quotient holes *d* will show 86953, which is the quotient required; if there had been any remainder, it would have appeared in the holes *c*.

Although by the ordinary limits of the machine a product of 16 places of figures and a quotient of 9 places of figures only can be obtained, yet by an intermediate record by the operator these limits may be virtually doubled for multiplication; while for division, provided the divisor does not exceed eight places of figures, the dividend and the quotient may be unlimited.

The use of the arithmometer in actuarial and other calculations has been shown in the papers read by Major-General Hannyngton and Mr. Peter Gray, F.R.A.S., F.R.M.S., respectively at the Institute of Actuaries (see the Journal of the Institute of Actuaries, p. 224, vol. xvi., and p. 249, vol. xvii.); and Mr. Thomas T. P. Bruce Warren, in a paper read before the Society of Telegraph Engineers, has shown the application of the instrument to electrical computations.

The Arithmometer is now, we believe, used in many Government Offices, in nearly all the Life Insurance Offices in England, in several Observatories; Sir W. Thompson, Prof. Tait, Prof. Galbraith, and Dr. Ball, also use them in the Universities and Colleges with which they are respectively connected.

The instrument can be seen, and all information obtained, of Mr. W. A. Gilbee, of 4, South Street, Finsbury, who, we understand, is sole agent for the Arithmometer.

*ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE**

III.

IMPERIAL STANDARD POUND

THE standard unit of imperial weight is the avoirdupois pound of platinum, constructed under the superintendence of the Commission for Restoration of the Standards. The mode of constructing this new standard of weight, together with full details of all the scientific processes employed, have been described by Prof. W. H. Miller, to whom its construction was more immediately entrusted. A drawing of the imperial standard pound has already been shown in Fig. 1.

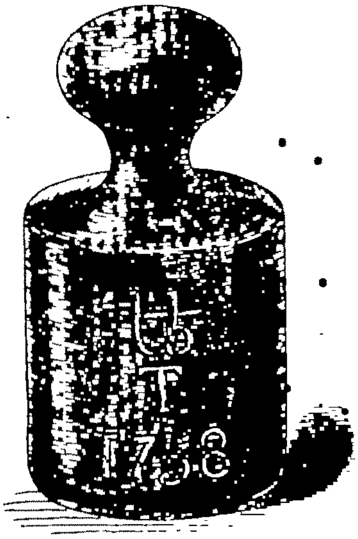


FIG. 4.—Form and size of the lost Standard Troy Pound

For constructing this standard, the first point to be determined was the exact weight of the lost standard Troy pound, from which the weight of the new standard Avoirdupois pound was to be derived. Upon investigation, this proved to be the most difficult problem to be solved by the Commission. The old standard had been constructed in 1758, together with two similar pounds, under the direction of the Parliamentary Committee of that year. It is stated to have been composed of gun metal, but unfortunately no record exists of its volume or density, and it is not probable that it was ever weighed in water. An accurate drawing of the lost standard pound had been made in 1829 by Captain Nehus, who measured its dimensions with the greatest care. (See Phil. Trans. 1836, p. 361.) It very nearly resembles a Troy pound now in the Standards Department, which was constructed at the same time, and is said to be the original from which the lost Standard was made. Its form and size are shown in Fig. 4.

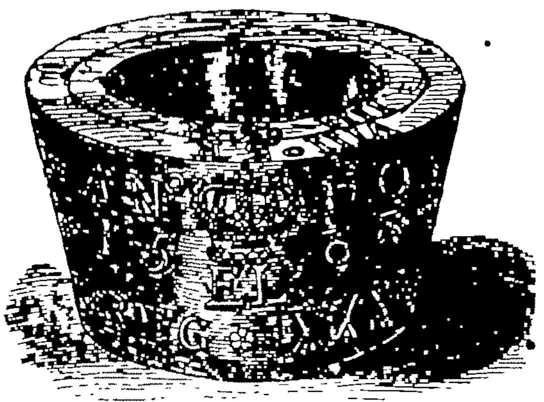


FIG 5.—Queen Elizabeth's Standard Troy Pound of eight and four ounces

When the Troy pound was constructed under the direction of the Committee of the House of Commons in 1758, it was made as nearly as possible of the genuine weight of the Troy pound according to the ancient Standard. For this purpose comparisons were made of the Exchequer Troy Standards with each other, and with other Troy standards belonging to the Mint and the principal scale-makers. At the period when the Troy pound of 1758 was constructed, there existed no distinct Standard Troy Pound at the Exchequer. The Exchequer Troy Standards of Queen Elizabeth, which were the legal standards in 1758, consisted of a binary series of Troy ounces from 258 oz. to $\frac{1}{8}$ oz., in the form of cup weights, fitting into each other. To obtain a Troy pound it was necessary to take the two Exchequer Standard weights of 8 and 4 oz., represented in Fig. 5.

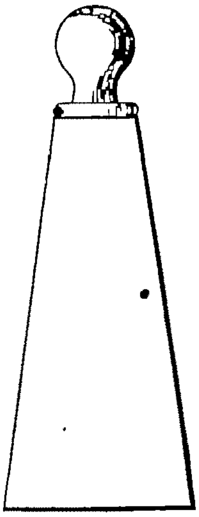


FIG 6.—Platinum Troy Pound, RS, of the Royal Society.

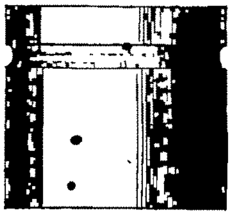


FIG. 7.—Platinum Troy Pound, T, of the Standards Department.

The form and size of the two platinum Standard Troy Pounds, RS and T, are as follows:—RS being a truncated pyramid surmounted by a knob, T cylindrical with a groove. The length of the side of the base of RS is 0·95 in., the total height 2·66 inches, the diameter of the base of T is 1·185 inch, the height 1·07 inch.

The two other Troy pounds constructed in 1758 were found by the Commission to be in existence, as well as two similar Troy pounds made at the same time and bearing similar marks, though all differed slightly in their dimensions, as well as in volume and weight. They were all in good preservation and were carefully examined by Prof. Miller, but there was no satisfactory evidence of their having been accurately compared with the lost standard so as to identify its weight, and thus to render them available for determining the proper weight of the new standard. One of the two last-mentioned pound weights (denoted as O by Prof. Miller) is shown in Fig. 4. This weight was purchased by the Commission, and is now deposited in the Standards' Department. It differs very slightly in its dimension from the lost standard, as shown above, and its weight in air was computed by Prof. Miller to be 5759·85625 grains of the lost standard.

For ascertaining the exact weight of the lost standard pound, the following weights, which had been accurately compared with it, were examined:—

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* Continued from p. 309.

The brass Troy Exchequer Standard pound, constructed in 1824 under the superintendence of Capt. Kater, and legalised as the official Standard;

Three similar brass pounds, constructed for the Cities of London, Edinburgh, and Dublin;

A platinum Troy pound and two brass pounds belonging to Prof. Schumacher;

The platinum Troy pound of the Royal Society.

It was found, however, from examining the results of several weighings of the brass Troy pounds that great discrepancies existed, attributable to the effect of oxidation or other causes. It was consequently resolved to rest entirely for evidence of the weight of the lost standard on the comparisons of the two platinum Troy pounds; denoted by Prof. Miller as Sp and RS. These two platinum weights had been constructed in 1829, and were intended to be equal to the lost Standard (denoted as U) when weighed in air. Each of them had been compared with V by Capt. Nehus at Somerset House in 1829, with the following results:—

Mean of 300 observations, Sp = U − 0·00857 grain, (mean $t = 65°\,62$ F. $b = 29{\cdot}722$ in.)

Mean of 140 observations, RS = U − 0·00205 grains, (mean $t = 65{\cdot}73$ F. $b = 29{\cdot}806$ in.)

The density of Sp had been determined by weighing it in water, to be 21·1874, and it was found to displace 0·32544 gr. of air of the stated mean temperature and atmospheric pressure. The density of U had never been determined, but it was assumed to be of the same density as one of the Troy pounds constructed at the same time, viz. 8·151, which is nearly the average density of brass and bronze weights, and to have therefore displaced 0·84646 gr. Whence in a vacuum Sp = U − 0·52959 gr.

The density of RS also had not been determined by weighing in water, but it was assumed to be of the same density as Sp, and therefore to have displaced 0·32629 gr. of air, whilst U displaced 0·84865 gr. Whence in a vacuum RS = U − 0·52441 gr. The mean value of the lost Standard Troy pound thus determined through Sp and RS, was the basis upon which the new Standard Avoirdupois pound was to be constructed. As a preliminary operation, a new platinum Troy pound, denoted as T, was constructed very nearly equal in weight to Sp and to RS, and taking the mean of 286 comparisons of T with Sp, and of 122 comparisons of T with RS, it was found that in a vacuum

$$T = Sp + 0{\cdot}00105, \text{ whence } T = U - 0{\cdot}52851$$
$$T = RS - 0{\cdot}00429, \text{ whence } T = U - 0{\cdot}52870$$

From the mean of these two results, giving to the first twice the weight of the second, in consequence of Sp having been compared about twice as many times with U and with T as RS was compared, it was finally determined that in a vacuum

$$T = U - 0{\cdot}52857 \text{ gr., or } = 5759{\cdot}47143 \text{ grs.}$$

It was also found that in air $t = 65°\,66$ F. $b = 29{\cdot}753°$ which was the mean of the comparisons of Sp and RS with U, and was adopted by Prof. Miller as the standard air,

$$T = U - 0{\cdot}00745 \text{ gr.}$$

It should be observed that all the standard Troy pounds were intended to be of their true weight in ordinary air, whilst the new standard imperial avoirdupois pound was to be made of its true weight when weighed in, or reduced to, a vacuum.

The next process was to determine the weight of the new avoirdupois standard pound, of 7,000 grains from the Troy pound T of 5,760 gr., and for this purpose four new platinum weights of 1,240 gr. each were constructed, all accurately verified in terms of T, and by employing other platinum weights of 800, 500, 400, 80, and 40 gr., the true weight in a vacuum of each of the 1,240 gr. weights was separately determined by numerous comparisons with T and with each other as follows:—

Grains.

$$\begin{aligned} A &= 1239{\cdot}88622 \\ B &= 1239{\cdot}88605 \\ C &= 1239{\cdot}88597 \\ D &= 1239{\cdot}88580 \\ \hline \text{Mean} &= 1239{\cdot}88601 \\ T + \text{Mean} &= 6699{\cdot}35744 \end{aligned}$$

It thus required only a weight of 0·64266 gr. to make up the full weight of 7,000 gr. The approximate weight 0·645 gr. was obtained from T in the following manner. By comparisons with the 40 gr. platinum weights, two platinum weights of nominally 20 gr. each, were found to weigh 19·998 gr. each, from which were derived W = 12·901 gr., V = 6·0451 gr. From V was derived Q the mean of ten weights of platinum wire, and equal to 0·645 gr. very nearly. It will be shown, hereafter, in describing the mode of weighing with a scientific balance, that small differences between two Standard pound weights of less than 0·1 grain are ascertained by the index scale of the balance. Means were thus afforded of determining the exact weight of 7,000 gr., which was the weight in a vacuum of the new standard pound, constructed of platinum, and denoted as PS, or Parliamentary Standard.

The weight of PS was actually determined by the mean results of 80 comparisons with each of the following sets of weights:—

	gr.		°		mm.
$PS \bumpeq T + Q + A -$	0·002936	in air $t.$ =	19·47 C.	$b.$ =	758·38
$PS \bumpeq T + Q + B -$	0·001731	,,	19·19		759·31
$PS \bumpeq T + Q + C -$	0·001621	,,	18·83		754·38
$PS \bumpeq T + Q + D -$	0·000774	,,	19·63		764·43

Mean of all, gr.

$$\therefore PS = T + Q + \tfrac{1}{4}(A + B + C + D) - 0{\cdot}00177 \text{ in air } t = 19°{\cdot}28 \text{ C. } b = 759{\cdot}12$$

The density of PS was determined by weighing in water to be 21·1572, and that of T and the smaller platinum weights to be 21·1661. PS consequently displaced 0·397 gr. of air, and T + Q + A displaced 0·39727 gr. Hence

$$PS = 7000{\cdot}00093 \text{ grains of which U contained 5760.}$$

Having arrived at this very close approximation to the desired weight of the new standard, it was resolved by the Commission that PS should be constituted the new Imperial Standard pound, and be consequently deemed to contain 7000·00000 grains of the new standard.

Four similar platinum pounds were also constructed, and their weight in terms of the new standard PS accurately determined. These were intended as auxiliary Standards of Reference, with the view that either of them might replace PS, in case of its destruction or damage. They were termed Parliamentary Copies (P.C.), and were deposited as follows:—

PC, No. 1, at the Royal Mint.
PC, No. 2, with the Royal Society.
PC, No. 3, in the Royal Observatory at Greenwich.
PC, No. 4, immured in the New Palace at Westminster.

Thirty-six other standard pounds of bronze gilt were also constructed, and their standard weight, both in a vacuum and in the standard air, adopted by Prof. Miller, was accurately determined, as well as the densities of all the new standard pounds. These gilt bronze pounds were distributed amongst different countries and public institutions of this country.

All the numerous weighings, both in air and in water, of the new standard pounds for determining their weights and densities were made by Prof. Miller himself, and full details of all these operations are given by him in his "Account of the Construction of the new National Standard of Weight."

The new imperial standard pound is of the true weight of an avoirdupois pound when in a vacuum. The principal advantage of the metal of which it is composed (platinum), consists in its not being affected by oxidation, which would unavoidably alter its absolute weight. But platinum has this disadvantage, if used as the material of a standard for regulating ordinary weights of precision made of brass, viz. that when weighed in air against a brass or bronze standard weight of so much greater volume, although of equal weight in a vacuum, its apparent weight is always about half a grain greater than that of the brass or bronze standard. To obviate this disadvantage, the weight in standard air of all the bronze Standard pounds verified by Prof. Miller were computed by him, *not in terms of the platinum standard pound, but of an ideal brass commercial standard pound, denoted by him as W.* He assumed W to be of the same density as the lost standard, and of the average density of brass or bronze. In standard air, $t = 65^\circ{\cdot}66$, $b. = 29{\cdot}75$ in. PS with a density of 21·1572 displaced 0·39644 grain of air, and W was assumed to displace 1·03051 grain. And as the official standard weights, by reference to which all commercial weights are verified, are made of brass or bronze, it was intended that they also should be regulated by their weight in air when referred to the brass commercial standard W. This has in fact been done. The only change since made has been under the sanction of the late Standards Commission, by which the standard air recited in the Act of 1824 for determining the weight in air of a cubic inch of water, viz. $t. = 62^\circ$ F., $b. = 30$ in. has been substituted for that adopted by Prof. Miller in consequence of its being the air in which the weight of the lost standard pound had been most accurately determined. The object of this change was to adopt one uniform normal temperature and barometric pressure for all standard purposes. In the new standard air ($\log \Delta = 7{\cdot}0852825 - 10$), PS displaces 0·40282 gr., and W, with a density of 8·1430, displaces 1·04706 grain of air.

H. W. CHISHOLM

(*To be continued.*)

THE TUSCAN MEMORIAL TO GALILEO

VILLARI, in speaking of Savonarola, and the men of his time, says:—"The world stood aghast at this new race of Titans, who arose to fight with the old idols, and it soon began to oppress them; but it worships their remains and lingers in their footsteps." And this is literally the case: the descendants of those Italians who burnt Savonarola at the stake, preserve, with religious care, the cell in which he wrote, morsels of his monkish garments and of his hair, his manuscript notes, indeed every memorial that remains of him. The custodian who showed us these remains, together with a picture representing Savonarola at the stake in his own Piazza della Signoria, of Florence, abused Alexander VI. and the Inquisitors, and the whole body of ecclesiastics concerned in the matter, so roundly and so fiercely, that we were led to wonder what manner of Catholic he could be; and to compare the Catholicism of 1872 with that of 1472. Thus, too, Galileo, persecuted during his lifetime, is now almost worshipped: the Tuscans have built him a shrine worthy of a saint; in the inscription on his house at Arcetri, they call him *Divinus Galilæus*; and in the shrine itself they have preserved, after the manner of a saintly relic, one of his forefingers which was detached from his body when it was removed from the chapel of SS. Cosmo and Damianus to Santa Croce. This relic is preserved in a small reliquary urn, upon the base of which is the following inscription written by Thomas Perelli:—

> "Leipsana ne spernas digiti quo dextera cœli
> Mensa vias nunquam visos mortalibus orbes
> Monstravit, parvo fragilis molimine vitri
> Ausa prior facinus cui non Titania quondam
> Sufficit pubes congestis montibus altus
> Nequicquam superas conata ascendere in arces."

Again we have *Via Galileo* and *Biblioteca Galileina*. The Pisans point with pride to the *Lampada Galileina* in their Cathedral, and honour his statue in their University; and these are the descendants of the men who paid Galileo tenpence a day for his services in the University; who made him abandon his professorship because he proved that Aristotle was not infallible; and who said derisively to his followers—"Ye men of Galilee, why stand ye gazing up into heaven?" or, as Ponsard has it:—

> "Ecoutez ce que dit l'Apôtre: *Dans les cieux*
> *Pourquoi Galiléens, promenez-vous vos yeux?*
> C'est ainsi que d'avance il lançait l'anathème
> Contre toi, Galilée, et contre ton système."

The Tuscan Memorial to Galileo is in Florence, in the *Museo di Fisica e di Storia Naturale*. It is entirely the work of Tuscans, and is said to have been constructed at a cost of 1,000,000 lire (nearly 40,000*l.*) It consists simply of a vestibule, from which opens a small rectangular hall, with a semicircular tribune, in which is placed the statue of Galileo, by Prof. Costoli. The interior of the hall is entirely lined with white marble, and with frescoes in admirable taste. The frescoes in the vestibule represent Leonardo da Vinci in the presence of Ludovic Sforza, Duke of Milan, to whom he is making known some of his great inventions. Apropos of this, there exists in the Ambrosian Library, in Milan, a large folio full of MSS. notes, and drawings, by Leonardo da Vinci, which the courteous director of the library is always willing to place in the hands of interested strangers, and which well repays the most careful examination. Some of the sketches of hydraulic apparatus, appeared to us to be worthy of minuter study than they appear to have received. The opposite fresco of the vestibule represents Volta explaining his invention of the pile to the members of the French Institute, in the presence of the first Consul, Napoleon, and Lagrange. In the vestibule are also placed marble medallions of Leo Baptista Alberti, and Baptista della Porta. A fresco in the hall by Bezzuoli, represents Galileo lecturing in Pisa, on the laws of falling bodies. This is a really striking and well-conceived painting: Galileo in his professorial toga stands by the long inclined plane, showing his results to his colleague, Mazzoni. In the foreground is a professor in a monastic habit, kneeling near the inclined plane, and counting the time of descent of the falling body, by the beats of his pulse. Young students are pressing round Galileo, in order, if possible, to aid him in his experiments; while on another side the Aristotelian professors are looking on with derision, and searching in vain in the writings of the Peripatetic for explanations of the new facts. In the background appear the cathedral and the leaning tower. The whole conception is noble and spirit-stirring, and one longs for a similar treatment of other great discoveries in science:—Davy discovering potassium, Faraday obtaining the first magneto-electric spark, and magnetising a ray of light. The opposite painting represents a meeting of the *Accademia del Cimento*: the patron of the Society, the Grand Duke Ferdinand II., is eagerly watching an experiment which is being made by Redi, Viviani, and Borelli, on the apparent (to them real) reflection of cold by a parabolic mirror:—one of the rough spirit thermometers recently invented by the Academy, is placed in the focus of the mirror, and a block of ice is used as the source of cold.

The three frescoes in the Tribune immediately around the statue of Galileo, represent three notable events of his life: in the first he is seen intently watching the swinging of a lamp in the Cathedral of Pisa; in the second we see him in the act of presenting his telescope to the Venetian Senate; and in the third he is represented as an old man, in his house at Arcetri, dictating the geometrical demonstration of the laws of falling bodies to his disciples Torricelli and Viviani. On the arch above the statue, the astronomical discoveries of Galileo—the Italians claim for him the Milky Way, the Nebula of Orion, the Phases of Venus, the Mountains of the Moon, the Satellites of Jupiter, the Solar Spots, and the Ring of Saturn—are represented very effectively on a blue ground. Bas-reliefs in marble on the pillars of the arch represent his terrestrial discoveries—his countrymen claim for him the Pendulum, the Hydrostatic Balance, the Thermometer, the Proportion Compass, the Keeper of Magnets, the Telescope, and the Microscope. Beneath the frescoes and around the statue are niches, containing some of Galileo's instruments, his telescope, an objective made by the astronomer himself, a proportion compass, and a magnet, with a keeper which he constructed for it. Immediately surrounding the statue we notice the busts of his most celebrated followers, Castelli, Cavalieri, Torricelli, and Viviani. In the hall there are six cases containing old apparatus, chiefly that of the Academy of Cimento. The various thermometers figured in the "Saggi di Naturali esperienzi" of the Academy are here to be seen; the vessels they used for showing the incompressibility of water; hygrometers; together with astronomical and geodesical instruments. Here, also, is the large burning-glass constructed by Bregaus of Dresden, employed by Averani and Targioni in their experiments on the combustion of the diamond, and afterwards employed by Sir Humphry Davy. The various inventions and discoveries of the Academy are shown in bas-relief on the pillars of white marble.

The memorial is altogether worthy of the man, and of the fine taste of the Florentines. It is, perhaps, the only *sanctuaire scientifique* which exists, but we may hope that the example of the Florentines will be followed in this and other countries. The Milanese have recently bought the collection of apparatus and the MSS. of Volta (for a sum, we believe, of 100,000 lire); a suitable museum for them will, no doubt, soon be fitted up. It is much to be wished that Faraday's apparatus could be collected together in one place, as a memorial to the man. This reminds us that soon after the death of Faraday a subscription was set on foot for the purpose of providing some suitable memorial, but we are unable to remember whether the designs of the committee were fully carried out, and whether the subscription attained the desired amount; if not, it is to be hoped that the matter will be kept well before the public.

We have spoken above of the discoveries attributed to Galileo by his countrymen. We are inclined to think that some of his claims have been pressed too far; but on such a subject an almost endless controversy might be carried on, for we may remember that even the invention of the telescope has been claimed for others of his own countrymen (Antonio de Dominis and Baptista Porta), and by the Dutch; and the invention of the thermometer has been attributed to Cornelius Drebbel, Sanctorio of Padua, and others. But if we put all this aside, Galileo still stands out pre-eminently as one of the fathers of experimental philosophy: he did not create it, but he introduced a taste for it, and enlarged it, and he possessed in an eminent degree the true spirit of philosophical inquiry, the ardent love of research, the "Provando et Riprovando" which the Academy of Cimento adopted as its motto.

G. F. RODWELL

THE SPHYGMOGRAPH AND THE PULSE

THERE are few valuable instruments or methods of research which have been brought before the scientific world under circumstances less auspicious than one, the inventor of which, the illustrious M. Marey, has quite recently visited this country. The sphygmograph, shortly after its first construction, was introduced into this country as an instrument which gave promise of being an invaluable aid in diagnosis, and of such universal applicability as the stethoscope and thermometer. Nevertheless, after an existence of more than ten years, it may be said that the general impression respecting it is that it is a failure, that it has not answered its expectations, and that it may as well be put aside, together with other curiosities of the physiological laboratory. How this result has come about is not difficult to discover. The instrument is a complicated one, and its indications are even more so. The stethoscope, when introduced, gave results at first sight palpable to the most ordinary minds, and the amount of mechanical knowledge necessary for the comprehension of some of its most striking results scarcely exceeded that of the principle of the common pump. But with the sphygmograph the case is different. Its indications are so detailed and so precise that before they can be understood, it is absolutely essential that several intricate and elaborate problems of hydrodynamics and physiology should be thoroughly investigated, and more than one of these have not, we are surprised to say, yet left the hands of the mathematicians in any decided form. How then is it to be expected, as it has been by many, that the sphygmograph should be found a valuable assistance in the diagnosis and prognosis of disease, before the physicist and physiologist can give an explanation of the language in which it appeals to them? There is no doubt that the instrument must be in the hands of the student of the healthy body for some time to come before its true value in the elucidation of disease will be appreciated; and all additions to our knowledge concerning it must be carefully weighed.

In a thesis for the M.D. Cantab.,* Dr. Galabin has published several results of his sphygmographic work in the study of renal disease, and what is more to the point on the present occasion, he gives his own ideas as to the analysis of the same trace in health. The fact of the author's being an accomplished mathematician, as well as a student of biology, gives more than ordinary weight to his remarks, and enables him to put several points in a light which is clearer and more precise than usual.

The author does not enter fully into the reasons in favour of his views, and does little more than simply state them; but as they differ in some respects from those generally accepted, they present features of interest to workers on the subject. He is one of those who consider the trace as it appears on the recording paper as a decidedly duplicate phenomenon, resolving it into the true pulsation, together with the oscillations of the lever, which necessarily result from the momentum acquired by its sudden movement. This he illustrates by superimposing on an ordinary pulse curve, as taken by the sphygmograph, an ideal one, such as, according to his conception, it would be if the instrument correctly followed the changes in the diameter of the artery under observation; the latter being little more than a uniform rapid rise followed by a similar but slower fall, that is slightly broken by the "dicrotic" wave, which is produced by the closure of the aortic semilunar valves. The excessive height of the primary rise is supposed to be due to the powerful impulse given to the lever at the commencement of the flow of fluid in the artery; and the small secondary, or "tidal" wave, which occurs just before the "dicrotic," is supposed to indicate the true arterial expansion, which the lever meets on falling from the height of its impulse. We quite agree with part of this explanation, being fully convinced, from many reasons, that the primary rise, or so-called "percussion" wave, is not a percussion wave at all in the ordinary acceptation of the term; in other words, that it does not result from the shock produced by the opening of the aortic valve, but that it is coincident with the flow of liquids, one reason being, as the author remarks, that the most violent impulse in an artificial model or schema of the circulation so communicated, as not to cause any flow of liquid, produces no upstroke, but only a slight quivering of the lever. However, that the primary oscillation of the lever in a sphygmograph trace is not, in a great measure, a genuine representation of the movements in the artery, it is equally impossible to believe, for in very slow pulses, where the main rise is not very decided, this wave is particularly pronounced, being gradual in its rise, and more gradual and paraboloid in its fall. It is also seen equally clearly by employing a reflecting sphygmoscope, in which the ray of light which acts as the long arm of the lever, has no weight, and consequently cannot produce any oscillation. Another great objection is that the notch between the first and second (the percussion and the tidal) waves always occurs at the instant at which the aortic valve closes at the heart,* the time it appears after the commencing pulsation *varying* with the length of the pulse-beat. In fact, the tendency of all observations is to make it evident that the second or tidal wave is a post-systolic act, being the oscillatory indication of the secondary tidal wave, which appears as such in the dicrotic rise, and originates from the closure of the aortic valve, as Dr. Galabin agrees with most in thinking; though Dr Sanderson holds the very different view that the second beat is a restoration of equilibrium which takes place by increase of pressure towards the heart and diminution towards the periphery, a consequence of the sudden projection towards the capillaries of the blood during the systole.

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* "On the Connection of Bright's Disease with Changes in the Vascular System." By A. L. Galabin, M.A. M.D., Fellow of Trinity Coll., Camb.

Dr. Galabin remarks that, "if the sphygmograph used have a secondary spring to keep down the long lever, the tidal wave may be replaced by two or even by a jagged line. Such a spring is better omitted, because it is apt to introduce oscillations of its own." It is this idea which has misled him. Tracings taken as he proposes appear much in favour of his explanation, but they are so because they are in reality less truthful than they might be. We have never seen the least indication of any imperfections caused by the employment of the small spring, but we have seen the "percussion" wave divided into two by it in very slow pulses, the former being a small true shock-rise, and the latter the real primary rise.

In conclusion, we cannot refrain from quoting a remark of Dr. Galabin, which, from the precise way in which it sets the question referred to at rest, is worthy of being quoted in every text-book. Referring to the rhythmical contraction observed by Wharton Jones and Schiff in the wing of the bat and the ear of the rabbit, and its supposed influence in assisting the circulation of the blood, he remarks, "Now a peristaltic wave in a tube would tend to produce a current in the liquid of its own velocity, and it would, therefore, accelerate a slower current, but retard a quicker one. Therefore, no peristaltic wave could accelerate the arterial stream, unless it travelled with the velocity of the pulse-wave. It is thus evident that no such slow rhythmical motions as have been observed could assist the arterial flow. And it is inconsistent with the usual character of involuntary muscle to suppose it capable of transmitting a very rapid wave of contraction. The arteries themselves indeed, when made to contract by artificial stimulus, do so slowly and gradually."

A. H. G.

AMERICAN EXPLORING EXPEDITIONS IN THE GREAT WEST

THERE are several important expeditions more or less employed upon scientific work in the least known portions of the Western territories. From some of these parties, a considerable amount of fragmentary information comes at irregular intervals; but in other cases the explorers prefer to withhold details as to their movements and work, whether scientific or otherwise, till after their return, when their report can be prepared officially. There is, however, a general and widespread interest taken in these explorations. It seems desirable for the sake of a clear understanding of news from time to time received, that a general *résumé* of the status and work of at least the more prominent expeditions should be presented.

What is known as the Yellowstone Expedition will first be mentioned, because in size it is much the most formidable. It proceeds through a region where it is deemed advisable to strike terror among hostile savages, and with that view has a military force of 1,900 men. Its movements also have reference to the establishment of two new military posts in the north-west, for which purpose Congress has appropriated 200 000 dollars. The force serves as an escort to surveying parties of the Northern Pacific Railroad, with reference to its completion from the town of Bismarck on the Missouri River in Dakota—about the centre of that territory, and near the 101st parallel west of Washington—to the Rocky Mountains; a distance of between 500 and 600 miles, on a line drawn in general east and west, and south of 47° N. lat.

This line may be divided into three parts; (1) from the Missouri River to the Yellowstone, about 200 miles, coming into the territory of Montana; (2) along the Yellowstone River about 100 miles; (3) thence west-ward, reaching the Rocky Mountains south of the town of Helena. At the date of latest advices, the expedition had passed over the first division, and was on the banks of the Yellowstone. The navigability of that river had just been demonstrated by the successful ascent of a steam-boat, built at Pittsburg for the purpose, which brought supplies from Fort Buford.

Of the scientific party accompanying the Yellowstone Expedition, the following names may be mentioned:—Dr. J. A Allen, of Cambridge, Mass., in charge of zoology, botany, and palæontology, and chief of the scientific party; Dr. L. R. Nettre, mineralogist and geologist; W. R. Pywell, of Washington, photographer; E. Konipucky, of the Museum at Cambridge, artist; and C. W. Bennett, taxidermist.

The Hayden Expedition, as that under the management of Dr. F. V. Hayden is generally termed, might be more properly designated as the United States Geological and Geographical Survey. It has a much larger scientific staff than any of the other expeditions. Its history dates from 1867, when what was then the territory of Nebraska was the subject of a survey by the United States, Prof. Hayden being appointed chief geologist to the survey under the Act of Congress by which the undertaking was authorised. The next year the survey was extended into Wyoming Territory, and in 1869, into Colorado and New Mexico. In 1870, a more careful survey of Wyoming Territory was made; and in 1871, portions of Montana, including the natural wonders of the Yellowstone region, became the subjects of exploration; ultimately resulting in the setting apart as a public pleasure-ground of the Yellowstone National Park, a district of 3,575 square miles. The survey of 1872 reached the region of the Yellowstone by separate routes of two divisions, of which one proceeded from Fort Ogden, Utah, and passed up the Valley of the Snake River in Idaho Territory; the other started from Boseman, a town in Montana near the Rocky Mountains, and on one of the Upper Forks of the Missouri River. The appropriations for this series of surveys have been increased year by year, starting with 5,000 dollars in 1867, and rising to 75,000 dollars for the survey now taking place.

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* See Proc. Roy. Soc., 1871, p. 320.

The district of this year's operations may be specified as the eastern half of mountainous Colorado, includes about 32,000 square miles, and is bounded east by long. 104° 30′, north by lat. 40° 25′, west by long. 107°, and south by the southern boundary of Colorado, lat. 37°. It is divided for the purposes of the survey into three parts by latitude lines 38° 30′ and 39° 30′; the northermost being called the "Middle Park Division," the middle one the "South Park Division," and the southern one the "San Luis Division." The examination of the gold and silver mines of the region, and the measurement of its mountains, are among the more important duties of the survey. Unusual prominence is given to procuring pictures by photographs and otherwise.

The camp was organised at Denver College in May; the expedition started thence July 1, numbering 41 men. The season has been unusually favourable, the streams being low and but little snow or rain falling. The location of the camp at the latest advices was on the eastern slope of the Rocky Mountains, at the head-waters of the Platte, Arkansas, and Blue Rivers. Accurate measurements of some of the more prominent peaks, among which are Pike's, Long's, Evan's, Gray's, Lincoln's, and the Holy Cross, have been obtained. The views from these summits where the snow melting on one side flows to the Atlantic, and on the other to the Pacific, are of vast scope and magnificence. There were in sight from one point by actual count, 150 peaks of not less than 13,000 ft., and at least 50 of 14,000 ft. in height. By the middle of July 150 stereoscopic views, and 50 11 × 14 in. negatives of this scenery had been secured. The mountains have, very generally, at a depth of 50 to 200 ft. from their surface, a limestone stratum 30 to 50 ft. thick, containing silver and lead, yielding on the average in the best mines 250 to 300 ounces of the former metal, to the ton of ore. The carboniferous and silurian rocks identified are said to contain rare fossils. The entomologists of the expedition have classified no less than 227 different kinds of grasshoppers. The direction of march projected at last accounts was to be toward the valley of the Upper Arkansas River and the unexplored region beyond.

There are more than 20 scientists taking actual part in the expedition: among them may be mentioned Dr. F. V. Hayden, geologist-in-chief; Mr. J. T. Gardner, chief geographer, who has attained great reputation in his connection with previous geodetic surveys in Colorado and on the Pacific coast; Mr. Marvine, geologist of the Middle Park Division; Mr. Henry Gannett, meteorologist and astronomer, and topographer, in charge of the South Park Division; Mr. W. H. Jackson, in charge of the photographic party; Dr. Endlich, geologist; Lieut. W. L. Carpenter, naturalist; Mr. Seward Cole, ornithologist; Mr. J. M. Coulter, botanist; Dr. A. C. Peale, geologist; Prof. W. D. Whitney, of Yale College, who is writing a series of interesting letters concerning the work, to the *New York Tribune*.

On some accounts the expedition of Prof. O. C. Marsh, sometimes known as the Yale College Expedition, because the fossils collected are sent to that institution, ranks next in importance. This is purely a private undertaking, at the expense of the persons composing the expedition. The United States Government furnishes a small but sufficient military escort. The reports are published under Government auspices. This is the fifth of a series of expeditions similarly undertaken by Prof. Marsh, has no reference to surveying or topography, and is devoted exclusively to research for the remains of extinct vertebrates in the tertiary and cretaceous formations. The districts explored hitherto with such remarkable success will probably supply the fields of the present undertaking. The directions previously taken were as follows:—

First expedition, in 1868, to Lake Como, Wyoming Territory. Second, in June, 1870, to the Loup Fork River, in Nebraska; the Bad Lands east of the Black Hills and between the North and South Forks of the Platte, in Wyoming and Colorado; and the Great Basin of the Green River, southward from Fort Bridger, bordering Utah. There were also minor trips during this expedition to Green River, in Wyoming, and to the Smoky River, in Kansas, which were productive of valuable results. The third expedition started in the summer of 1871, and again explored the Smoky River region in Kansas, the Green River Basin, above mentioned, and investigated two basins, likewise of the Tertiary age, one in Idaho and the other in Oregon. The fourth was a trip with a comparatively small party in the autumn of 1872. It concentrated at St. Louis, went to Fort Wallace by way of Kansas City, and, receiving escort, proceeded to Smoky Hill Fork. On this expedition some explorations were made near Cheyenne, and several days were spent in researches, with varying success, at Crow's Creek, Colorado.

At the most recent dates the present expedition, leaving North Platte Station on the Union Pacific Railroad, had made a nine days' march through a desert country, undergoing great hardships; had reached the Niobrara River, made investigations on both its banks for more than 100 miles below the mouth of Rapid River, and had returned, laden with fossils, to Cheyenne, expecting to make the next start from Fort Bridge in Wyoming Territory. This expedition may extend its researches, as Professor Marsh informed the writer, to the Pacific Coast, and is not expected to return till late in the autumn.

The expedition known as the Wheeler Exploration Party is under the management of the U.S. War Department, Bureau of Engineering. Its chief is Lieut. G. M. Wheeler, of the U.S. Engineers. The operations of the present season will consist of exploration and survey west of the 100th meridian and south of 40°, principally in New Mexico and Arizona, down to the borders of Mexico. The following are named amongst the scientific force:—Messrs. Henry Leubbers, G. Thompson, J. J. Young, and E. Somer, topographers; G. R. Gilbert, E. E. Howell, J. J. Stevenson, and Oscar Loew, geologists; H. W. Henshaw and John Wolfe, naturalists; B. Gilpin, meteorologist; J. H. Clarke, Dr. F. Kampf, W. W. Marryatt, and Prof. H. B. Herr, astronomical observers. The establishment of an astronomical observatory, substantially built of brick, having three observing-rooms, at Ogden, Utah, will form part of the labours of this expedition, which concentrated its forces to start from Denver in June last.

There is an expedition under command of Capt. W. A. Jones, of the U.S. Engineers, which started from Omaha on the 2nd of June. Its objects are mainly topographical, having direct reference to the Yellowstone National Park; but it may be extended to the Big Horn country, a wild region imperfectly known, and said to be fabulously rich in minerals, situated south of 44° and between meridians 106 and 108. Among the scientific men attached to this party are Lieut. S. E. Blunt, astronomer; P. Le Hardy, topographer; Dr. C. C. Parry, botanist and mineralogist; and Mr. T. B. Comstock, of New York, geologist.

Whether there is a surveying party under Mr. Clarence King, geologist, still in the Wasatch Mountains, at work on the line of the 40th parallel; whether that of Major J. W. Powell has returned from its investigations having principal reference to the cañons of Colorado; and whether a party that went from Philadelphia—consisting principally of Prof. Joseph Leidy, palæontologist, Dr. Henry Chapman, zoologist, Mr. Joseph Willcox, mineralogist, all of that city, and Prof. Porter, of Easton, Pennsylvania, botanist—is still in the wilds of Wyoming and Colorado, the writer is unable, at the present date, to determine.

New York, Aug. 8

NOTES

FROM a private letter just received from Prof. Wyville Thomson, we learn that the *Challenger* left St. Vincent, Cape Verde Islands, on August 2, for Bahia, for the purpose of making her fourth section across the Atlantic. As it is now the middle of the rainy season, and as part of the course of the *Challenger* lies along the coast of Africa to the southward, the members of the expedition expect to be very uncomfortable for a time. On July 15 a very successful month's cruise from Bahamas was completed, some of the details of which we expect to be able to publish next week. "We are getting on first rate," the letter says; "the arrangements continue very complete and satisfactory."

THE French Association for the Advancement of Science opens to-day at Lyons, under the presidency of M. de Quatrefages.

THIS year's meeting of the Iron and Steel Institute opened on Monday at Liège, where the members received a most enthusiastic reception. The first meeting was held at the Academic Hall of the University, when Mr. Lowthian Bell, the President, delivered a speech, in which he warmly thanked the Belgian ironmasters for their friendly reception, and then spoke at length on various technical matters. On Tuesday a second meeting was held, when several papers were read. It was announced that the members were invited to hold their meetings next year in the United States. Many fêtes, receptions, and other entertainments have been got up for the members, who are also to visit the principal mines and iron foundries of the district. To-day the members are to be received by the King of Belgium at the Royal Palace in Brussels.

THE British Archæological Association commenced its yearly meetings at Sheffield on Monday, under the presidency of the Duke of Norfolk, who entertained the members, and others, at dinner in the evening. The members received a hearty welcome from the town, and have been visiting several places of interest in the neighbourhood. On Tuesday evening several papers were read in the Cutlers' Hall on Yorkshire archæological and antiquarian subjects. Among these was a paper by Mr. J. R. Planche, Somerset Herald, on "The Early Lords of Holderness," and one by the Rev. Dr. Gatty, on "The Town and Parish Church of Sheffield."

THE twenty-fifth annual meeting of the Somersetshire Archæological and Natural History Society commenced at Wells on Tuesday. The opening meeting was held at 12 o'clock at the Town Hall, the retiring president, Mr. W. A. Sanford, of Minehead Court, taking the chair. After a brief speech he resigned the presidency to Lord Hervey, the Lord Bishop of the diocese. In the report of the Council, the following subjects, among others, were referred to:—The druidical circles of Stanton Drew, the chambered tumulus of Stoney Littleton and Cadbury Camp have, through the influence of the Council, been enumerated in Sir John Lubbock's Bill for the preservation of public monuments. It is proposed to purchase the castle of Taunton as a museum for the rapidly growing collections of the society; 3,000*l*. are wanted. Mr. Ayshford Sanford, in urging the purchase of Taunton Castle, mentioned that it is the oldest fortress of English origin in the west of which the date is certain. It was built by King Ina, about the year 700, and has a Norman keep, and specimens of architectural additions of every date down to the Perpendicular. The earthworks are in good preservation. Mr. E. A. Freeman, D.C.L., in speaking on the question whether the next meeting should be held out of Somersetshire, said the study of the Church architecture of the district was incomplete unless it included Sherborne Minster at one extremity and St. Mary Redcliffe, Bristol, at the other. Sherborne, too, was the old bishopric out of which Wells was carved. After some routine business, the Bishop gave his address. He pointed out some peculiarities of Somersetshire as a county, its many double-named places, its number of small holders, and the absence of any old baronial seats.

THE *Gazette d'Augsbourg* contains some interesting details in connection with the recent meeting at Copenhagen of the Scandinavian Scientific Congress. This is the oldest of the many northern societies, having been instituted at Gothembourg in July 1839. Among the original members are the names of E. H. Œrstedt, J. F. Schouw, Forchhammer, E. Fries, Nilson, Berzelius, Hansteen, all men of the highest eminence in their own departments. The meetings of this Association are held alternately at longer or shorter intervals, in each of the three Scandinavian kingdoms, at Copenhagen, Stockholm, and Christiania, the kings of the countries always showing an active interest in the doings of the Association. At the recent—the eleventh—meeting at Copenhagen, the number of members was 400, the President being M. Steenstrup, who delivered the opening address in the presence of the King and Crown Prince of Denmark. The meeting was divided into ten sections, in each of which many papers were read; general meetings were also held, and several excursions made to places in the neighbourhood.

MR. SMITH, the leader of the *Daily Telegraph* Assyrian Expedition, gives in the *Telegraph* of Tuesday a number of interesting details of his work. He gives a translation of the tablet which relates the curious legend of the descent of Ishtar, the "daughter of Sin" (the moon-god), into the infernal regions. The boxes containing the more portable of the treasures exhumed by Mr. Smith have, after many hazardous adventures, safely reached this country. These, with several very valuable memorials purchased in Mesopotamia by Mr. Smith, and the expense of which the proprietors of the *Telegraph* have very generously charged themselves with, are now safely lodged in the British Museum. The heavier articles are expected to arrive in this country very shortly.

THE following among other exhibitors have received diplomas of honour at the Vienna Exhibition:—In the Mining Department: the Geological Survey Office, Calcutta. In Group 22: the South Kensington Museum, London. Educational matters: the National Educational Bureau, Washington; Dr. Leitner, Lahore, India; the Government of Massachusetts; and the Smithsonian Institution, Boston, U.S.

MR. G. F. RODWELL, Science Master in Marlborough College, has resigned the Lectureship on Natural Philosophy in Guy's Hospital.

WE should advise all connected with Science teaching in schools connected with the Science and Art Department, to obtain a copy of the new syllabus in the following subjects, just issued by the Department:—Subject XIV., Animal Physiology; XV., Zoology; XVI., Vegetable Anatomy and Physiology; XVII., Systematic and Economic Botany. From the Syllabus it will be seen that (*a*) Subject XIV., Animal Physiology, is altered in certain details. (*b*) Subject XV., has now become "Elementary Botany," being a modification of the former Subject XVII., Systematic and Economic Botany. (*c*) Subjects XVI. and XVII. together now form a new subject, Biology, into which the former subjects of Zoology and Vegetable Anatomy and Physiology are absorbed. The elementary stage is the same for both Subjects XVI. and XVII., the advanced stages of these subjects being respectively Animal Morphology and Physiology, and Vegetable Morphology and Physiology. As respects the existing qualifications of teachers for earning payments on the results of instruction, the deductions in those payments on account of the previous success of the pupil, and the prizes to the pupils—(*a*) Subject XIV., Animal Physiology, will be in no way affected by the change now made in the syllabus. (*b*) Subject XV., Elementary Botany, will be treated as if it were the same as the former Subject

XVII., Systematic and Economic Botany. (c) Subjects XVI. and XVII. will be treated as perfectly new subjects, except that all persons will be qualified to earn payments on results in those subjects who are now qualified in Subject XV., Zoology, and Subject XVI., Vegetable Anatomy and Physiology, and also all those persons who have obtained a class at the courses in Biology and Botany respectively for teachers at South Kensington. (d) As the elementary stage of Subjects XVI. and XVII. is the same, payments can only be made on account of a pupil's success in one or the other, and not in both. Payments for the advanced stage and for honours can be obtained in both.

THE following are the regulations for exhibiting Recent Scientific Inventions and Discoveries of all Kinds, at the International Exhibition of 1874:—Division III. Recent Scientific Inventions and Discoveries will consist of objects the excellence and novelty of which are considered by the Committee of Selection to be so great as to render it undesirable that their introduction to the public should be delayed until the proper year for the exhibition of their Classes of Manufacture in Division II. No objects will be admitted into Division III. which have been shown in previous International Exhibitions of this series, unless very important alterations or improvements have been added to them since the date of their previous exhibition. The latest day appointed for receiving objects in this Division is Wednesday, March 11, 1874.

THE Birmingham Natural History and Microscopical Society propose to undertake a novel and commendable enterprise in the shape of a marine excursion. The sub-committee appointed to consider the practicability of the proposal are of the opinion that if such an excursion be properly carried out, it cannot fail to be productive of interest and enjoyment to the members. Taking all matters into consideration, the sub-committee are of opinion that the South Coast of Devon is the most favourable for the proposed excursion; and if Teignmouth be selected as headquarters, it will allow of dredging and shore collecting in the vicinity, and in Tor Bay, and off Berry Head, as well as botanical and geological excursions in the neighbourhood, and (if time permits) visits to the wilds of Dartmoor and the beautiful and picturesque scenery of the River Dart, Holne Chase, Lustleigh Cleave, Becky Falls, &c. It is proposed that the excursions commence on Monday, September 1, which would allow six clear days dredging in the neap tides after the August new moon, and some shore collecting during the September full moon. A first-class yacht, with two men and a boat, can be hired for a very moderate sum, and the Midland Railway Company offer return tickets at very moderate rates with the privilege of staying in Devonshire for 17 days. Various members have undertaken to superintend the dredging, botanical, microscopical, and geological work, and altogether the arrangements proposed are very complete and seem likely to make the excursion a success. We hope it will prove so, and that the example of the enterprising Birmingham Society will be followed by others, either singly or in combination. Inquiries should be addressed to Mr. W. G. Blatch, Hon. Secretary, Green Lane, Small Heath, Birmingham.

THE Brighton and Sussex Natural History Society has determined to collect facts in connection with the Natural History of Sussex, for the purpose of verifying existing Lists, and preparing (with a view to ultimate publication) an authentic systematic record of the land and marine fauna and flora of the county. The Society will be much obliged to all who can render assistance in any or all of the following ways:—(1) By forwarding to the Society lists of such species as may have fallen under one's own *personal* notice; (2) by contributing facts relating to such points as *approximate* locality (in order to prevent the extirpation of rare species, the approximate, and not the exact, locality is asked for); whether rare, local, or common; accidental variations; apparent extinction and re-appearance; times of appearance; any noteworthy matters connected with the life history of species; and (3) by sending specimens to be deposited in the Brighton Free Museum or other Museums in the county. Communications will be thankfully received by Mr. R. Glaisyer, Honorary Curator, Dispensary, Queen's Road, Brighton; or by T. W. Wonfor, and Jno. Colbatch Onions, Hon. Secs.

SCHIAPPARELLI has recently published two very interesting memoirs, the one an elaborate historical monograph on "The Precursors of Copernicus," and the other on "Falling Stars."

SIGNOR AUGUSTO RIGHI, Demonstrator of Physics in the University of Bologna, has published a very interesting memoir, *Sul Principio di Volta* (Bologna: Tipi Gamberini e Parmeggiani, 1873). In this he discusses at great length Volta's theory of electrical excitation. A number of original experiments are given, and photographs of a new apparatus employed for them.

ENGINEERS have been busy on the estate of Mr. W. Gilford, at Dalby-on-the-Wold, and other places in Leicestershire, investigating the allegation that the Midland coal measures extend in an almost direct line from near Leicester to Melton Mowbray, and through the Vale of Belvoir, embracing an area of many square miles. As the reports made are of a highly favourable character, and as the importance of having a coal-field close to the town of Leicester can scarcely be over-estimated, it is proposed to bore to a depth of 1,000 ft., and to divide the expense *pro rata* amongst the landowners. Several of those most interested have signified their desire to have the problem solved in the only practical manner. Mr. Harrison, of the Mining School, Nottingham, is of opinion that "coal exists under East Notts and East Leicestershire, there being an anticlinal fault throwing out all the measures in the western part of Notts, and throwing them all in on the eastern side. From this and other considerations" he is convinced "that there is an immense coal-field stretching along the county of Nottingham, by Bingham, through the Vale of Belvoir, as far as Melton Mowbray, and will be found at a workable depth."

AT the last monthly meeting of the council of the Victoria Institute, it was announced that seventy-nine new members had joined during the past seven months. It was also reported that in accordance with a resolution passed at the previous meeting the Institute had joined in the application made to the Government for adequate aid to the expeditions to observe the transit of Venus, more especially those so strongly urged by the Greenwich board.

THE valuable library of Conchological and other Natural History books belonging to the late Mr. Thomas Norris, of Preston, was sold by auction on July 30, by Mr. J. C. Stevens, for 322*l*. Mr. Stevens also sold, on Aug. 7, the library of the late D. H. Beaumont Leeson, F.R.S., of Bonchurch, for 580*l*.

THE recent earthquake in South America extended, it is stated, over 30,000 square miles.

THE following is from the *Gardener's Chronicle*:—"We learn that Baron von Mueller is about to retire from the directorship of the Botanic Garden, Melbourne. On scientific grounds this is much to be regretted, for no one has done so much as the Baron to forward the interests of Botanical Science and practical applications in Australia as he has done. We cannot profess to judge the circumstances which may have led to this step; but if, as is alleged in some of the Melbourne papers, 'the gardens are henceforth required more as an ornamental adjunct to the Vice-regal domain than as the centre of Botanical Science and experiment in Australia,' then undoubtedly the authorities manifest an ignorance of the proper functions of a botanic garden which is, unhappily, not confined to Australia. Everyone must desire that the garden should not be a 'cheerless' 'scientific desert' at the same time it is equally clear that it should not be transformed merely into 'a pleasure-ground worthy of the name.' It is satisfactory, however, to learn that the Baron's services to the State will not be lost, that he will not suffer in pocket by the change, and that additional and much needed assistance will be given him."

THE *Canadian Ornithologist* is the name of a serial started last month, "with the object of making a monthly depository of facts, theories, and anecdotes relating to our feathered friends." Dr. Ross of Toronto is the editor. The first number leaves much room for improvement in its successors.

THE last number of the *Journal of the Society of Arts* contains a report by Dr. R. J. Mann, on "Recent Scientific Inventions and New Discoveries at the International Exhibitions."

THE following is the list of candidates successful in the competition for the Whitworth Scholarships, 1873 :—Samuel Dixon, 23, draughtsman, Manchester; Roger Atkinson, 20, analytical chemist, Crewe; Joseph Amscow, 22, chemist, Crewe; W. R. Bousfield, 18, student, Cambridge; W. H. Warren, 21, engineer, Wolverton; William Barber, 20, draughtsman, Nottingham; William H. Fowler, 19, engineer, Oldham; Thomas Sugden, 23, mechanic, Oldham; Cyrus Bullock, 22, millwright, Worsley, near Manchester; John Lockie, 20, engineer, Glasgow.

THE following gentlemen have passed in the First Division on the First B.Sc. Examination for 1873, in the University of London :—P. Bedson, E. B. Cumberland, T. F. Harris, S. A. Hill, W. Hudson, J. Viriamu Jones, O. Lodge, J. G. MacGregor, W. R. Parker, T. S. Tait, C. M. Thompson, A. T. Wilkinson, B.A.

THE "Proceedings of the Geologists' Association," for July, is almost wholly occupied with an account of the interesting and instructive excursions of the Association during the summer months of last year. It contains, besides, a paper by Mr. John Paterson, "On a Visit to the Diamond Fields of South Africa," and another by Mr. John Curry, "On Columnar Basalts."

THE "Mineral Statistics of Victoria for 1872," are made up as usual of a host of tabulated details of all kinds, relating to the minerals and mines of that colony. Owing to changes in the law it seems to be more difficult than heretofore to collect accurate statistics as to the quantity of gold raised, many mine-owners being unwilling to furnish returns. According to returns furnished by the Commissioners of Trade and Customs, the quantity of gold exported in 1872 was 1,160,554 oz. 19 dwts., the estimates of the Mining Registers being 1,331,377 oz. 18 dwts.

A SPECIAL Report on Emigration by the American Government has been sent us, containing a great amount of information likely to prove very valuable to intending emigrants, as well as to statisticians. Not only does it contain statistics as to the number, nationalities, &c. of emigrants during the last few years, but much information as to rent of land, staple products, kind of labour in demand, wages to be earned at various trades and occupations, &c.

THE additions to the Zoological Society's Gardens during the past week include a Silvery Gibbon (*Hylobates leuciscus*) from Java, two Slow Loris (*Nycticebus tardigradus*) and a Binturong (*Arctictis binturong*) from Malacca, a Tiger (*Felis tigris*) from India, presented by Sir Harry Ord, C.B.; a Malay Bear (*Ursus malayanus*) from Borneo, presented by Mr. A. C. Crookshank; a common Marmoset (*Hapale jacchus*) and a Black-eared Marmoset (*H. penicillata*) from Brazil, presented by Mr. J. Stanley; a Cornish Chough (*Fregilus graculus*), presented by Mr. G. Holford; a Gazelle (*Gazella dorcas*) from Muscat, presented by Major C. B. E. Smith; two Blue-headed Pigeons (*Starnaenas cyanocephala*) from Cuba, a White-headed Saki (*Pithecia leucocephala*) from Demerara, and a Hawk-headed Parrot (*Deroptyus accipitrinus*) from Brazil, deposited.

SCIENTIFIC SERIALS

THE *Zoologist* for this month commences with an interesting paper by Mr. T. H. Potts, who is paying so much attention to the birds of New Zealand, on the habits of the Night Parrot of that country (*Stringops habroptilus*). One of its favourite foods is the younger part of the fern *Asplenium bulbiferum*, called Piki-piki, which, being only partly digestible, forms large pellets of excreta on the floor of their tunnel homes. All those who have kept a bird of this species as a pet, agree in testifying to its intelligence and companionableness.—Mr. Cecil Smith, among his ornithological notes from Somersetshire, records experiments, suggested by Prof. Newton, with a view of ascertaining how far birds in general, and especially some of the foster-parents of the cuckoo, have any objection to eggs of a different colour being placed in their nest. In nearly every case the exchange was perfectly successful.—Mr. Gatcombe had an opportunity of examining a Night Heron obtained near Ivybridge, in Devon; he also records other ornithological notes.—A specimen of *Scyllarus arctus* is mentioned by Mr J. S. Bowerbank, as having been obtained by him at St. Leonard's (it was five inches long), as well as an Angel Fish.—Mr. A. G. Butler finds, as one of the effects of the Wild Birds Protection Act, that farmers employ boys to collect and break up all the eggs on their grounds, as they are now deprived of the satisfaction of destroying the birds.

SOCIETIES AND ACADEMIES

LEEDS

Naturalist's Field Club and Scientific Association, Aug. 5.—Mr. Louis C. Miall read a paper on "The Permian Rocks of the Neighbourhood of Leeds." He first described the base of the Permian System. The carboniferous rocks having been disturbed, thrown into anticlinals and faulted, were greatly denuded, and the Permian rocks were then deposited upon the new surface thus produced. The conditions of deposit of the magnesian limestone were then considered. The abundance of mineral salts, exclusive of carbonate of lime, the scantiness of animal life and the dwarfed state of the mollusca, all point to deposition in an inland sea or confined basin similar to the Caspian, Dead Sea, or Great Salt Lake of the present day. In parts of he Triassic period the previous marine surface appears to have become, in part at least, terrestrial or fresh water. At a much later period the Permian rocks, with others of subsequent formation, were denuded extensively, and reduced to the state in which they now occur. The Permian series of the neighbourhood of Leeds were then specially referred to. The Lower New Red Sandstone of South Yorkshire (the Pomfret Rock of Smith) does not appear to be present, at all events in a conspicuous state, in this district. The so-called Lower New Red Sandstone of Plumpton is undoubtedly of carboniferous age. The Upper and Lower Magnesian Limestone are well displayed. Various sections of these rocks at Rigton, East Keswick, Collingham, Whin Moor, and Knaresborough, were described in the paper. Remarks on the colour of the soil produced by underlying Permian rocks on the few fossils which have occurred at Garforth and Cold Hill, near Sherburn, and on the superficial drift, concluded the paper.

VIENNA

Imperial Academy of Sciences, April 24.—Dr. Wiesner presented a work on the influence of temperature on the development of *Penicillium glaucum*. Germination of spores takes place between 1·5° and 43° C.; development of mycelia between 2·5° and 40°; and formation of spores between 3° and 40°. These processes attain maxima of rapidity, the first and third at 22°, the second at 26°.—Dr. Hause gave a paper on the decrease of heat with the height in Asiatic monsoon countries. The decrease is less on the windy side than on the lee. The yearly average decrease is not less in the tropics than in central Europe.

May 8.—Dr. Thin presented a memoir on the structure of touch bodies.

May 15.—Dr. Boué read a paper on petrified bodies which have been forced from their place of deposition; and another on the formation of the dolomitic Alpine Breccias, as compared with some tertiary mountains in Lower Austria, which resemble them, but are quite distinct in origin.

May 23.—A communication from Prof. Horsford, of Cambridge, U.S., treated of the reduction of carbonic acid to carbonic oxide through phosphate of iron.—MM. Hlasiwetz and Habermann concluded their account of researches on protein-stuffs. They find the decomposition-products of casein to be, exclusively, these: glutamic acid, aspartic acid, leucin, tyrosin, and ammonia.—Dr. Heitzmann gave a paper on the relation between protoplasm and ground substance in animal bodies.

June 13.—Dr. Basch presented a note on the retardation of intestinal motion through the nervus splanchnicus.

June 19.—M. Fritsch presented the third part of his normal flower-calendar for Austro-Hungary.—Prof. Maley described researches made along with Dr. Donath on the chemistry of bones. One chief object was to ascertain whether the substance of bones is a combination of calcic phosphate with the lime-furnishing mass, in chemical sense, or whether it is not rather an intimate mechanical mixture of the two constituents. They adopt the latter view.—Prof. Topler described two applications of the principle of air friction to measuring instruments. A suspended magnet has, connected with it below, and in the same plane, a vertical plate, moving in a closed case, the vertical section of which it nearly fills. By inserting cross walls in the case, the motion of magnet and plate may be deadened by air friction; and that in proportion as the cross plates are pushed far in or not. The other application is for levelling purposes. The observer looks through a telescope at a little square mirror suspended by two threads in a glass case scarcely larger than it. The mirror moves as if in a viscous liquid.—Prof. Suess presented a memoir on the earthquakes of Lower Austria. Two lines of direction are distinguished.—Dr. Holetschek discussed the path of the first comet of 1871.—Dr. Heitzmann described experiments in which he had fed carnivorous animals with lactic acid, and also injected it subcutaneously; the result being arthritis and osteomalacia.

June 26.—Dr. Heitzmann read a paper on the life phases of protoplasm.

July 10.—M. Simony gave the principal results of a large theoretical work occupying him, in which a new molecular theory will be developed, requiring only one matter and one principle of force.—Dr. Böhm gave a note on the germination of seeds in pure oxygen gas. In such gas, of ordinary density, seeds did not get beyond the first stages; but, curiously, if the gas was diluted with ⅘ of its volume of hydrogen, or rarified to a pressure of 150 mm. they germinated as in air.—Dr. Heitzmann read a paper on the development of periosteum, bone, and cartilage.

July 17.—Dr. Böhm presented a note on the influence of carbonic acid on the verdure and growth of plants. In an atmosphere containing only 2 per cent. CO_2 the formation of chlorophyll was retarded; while 20 per cent. suppressed it entirely in most cases. The gas was also found prejudicial, in various degrees to the germination of seeds.—Dr. Sigmund Mayer described some experiments on direct electrical stimulation of the heart in mammalia.—Prof. Suess gave a paper on the formation of mountains in central Europe, and Dr. Heitzmann one on inflammation of periosteum, bone, and cartilage.

GÖTTINGEN

Royal Society of Sciences, June 14.—M. Waitz read a note on some lost Mayence Annals.—M. Benfey presented a philological paper on the suffixes *anti*, *âti*, and *ianti iâti*, in Sanscrit, Latin, and Greek; also a notice of some Mongolian and Cingalese legendary fragments; and sketched the design of a treatise on "eye-speech," pantomime, gestures, and modulations of the voice, phenomena which he urges travellers to make careful note of, and grammarians to study more than previously, as throwing light on the development of speech and languages.—M. Quincke described a new method of observing circle divisions in telescopic work.—Dr. Voss communicated mathematical notes on the simple transformation of plane curves, and the geometry of surfaces.—Dr. von Brunn described certain smooth muscular fibres found in the suprarenal bodies, accompanying the larger veins, and forming cylindrical or flat bundles.—M. Enneper presented a second note on orthogonal surfaces.—M. Bjerknes made some historical observations on Dirichlet's problem of a ball at rest in an agitated, unelastic, infinite liquid, and generalised some results previously obtained on the subject.—M. Klinkerfues made some remarks on the method of determining parallax by radiants; the results of this method, for Sirius, agree pretty closely with observation.—M. Lolling contributed a lengthy memoir on the topography of Athens. From local study, and the Greek authors, he seeks to determine the position and nature of the Pnyx, the Bema, the cave of Apollo in the Acropolis, and the Metroon. He is now prosecuting these inquiries further.

July 5.—M. Benfey made some remarks on the dual nominative "Samritadhrû" occurring in the Rigveda.—Fr. Wieseler gave a description of certain valuable specimens of early Grecian sculpture and other antiquities obtained in the East.—Dr. Riecke discussed Weber's fundamental law of reciprocal electric action in its application to the unitarian hypothesis, instead of the dualistic, which Weber adopted; and points out some differences these hypotheses involve in their results.—Dr. Voss read a paper on the geometry of Plücker's line forms.—Dr. von Brunn communicated a short note on ossification of cartilage.

PARIS

Academy of Sciences, Aug. 11.—M. de Quatrefages, president, in the chair.—The following papers were read:—A reply to M. Tacchini's new objections, by M. Faye. The author answered the observations and objections lately published by that observer, of whom he said that "the facts which he cites are in contradiction with the theories which he attributes to me, but not with those which I have really published."—On the Cyanides, by M. Berthelot.—On the re-solution of precipitates, by M. Berthelot.—On the palms of New Caledonia, by M. Ad. Brongniart.—On the carpellary theory applied to the *Ranunculaceæ*, by M. Trécul.—M. Elie de Beaumont furnished some further descriptive matter on the detailed geological map of France.—M. A. Ledieu read the fifth portion of his paper on thermo-dynamics.—On the movements of the tide on the coasts of France, change in the time of high water at Havre since the embankment of the Seine, by M. L. Gaussin.—On the passage of gases through colloidal vegetable membranes, by M. A. Barthélemy.—Note on the methods employed for the analysis of the natural phosphates employed in agriculture, by M. C. Mène. The author strongly advocated the use of the bismuth process, which, he says, never admits of a greater error than 0·25 per cent.—On a cave of the period of the reindeer, at Lortet, Hautes-Pyrénées, by M. E. Piette. The author announced the discovery beneath a deep layer of stalagmite, which covered reindeer remains, of a quantity of prehistoric human relics, and upwards of 500 cubic metres of ashes. The human relics include a drawing, on rein-deer horn, of a heath-cock.—Analytical solution of curve-traces of several centres by means of Perronet's geometrical process, by M. Revellat.—On fluorene, by M. Barbier. This is the name given to a hydrocarbon exhibiting great fluorescence, and occurring in coal-tar boiling between 300° and 340°.—On the action of platinum and palladium on the hydrocarbons, by M. Coquillion.—On the variations of hæmoglobin in various diseases, by M. Quinquand.

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ERRATA.—Vol. viii. p. 299, col. 2, at bottom, Equation (6), should read:—

$$2\left(\frac{1}{D_{12}}\right)^{\frac{1}{2}}\left(\frac{1}{w_1}+\frac{1}{w_2}\right)^{\frac{1}{2}} = \left(\frac{a_1}{\mu_1}\right)^{\frac{1}{2}}\left(\frac{2}{w_1}\right)^{\frac{1}{2}} + \left(\frac{a_2}{\mu_2}\right)^{\frac{1}{2}}\left(\frac{2}{w_2}\right)^{\frac{1}{2}} \qquad (6)$$

The calculations were not made by means of this equation, either in its right or wrong form, but from the values of k given in Table I.—J. CLERK-MAXWELL

P. 300, 1st col. equation (7) should be $l = \frac{1}{\sqrt{2}\pi s^2 N}$ &c.

P. 309, transfer top line of col. 1 to top line of col. 2, p. 308.

THURSDAY, AUGUST 28, 1873

THE REPORT OF THE SCIENCE COMMISSION ON THE OLD UNIVERSITIES

II.

In relation to the Colleges, the attention of the Commissioners has been principally directed to the following points:—1. The Scholarships. 2. The Fellowships. 3. The Organisation of the Instruction given in the Colleges in relation to the Instruction given in the Universities. 4. Contributions from the Colleges to a fund for University purposes.

After giving a list of the Scholarships filled up in Oxford from January to December 1872, it is remarked that "it is evident upon a comparison of the numbers contained in this list that the Scholarships offered for Natural Science are but a small fraction of the whole number. The state of the case appears to be that the Colleges do not offer Scholarships for Natural Science because they fear they would not get good candidates from the schools; and the schools do not teach Natural Science because they are afraid of injuring the prospects of their pupils by diminishing their chances of obtaining a Scholarship. It cannot be doubted that the effect upon the schools of this unequal distribution of rewards has been, and is, very discouraging to scientific study; and that it has exerted a most unfavourable influence upon the number of Natural Science students."

Without being prepared to concur in this estimate of the relative value of the two objects, we are nevertheless of opinion that it is of great importance, with the view of promoting the study of Natural Science in the first grade schools throughout the country, that there should be an immediate, and ultimately a large, increase in the number of Scholarships offered for this subject by the Colleges.

The part of the report which deals with the Fellowships is of great importance.

After quoting from the evidence of the Chancellor of the University of Oxford and others, evidence to the effect that the present application of the revenues to Fellowships is exceedingly unsatisfactory, the report proceeds:—

"Whilst giving every weight to the considerations urged by Prof. Jowett, and admitting to the fullest extent the great stimulus which the higher education has received at Oxford from the system of election to Fellowships by open competition, we are nevertheless satisfied by the evidence laid before us that an unduly large proportion of the revenues of the Colleges is expended in sinecure Fellowships; and we have reason to believe that this opinion is shared by a large and increasing number of the resident members of both Universities. . . .

"It is doubtless advantageous to the country at large, as has been urged by some of our witnesses, that young men of ability, who choose to enter into one of the great professions, should be supported, or nearly so, in the early years of their professional career, and thereby be enabled to apply themselves at once to the higher studies of their profession, instead of wasting their energies in drudgery of some kind, for the mere purpose of obtaining a temporary livelihood. But this end may be secured by means of Fellowships tenable only for a limited period. It has been urged that the feeling of security given by the system of unlimited tenure greatly enhances the value of a Fellowship. No doubt it is a very comfortable thing for a young man to feel that, come what may, he is secure of an income so long as he chooses to remain single. But we can see no adequate reason why he should be thus comforted at the expense of the College, when he has preferred the more attractive prospect of a professional career in the outer world to the work of the College. . . .

"We are therefore decidedly of opinion that the Fellowships awarded as prizes are excessive in number if not in value, and that the system ought to be remodelled. We are further of opinion that in any such remodelling a considerable proportion of the Fellowships should be suppressed or consolidated for the purposes of contributing to the general fund of the University and of endowing, within the Colleges and the University, new institutions, new offices, in aid of education or research. But it must be remembered that, as Prof. Jowett has stated, the property of the Colleges at Oxford, in some instances at least is greatly increasing, so that quite independently of the suppression of Fellowships there will in all probability be considerable sums available for these purposes. In any case, therefore, we are prepared to admit that a great part of the Fellowships ought to be retained as Fellowships, and the problem that has to be solved is how to employ those which are so retained in the most useful manner possible.

"The following are the chief purposes to which, in our judgment, the Fellowships should be applied:—

"In the first place, a certain but not a very large proportion of the Fellowships will be always required, as at present, for the payment of the persons entrusted with the management of the College estates, and with the government and administration of the Colleges themselves.

"Secondly, a large number of the Fellowships is at present employed, and probably a still larger number ought hereafter to be employed, in connection with the instruction given in the Colleges.

"Thirdly, a smaller, but still a considerable number of Fellowships ought to be employed as Terminable Prize Fellowships.

"Fourthly, a certain number of Fellowships ought, as we have already said, to be united with Professorships in the University; the University professor becoming *ex officio* a Fellow of the College and a member of its governing body.

"Lastly, it is, in our opinion, most important that a certain number of Fellowships should be appropriated to the direct promotion of learning and research in various directions. It has been objected to this proposal that the Fellowship system, as hitherto administered, has not shown any great tendency to encourage original research, either in the field of learning or in that of Science; that, when an office is created simply and solely with the view of giving a man leisure and opportunity for original research, there is always the appearance, to say the least, of creating a sinecure; and that it is impossible, as Prof. Jowett has said, to get a man for money who can make a discovery. But, though you cannot get a man for money to make a discovery, you may enable a man who has shown a special capacity for research to exert his powers; and we are of opinion that, unless an effort is made to do this, one of the great purposes for which learned bodies, such as the Colleges, exist, may run the risk of being wholly lost sight of. Scientific discoveries rarely bring any direct profit to their authors, nor is it desirable that original investigation should be undertaken with a view to immediate pecuniary results. 'Research,' as Lord Salisbury has observed, is 'unremunerative; it is highly desirable for the community that it be pursued, and, therefore, the community must be content that funds should be set aside to be given, without any immediate and calculable return in work, to those by whom the research is to be pursued.'

"It may be that properly qualified candidates for such scientific offices would not at first be numerous, but we believe that eventually a considerable number of Fellowships might be advantageously devoted to the encouragement of original research.

"We think that such Fellowships as might be expressly destined for the advancement of Science and Learning should only be conferred on men who by their successful labours have already given proof of their earnest desire, and of their ability, to promote knowledge; and we believe that appointments, made with a due regard to this principle, would be abundantly justified by results. A man who has once acquired the habit of original scientific work, is very unlikely ever to lose it, excepting through a total failure of his health and strength; and even if it occasionally happened that a Fellowship awarded on the grounds of merit, as shown in original research, should only contribute to the comfort of the declining years of an eminent man of science, there are many persons who would feel that it could not have been better expended in any other way.

"We should not wish to attach any educational duties properly so called to a Fellowship awarded with a view of encouraging original research in Science. But for many reasons we should think it desirable that the holder of such a Fellowship should be expected to give an account, from time to time, in the form of public discourses, of the most recent researches in his own department of Science."

The last section of the Report dealing with the duty of the Universities and Colleges with regard to the advancement of Science is so important that we give it at length:—

"*Research a primary Duty of the Universities*

"On no point are the witnesses whom we have examined more united than they are in the expression of the feeling that it is a primary duty of the Universities to assist in the Advancement of Learning and Science, and not to be content with the position of merely educational bodies. We entirely concur with the impression thus conveyed to us by the evidence, and we are of opinion that the subject is one to which it is impossible to call attention too strongly. We think that if the Universities should fail to recognise the duty of promoting original research, they would be in danger of ceasing to be centres of intellectual activity, and a means of advancing Science would be lost sight of which, in this country, would not easily be supplied in any other way. There is no doubt that at the present time there is a very strong feeling in the country in favour of the wide diffusion of education, and of the improvement of all arrangements and appliances which tend to promote it, from the simplest forms of primary instruction up to the most advanced teaching that can be given in an University. But there is some reason to believe that the preservation and increase of knowledge are objects which are not as generally appreciated by the public, and of which the importance is not so widely felt as it should be. On this point we would direct especial attention to the remarks of Sir Benjamin Brodie: 'For education we construct an elaborate and costly machinery, and are willing, for this end, to make sacrifices: but, on the other hand, the far more difficult task of extending knowledge is left to the care of individuals, to be accomplished as it may; and yet it is this alone which renders education itself possible. I really am inclined to think that in former days a more real and earnest desire must have existed to preserve knowledge as a valuable national commodity for its own sake than exists now; and the reason that I say this is, that we have existing in the Universities of Oxford and Cambridge records of another condition of things with regard to knowledge than that which exists at present. In the first place we have extensive libraries which could only have been founded and preserved for the sake of the preservation of knowledge itself; and in the next place the collegiate foundations in the Universities were originally and fundamentally, although not absolutely and entirely, destined for the same objects. . . . This object is certainly not less important in modern than in ancient society. I presume that in the middle ages knowledge would altogether have perished if it had not been for such foundations, and it appears that now from other causes the pursuit of knowledge and of general scientific investigation is subject to very real dangers, though of another kind to those which then prevailed, and which make it very desirable for us to preserve any institutions through which scientific discovery and the investigation of truth may be promoted. . The dangers to which I refer are dangers which arise partly even from the growing perception of the practical importance of knowledge, which causes a very great draught indeed to be made upon the scientific intelligence of the country. In the first place, almost every scientific man is caught up instantly for educational purposes, for the object of teaching alone; and, in the next place, a very great draught indeed is made upon Science for economical purposes; I mean for purposes connected with practical life. In sanitary matters we have numerous examples of the vast amount of work done by scientific men for public and practical objects. So that the supply of scientific men is not equal to the demand for those objects alone. Manufactures offer another great field of scientific employment, and it is to be observed that these are the only ways through which an income can be obtained, the pursuit of scientific truth being an absolutely unremunerative occupation.'

"We believe that the dangers referred to in these remarks are real; and their existence induces us to lay down, as emphatically as possible, the position that the promotion of original work in Science should be regarded as one of the main functions of the Universities, and should be specially incumbent upon the holders of those fellowships which, as we have already recommended, should be awarded with a view to encouraging original research. As regards the professors, we have already insisted on the importance of so arranging their duties as to give them abundant leisure, and, what is no less indispensable, abundant opportunities for original investigation, by providing the external appliances necessary for it. We think that the great national interests connected with the advancement of Science form one, although only one, of the grounds upon which the endowment of professorial offices is defensible, and regard it as a great advantage that an opportunity is afforded by the peculiar circumstances of the Universities of giving encouragement and maintenance to a class of persons who are competent to advance Science, and who are willing to make its advancement the principal business of their lives.

"We have already stated, but we would repeat it here, that we would on no account have offices founded within the Universities without special duties attached to them. It is an absolute advantage, if not in all, at least in many cases, to a man who is engaged in some abstract part of Science, to be compelled to produce, in the form of public discourses, the results of his labours; and it can be no disadvantage to him, under any circumstances, to be obliged to devote some moderate part of his time to showing, if it were only by the example of his own work, to younger men, how scientific studies should be carried on with the view of promoting human knowledge. We believe that in all ordinary cases a certain amount of educational work is of advantage to the scientific worker, and we also believe that for the promotion of the highest scientific education it is very desirable to bring the original worker into direct personal contact with the student.

"We have also already spoken of the propriety of awarding Fellowships in certain instances, not, as at present, by an examination test, but for services rendered to Science in Original Research. Although we should wish, as we have already said, to see this done from time to time (as it has already been done at Cambridge) in the case of persons who have already made themselves eminent in Science, and whose accepting the Fellowship is rather to confer an honour upon the office than to receive one from it, we also think that a wider application should be given to this principle; and, that whenever a Fellowship in Natural Science is offered for competition among the younger Graduates of the University, such evidence as any candidate can offer of his aptitude to become an useful worker in Science, should always be taken into account in the award. Nothing, we believe, would tend to give the students at the Universities so just an idea of what Science is, or of what the objects are which those who pursue it should have in view, as the adoption of the principle by the Universities and the Colleges, that the highest honours and rewards in Natural Science are to be conferred upon men who can offer some evidence that their names are likely afterwards to find a place on the list of those who have added to human knowledge.

"The proposals to which we attach the most importance with a view to the encouragement of Original Research at the Universities are the two to which we have just referred: (1) the establishment of a complete Scientific Professoriate; (2) the appropriation, under certain conditions, of Fellowships to the maintenance of persons engaged in Original Research. But, in addition to these main proposals, other suggestions are contained in the evidence before us, to which we would call especial attention: (1) that Laboratories should be founded expressly intended for Research, and for the Training of Advanced Students in the methods of research; (2) that Scientific Museums and Collections should be maintained to an extent beyond what is required for purely educational purposes; (3) that a Doctorate in Science should be instituted.

"*Proposed Laboratories for Research*

"It is one of the disadvantages of an University course that a young man, up to the time of taking his degree, is straining every nerve in order to master a certain amount of knowledge in which he has to pass an examination; and however improving this process may be to him in certain respects, the impression is widely entertained that it is not caculated to develope the originality of his mind, or those peculiar qualities which fit a man to become a discoverer in Science. As it is indispensably necessary that the student should be well grounded in his work, and should have a thorough comprehension of the methods and principles of his branch of Science, before he attempts to add to it, it is not easy to see how this disadvantage could be remedied during his undergraduate course; but as soon as his examinations are passed, it is surely time that he should be led to regard his studies from another point of view, and to give them a different direction. He should then be placed in a laboratory devoted to original research, and under the immediate care of persons who are principally engaged in work of that nature.

"On this point we would again refer to the evidence of Sir Benjamin Brodie: 'I should like (speaking of my own department and departments which are cognate with it, and I have no doubt that the same remark would also apply to Physiology and to other subjects) to see those professors have under their control laboratories suited for scientific research and investigation, in which they should take a certain limited number of students who would work, partly as their pupils and partly as their assistants, for those ends. And I should myself say that this is an educational function of the most important character possible, because you would here really carry scientific education to its end. If you do not do this you stop short of the most important part of all in scientific education. Now the real perfection of Science is shown only in scientific inquiry—the perfection of Science not only in its general results, but the perfection of Science as an instrument for education; and if you leave out in the University system any provision for scientific research, you are leaving out the most important feature of the subject. Those pupils would be persons who would ultimately pursue the science as their main business in life, and become in their turn the teachers and the professors of the subject. I am not giving a mere chimera or dream, but this is already, though not exactly in the way that I am suggesting, carried out to a great extent in Germany.'

"No less important, as giving one view of this question, is the evidence which we have received from Dr. Frankland, who says, 'In my opinion the cause of this slow progress of original research (in England) depends, in the first place, upon the want of suitable buildings for conducting the necessary experiments connected with research; secondly upon the want of funds to defray the expenses of those inquiries, these expenses being sometimes very considerable; but, thirdly and chiefly, I believe that the cause lies in the entire non-recognition of original research by any of our Universities. Even the University of London, which has been foremost in advancing instruction in experimental Science, gives its highest degree in Science without requiring any proof that the candidate possesses the faculty of original research, or is competent to extend the boundaries of the science in which he graduates. I consider that this circumstance is the one which chiefly affects the progress of research in this country, because if we inquire into the origin of those numerous Memoirs upon original investigations that come from Germany, we find that a considerable proportion of them are investigations made by men who are going in for their Science degrees, and who are compelled, in the first instance, to make those investigations, and they attain by that means the faculty and liking for original research, and frequently follow it out afterwards; so that a considerable proportion of the papers themselves are contributed in the first place by those men going in for degrees, and a considerable proportion of the remainder are obtained, I believe, through the influence of this previous training in research upon the men who have taken the degrees. Further, the entire ignoring of research in the giving of degrees in this country diverts also, or has a tendency to divert, the attention of the professors and teachers in this country from original research. They have not to take it into their consideration in the training of their students; they have not to devise, as is the case in Germany, suitable subjects for research to be pursued by their students; and thus their attention is, as it were, taken away entirely from this highest field of Science. And, indeed, if they themselves devote some of their time to original research, it almost appears to them to be a neglect of their class duties because their class duties do not require it. Their students are to be trained for subjects which are foreign to original research; they are to be trained chiefly in subjects that are to be taught by lectures, and by what I should call "descriptive," as distinguished from "experimental" or "practical" teaching; and, consequently, I think that in both ways—both by not bringing students into contact with original experimental work, and by diverting the attention of the teachers and professors in this country from such work, great damage is done to the progress of investigation in Great Britain by the attitude of our Universities.'

"Sir William Thomson has gone even further, and has expressed an opinion that the systems of examination in the Universities, as at present arranged, so far from doing anything to encourage the spirit of scientific research, have an exactly opposite tendency. 'That, to some degree, competitive examinations produce an elementary smattering of Science I have no doubt whatever, but I cannot see that they produce much beneficial influence; and in the higher parts especially, they have, I fear, a very fatally injurious tendency in obstructing the progress of Science.'

"The kind of assistance which we should desire to see given in the English Universities to young men who have completed their university course, and who propose to adopt a scientific career, has been from time to time afforded at various institutions in the United Kingdom, among which we may particularly mention the Laboratory of the University of Glasgow, under the direction of Sir W. Thomson. The plan has been adopted in some of the German Universities, and even in the great Polytechnic Schools of that country. In France it has recently been organised on a most complete and extensive scale. The École Pratique des Hautes Études is a Government Institution of which the object is to encourage young men to devote themselves to scientific research, and to give them opportunities of learning its methods. The course pursued by this institution is to take young men who have completed their preliminary scientific studies, and, allowing them an annual stipend to defray the expenses of their maintenance, to place them under the care of competent professors, who give them assistance and advice in their first researches, and to whom they afterwards become useful. This plan appears to us so excellent in itself, and at the same time so academic in its general character, that we desire to recommend it for adoption at Oxford and Cambridge. To insure due attention to both classes of students, it would be proper that the laboratories intended for training in the methods of research should be distinct from those in which more elementary instruction is given.

"We are also of opinion that arrangements should be made in some of the public buildings of the Universities, for giving opportunities to members of the Universities, no longer *in statu pupillari*, of prosecuting researches; although we should regard it of primary importance that these arrangements should be such as not to interfere with the teaching duties, or with the scientific work, of the professors. We agree with Dr. Frankland that one 'cause of the slow progress of original research' in England is 'the want of suitable buildings for conducting the necessary experiments connected with research;' and we think that the Universities might, with great propriety, supply this want, so far as their own members are concerned. We also think that collections of apparatus should be formed, which would be available for the use of such independent workers in Science. There are some obvious difficulties involved in this plan, which has been strongly recommended by some of our witnesses, but which, so far as we are aware, has not been anywhere practically tried. We should, however, look with confidence to such a body as the proposed 'University Council of Science' to frame suitable regulations as to the fitness of the persons admitted to the privilege of working in an University laboratory, and as to the securities to be taken for proper care in the use of valuable instruments. We are disposed to think that, under the special circumstances of the Universities, they would do more to promote original work by offering facilities of the kind which we have described than by making grants of money similar to those which are made in aid of special researches by the Government Grant Committee of the Royal Society. The plan would have the collateral advantage of rendering residence at the Universities attractive to scientific men.

"Proposed Special Scientific Collections

"Although we think it desirable that Scientific Museums and Collections should be maintained in the Universities to an extent which would render them available for original research, as well as for the purposes of education, we do not attach the same importance to this point as to the preceding, because museums and collections have been formed and will be formed in other places than in Universities, whereas laboratories adapted for the instruction of students in the methods of scientific investigation are not likely to be founded except in connection with educational institutions; and although it is a disadvantage to a scientific man not to have all the collections that he desires immediately at his hand, yet, considering the proximity of the Universities to London, it cannot be said that this disadvantage amounts to more than an inconvenience.

"We also are of opinion that it is very desirable that such more extensive collections as may be formed in the Universities should, as far as possible, be kept separate from the more limited collections intended for educational purposes. A Museum may be very easily made too large for these purposes, and instead of giving the student clearer ideas, may serve to confuse him.

"Proposed Doctorate in Science

"We have already referred to the possibility of instituting Higher Degrees, to be conferred upon students, not in accordance with the results of an examination, but upon their giving proof of capacity for original research. The evidence of Dr. Frankland and of Sir William Thomson, which we have already quoted, and to which we might add that of the late Prof. Rankine, appears to us conclusive upon the point that there is a real danger in the examination system; and in our opinion this danger might be guarded against by instituting a higher degree in Science, the obtaining of which should be regarded as a great honour, and which should not be awarded except with reference to original work. The plan of requiring from a candidate for the Doctorate in Science a dissertation embodying an account of some original research of his own is strongly approved by such competent witnesses as Dr. Siemens, Dr. Carpenter, and Prof. Frankland. This plan has been adopted in several of the German Universities, and has now become the established rule in France."

METEOROLOGICAL CONFERENCE AT LEIPSIG DURING AUGUST 1872[*]

OF the Congresses which have recently been held, none were more urgently called for than an International Congress of Meteorologists. Doubtless even under the diverse systems of observation which have been in use at national observatories and among meteorologists of different countries, large and valuable contributions have been made to Climatology and other departments of Meteorology. We need only refer to the various charts which have been published, showing the geographical distribution of atmospheric and oceanic temperature, of atmospheric pressure, of humidity, of prevailing winds, and of rainfall, and to the enormous amount of materials now being amassed, illustrative of the nature and course of storms, to show the important results which have been obtained. It must, however, be confessed that, as respects nearly the whole of this information, it can be regarded as valuable only in the sense of its being sufficiently approximate so as to meet the requirements of some of the more pressing practical questions of the science, and not because it is precise.

It is when we attempt inquiries into such questions as the diurnal and annual march of the different meteorological elements, and the relations of these elements *inter se*, and of weather on a large scale, that the general unsatisfactoriness of the systems by which observations are made in different countries comes to be forcibly felt, owing to their want of precision and uniformity. The want of uniformity is most conspicuous as respects temperature, humidity, and wind—or just those fundamental facts which must be scientifically observed and discussed before we can hope to solve the problem of weather changes.

In order to bring about a greater uniformity of procedure in different countries, it was proposed to hold a Meteorological Congress at Vienna in 1874. In June last, Bruhns of Leipsig, Wild of St. Petersburg, and Jelinek of Vienna, issued an invitation to meteorologists to attend a preliminary conference to be held at Leipsig in August, for the purpose of preparing the programme for the Vienna Congress, to instigate preparatory experiments on some of the more important questions, and thereby render it possible for the Congress to arrive at immediate conclusions on many points. The Conference was thus only consultative. Accompanying the invitation were a series of twenty-six questions, which it was proposed to submit to the consideration of the Conference.

Upwards of fifty persons attended the meetings of the Conference, which lasted three days. The opinions of the different speakers on the points raised by the 26 questions are detailed in the pamphlet before us, which contains also the written opinions of 14 meteorologists who were unable to be present, including the well-known names of Dove, Ribenson, Mohn, Mühry, and Wolf, as well as the results of the deliberations of the French meteorologists at Bordeaux in September. The subjects treated of may be conveniently classed under the heads of instruments, their position, the methods of discussing, publishing, and utilising the observations.

Barometers.—To those who have attempted to discuss weather, it is well known that nothing exact or satisfactory need be looked for in the result, unless observations from numerous barometers well distributed be available. It is thus desirable that barometers be procurable at a moderate price for stations of the second order. Are Board of Trade barometers—barometers fitted with a float—or aneroids, suited for such stations; or is there any other cheap form of barometer that would serve the purpose? After a lengthened discussion it was referred to Dr. Hann of Vienna to prepare a report for the Vienna Congress. The most diverse opinions were expressed regarding the aneroid, arising probably from the experience of the different writers and speakers—some aneroids going well for years with no permanent alterations occurring in their indications; some going well so long as a small range of pressure is recorded, but undergoing alterations after every great barometrical depression; some constantly altering in one direction, others in either direction, &c. Since, however, it can be safely affirmed of no aneroid, how good soever it may have proved itself to have been, that it will continue to indicate correctly for even a brief time to come, the Conference came to the sound conclusion that the aneroid should not be used instead of the mercurial barometer, but only as an interpolation instrument, to fill up blank when the mercurial barometer is out of order, or when it cannot be observed on board ships in rough weather.

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[*] Report of the Proceedings of the Meteorological Conference at Leipsig. Protocols and Appendices Translated from the Official Report, Appendix to Vol vii of the "Zeitschrift für Meteorologie" Published by the authority of the Meteorological Committee. London, 1873.

Maximum and Minimum Thermometers.—Rutherford's minimum spirit thermometer was regarded as satisfactory. On it being pointed out by several members that this thermometer is liable to go out of order by the spirit evaporating and condensing in the upper end of the tube, Ebermeyer, of Aschaffenburg, stated that this objection could be removed if the tube were at its entrance into the bulb inserted nearly up to the inner side of the bulb. We commend this suggestion to opticians; for if Ebermeyer's experience be confirmed, a source of serious and not infrequent error will be removed. On the other hand the performance of no *maximum thermometer* was considered to be so satisfactory that a uniform construction could be generally recommended; and the opinion was expressed that it was very desirable that a trustworthy maximum thermometer was devised, not liable for instance to have the mercury disturbed during high winds like Negretti and Zambra's, or the index portion go out of order as Rutherford's or Phillip's.

Instruments for Radiation.—Mr. Symons, who has paid much attention to this question, has been requested to give a report to next Congress on the modes of observation adopted in England for radiation. But it must be confessed that the methods of observation in this important inquiry are still in a very primitive state. Mr. Salt well pointed out that at present the results obtained with different instruments were not comparable with each other, and one hardly knew with the instruments now in use what was really observed.

Hygrometers.—Since the dry and wet bulb hygrometer is not trustworthy at low temperatures and in cases of extreme dryness, and the hair hygrometer fails also at the dew points, and since there is no hygrometer yet devised, at least for regular observations at stations, which gives approximately exact results as to moisture in all cases, it was recommended to make further experiments and collect the experience of meteorologists on the subject. From the favourable opinions expressed by Wild and others of the action of the hair hygrometer, further experiments with this instrument are very desirable, so that it might be made available for more accurate observations on the hygrometry of the air at temperatures below the freezing-point than the dry-and-wet hygrometer admits of. Another desideratum is an extensive series of experiments with Regnault's hygrometer in conjunction with the dry-and-wet bulb hygrometer in dry hot climates such as N.W. India, for the purpose of ascertaining how far the readings of the dry-and-wet bulbs can be used as data from which the dew-point may become known; and determining the requisite data for the correction and completion of the present hygrometric tables, particularly at points below freezing, and at high temperatures combined with great dryness.

Wind.—Curiously enough, the question of proper instruments for measuring the velocity and force of the winds does not seem to have been under discussion, even though it is one of the most important and pressing questions of the science. Anemometers, both for velocity and pressure, are indispensable to properly equipped observatories. Now it cannot yet be said that the anemometers for velocity give quite correct indications that they are comparable, *inter se*, and that we have a practicable means of ascertaining their errors from time to time.

Equally remarkable was the omission in the discussions, to consider what are the required conditions which anemometrical stations ought to fulfil, so that the instrument shall indicate the true movement of the air over the region where it is placed; or, if this cannot be accomplished, what observations should be instituted in order to ascertain how far the direction of the wind is deflected by the physical configuration of the surface, and its force diminished (or in rare cases accelerated) as compared with the general movement of the air over the place.

Pressure anemometers at a moderate cost are a great desideratum. Little satisfactory is known of the relation of pressure to velocity.

Rain.—The Committee proposed that a report of all the experience regarding the position, size, height above ground, and time of reading the rain gauge which has been yet gained should be prepared for the Vienna Congress. For the preparation of such a report the great storehouse of facts at hand are those collected by Mr. Symons in the successive parts of his "British Rainfall" and "Meteorological Magazine," which the members of the Congress would do well to consult.

Evapometer.—The present state of the evapometer is one of the least satisfactory of all the meteorological instruments. Considering the importance of the drying property of the air in relation to meteorology generally, but especially as one of the most important constituents of climate, it is to be hoped that some method will be devised by which results, at least roughly comparable to begin with, may be obtained.

The difficult, but vital question of the position of the thermometer does not seem to have been faced by the conference. It is earnestly hoped that the Vienna Congress will not shirk this question, but will seriously discuss it and arrive at some decision, or suggest some steps to be taken, that will ultimately lead to the degree of uniformity which is so imperatively called for. Till this be secured, the expensive systems of horary or continuous registration of temperature carried on at the great observatories of this and other countries, cannot supply the data for the determination of temperature "constants," seeing that they are incomparable with each other, as well as with the observations made at those numerous stations of the secondary order to which we must look for the working out of the great national question of local climates in their bearing on the health, productions, and commerce of the country. The question would be of comparatively easy solution were it possible, in the interests of cosmical inquiries, to ignore the past. But it is essential in the case of the older observatories to adhere to the same system of observing hitherto in use; until at least four or five years' observations have been made simultaneously with a second set of instruments placed in uniformity with those of other observatories.

The question of the practicability and utility of Weather and Storm Signals in Europe was considered, and it was remitted to Messrs. Buys Ballot, Scott, and Neumeyer, to collect the opinions of meteorologists on this important question, and draw up a report for the Vienna Congress. As it is understood that the committee have collected a good deal of information, some valuable results may be expected.

In the "Sequel to the Suggestions," Dr. Buys Ballot has suggested for the consideration of the Congress, the establishment, by societies, of stations in regions which are at present a blank. The Smithsonian Institution, the Dutch Meteorological Institute, and, in our country, the Scottish Meteorological Society have, with the means at their disposal, done a good deal in this direction, with results which have aided much in the furtherance of the science. But to fill up the enormous blanks which still disgrace British America, South America, most of Africa, and the Pacific, some concerted action on the part of meteorologists is indispensable. In connection with this proposed development, reference may be made to the scheme in contemplation by the Chinese Government, in carrying out which Mr. Campbell has been sent to this country to request advice from scientific authorities as to the general organisation of the stations, and to procure the necessary instruments, registers, &c. Towards the carrying out of this plan, the Congress will doubtless give Mr. Campbell very hearty support.

THE TYPHOID EPIDEMIC IN LONDON

THE recent outbreak of enteric fever in the West End of London presents many points of remarkable interest and teaches many useful lessons. Typhoid, Enteric, or Pythogenic fever, although a disease about which all our accurate knowledge is quite recent, is a fever about the causes of which we really know a great deal, but which, for all that, seems to appear from time to time in the places where it might be least expected.

About the nature of the poison which produces it we know as yet but little; we know that its habitat is in the refuse matters excreted from human intestines; we know that it is, under certain circumstances, developed in such excretal matters during their decomposition, but it is yet a moot point whether it is from time to time produced *de novo* under suitable conditions, or whether it is always necessary that some of the poison, however small a quantity, be introduced from without to cause such decomposing matters to become infectious. We are accustomed to regard this as the least specific of the diseases of its kind, but each outbreak which is traced to its source gives a rude shock to such ideas. The "filth-born" fever *par excellence*, it ought not, one would think, to need to wait to be introduced to the country places where, year after year for centuries, the shallow wells from which drinking water is obtained are, in effect, the drains of the premises; or to the town houses, in which the only ventilator to the sewer is the waste pipe which opens directly over the surface of the water in the cistern; but yet such is the case so universally, that when we cannot find out how the poison has been introduced, we should acknowledge our inability to do so, and not cut the knot by saying that it has originated on the spot, a conclusion for which, in the present state of our knowledge, we have no real proof whatever. The number of instances in which epidemics have been traced to single imported cases is now so great that, although it does not actually prove that such is always the case, still it should make us hesitate before declaring that the disease has broken out without direct importation in any given place.

The facts relating to the epidemic which still engages general attention in England, are, in order of sequence, and independently of any theory at all, as follows:—

The disease was noticed to be prevalent, in the middle and latter part of July, in certain houses in the parish of Marylebone, and notably in houses inhabited by medical men, houses where every possible precaution was believed to have been taken: it was observed by Dr. Murchison that an undue proportion of the persons attacked obtained their milk from a particular dairy, and on further investigation the conviction grew upon him that this milk was, somehow or other, contaminated with typhoid poison, and was spreading the disease. A difficulty arose, inasmuch as the locality in which the fever cases were was only a small part of the district supplied with milk from the suspected dairy; but Mr. Radcliffe, on examining the mode of distribution of the milk, showed that on the hypothesis that the milk from one of the several farms was contaminated before coming to the dairy, a localised outbreak or several localised outbreaks of fever must have been the result; so that any suspicion which may have existed as to the cause being possibly to be found in the precincts of the dairy in London, vanished at once.

On the other hand it was found that the owner of one of the dairy-farms had died on June 8; that he had been out of sorts since early in May, and sufficiently so for his two medical men to consult with a third on the subject; that the medical men all suspected that he had enteric fever; that this suspicion became stronger when the patient passed a large quantity of blood and putrid matter on June 1, which blood, &c., was ordered to be buried away from the house, as being most probably infectious; that the patient became considerably better towards the end of the first week of June, but that he died suddenly on June 8 while getting out of bed, no medical man being present; and finally that the medical attendant not being sure of the diagnosis of enteric fever, and considering that, anyhow, the man had got over it, certified that he died from heart disease, as he had for years been suffering from the effects of a "fatty heart;" nevertheless he took the precaution to have the body buried as speedily as possible, thinking that it might be infectious.

Taking all the facts together, these two series of events present at any rate a most remarkable coincidence; and when we find that enteric fever is and has for some months been prevalent in the villages near the farm and in daily communication with it, and that a son of the farmer has since had the disease, the conclusion is irresistible that the farmer died of enteric fever, and that he had it at a time most singularly adapted to account for the outbreak in London.

The description of the farm-yard itself has been given elsewhere; suffice it to say that the well really drained the premises, and there is little doubt but that the poison got into the water, which was so bad that it had long been condemned as unfit to drink.

Hitherto epidemics of typhoid spread by means of milk have been attributed to the admixture of water as an adulteration with it; in this case no such suspicion arises, the milk was exceptionally rich, and was daily tested with sufficient accuracy to show adulteration with any but a small amount of water; but the water from the well was conveyed to the dairy pump by a pipe, and was used for washing the dairy utensils, so that it is easy to account for the presence of a small amount in some of the "churns," an amount, however, enough in so favourable a pabulum as milk to infect a very large quantity of it.

The lesson to be drawn is that all dairy-farms must be subject to regular sanitary supervision, especially as to their water supply, that such details of arrangement with regard to the cleansing of the vessels as may seem to offer least chance of the possibility of mischief should be adopted, and that the presence of infectious disease among the *employés* should be noted at once, and the proper precautions, which are now well known, taken.

W. H. CORFIELD

DOLMEN-MOUNDS *v.* FREE-STANDING AND TRIPOD CROMLECHS

MR. W. COPELAND BORLASE, the talented author of "Nænia Cornubiæ," in his communication to NATURE (vol. viii. p. 202), calls attention to the structure of Lanyon Quoit as an undeniable example of a British tripod cromlech or free-standing dolmen, by way of "protest against the *dictum* of Mr. Lukis being extended to our British examples, before a careful scrutiny has been made of every monument of the kind, from one corner of our isles to the other."

To my friend Mr. Borlase I owe my personal acquaintance with the numerous non-historic rude stone monuments in the Land's End district; and, as he is a life-long resident in the immediate vicinity of these interesting relics, to which I am a mere casual visitor, it is with feelings of great delicacy and diffidence that I now venture to place in a somewhat different aspect the statements and conclusions which he would wish your readers to adopt.

It were strange if Mr. Borlase did not turn out the best authority on early Cornish remains, for within six or seven miles of his residence at Castle Horneck (itself the site of an ancient Cornu-British encampment) there are at least twice as many dolmens as in all the rest of England; and though there may be perhaps as many in Anglesea, and twice as many in Wales, still West Cornwall has an advantage over both these districts, viz., that in Wales and Anglesea, the country of the Silures, there are no circles but only dolmens; in Cornwall, as in the Isle of Man, there are both circles and dolmens, the result, as Fergusson tells us, of an Ibero-Aquitanian admixture with Celtic and other (Scandinavian?) blood in the inhabitants. (*Vide* "Rude Stone Monuments," p. 163.)

Inheriting the tastes and following in the footsteps of his great-grandfather of antiquarian renown, Mr. Borlase has made great use of his opportunities, and is continually adding to, or accumulating store of facts with regard to the ancient history of our country. On the other hand, most antiquarians will probably agree with me in maintaining that the Lukis family may be reckoned some of the best, if not the very best authorities, on the chambered barrows of France and the Channel Islands. Enormous numbers of these structures have been scientifically examined and exhaustively described by the Messrs. Lukis: and the Rev. W. Lukis, in company with Sir Henry Dryden, is now employed in drawing to scale plans and elevations of the Isle of Man remains, and thereby carrying out his share of that scrutiny which Mr. Borlase anxiously demands in his letter.

When such authorities disagree, it would seem almost impertinent to interfere; but knowing my friend Mr. W. Lukis to be busily engaged in the Isle of Man, and too far off to personally examine the monument in dispute, whilst I was within a three hours' journey of the structure I determined to see the cromlech myself, and having done so, cannot allow Mr. Borlase's letter to remain unchallenged.

In taking up the cudgels for Mr. Fergusson, Mr. Borlase must not be looked upon as an implicit follower of that author, whose work he characterises as "*unreliable*,"* although, with him he is convinced "that the barrows and the cromlechs (if not the circles too) were the sepulchres of the dwellers in the hut circles and the earthworks; and that these latter were the residences of the Romanised Britons in the earlier centuries of the Christian era;" for before the appearance of "Rude Stone Monuments," he struck out for himself the formation of "a small class or species of dolmen," viz. the tripod cromlech, or dolmen proper (see "Nænia Cornubiæ," p. 14, *et seq.*), "where, as Col. Forbes Leslie remarks, 'the vertical supporters of the tabular stone are columnar,' and cannot be said to enclose a space."

Before proceeding, it may be as well to remark what Mr. Borlase ignores, viz. that (as may be seen from the title to his paper) the criticism of Mr. Lukis (deserved, if severe) of "Rude Stone Monuments," was based upon the application of the "*Free-standing*" theory, by the author, to the monuments of France, where he *proved* it was inapplicable. He said nothing at Somerset House about English monuments, although I believe it is his intention to say something about them on a future occasion. Mr. Borlase severely attacks Mr. Lukis, as though, in removing the *French* monuments from the supposed "free-standing class, he condemned all persons who held their own views on *British* ones. Mr. Lukis' views are not "hypotheses." He simply declares that the plans of *French* monuments which he produced before the Society of Antiquaries in London teach the proposition he laid down, and that it is the duty of those who are unacquainted with these examples to verify or disprove his statements and descriptions by visiting and inspecting them, and not to try and write him down when they have a very imperfect knowledge of them, or none at all. Previous to taking stock of Mr. Borlase's weighty evidence in support of Lanyon Quoit as originally a dolmen proper, *i.e.* a tripod cromlech, it should be noted what Fergusson states in respect to the West of England dolmens. In "Rude Stone Monuments," p. 163, he says: "*Even a cursory examination of these West Coast dolmens would, I think, be sufficient to prove to any one that the theory that all were originally covered with earthen mounds is utterly untenable.*" Exactly so! A *cursory* examination (which, if we are to believe Mr. Borlase, it appears that Fergusson never took the trouble to make, at least as regards the Cornish circles)† is very likely to lead the uninitiated hasty observer to suppose as above. What a prolonged investigation will prove I leave the reader to find further on. It is, at all events, unfortunate for this theory that Mr. Borlase can only produce *two*‡ examples of the tripod class in all Cornwall, viz. those of Lanyon and Caerwynen, and those are both *modern restorations of dilapidated ruins*: not a single stone of either of these examples is as it originally stood "*in situ*." I did not see Mr. Borlase's letter to NATURE until the 3rd inst. On the 5th I obtained old Dr. Borlase's quaint volume on the "Antiquities Historical and Monumental of the County of Cornwall" (2nd ed. 1769), from a chapter in which volume Mr. Fergusson borrows his title of "Rude Stone Monuments," and on the following day visited Lanyon Quoit itself, sketched it, and compared the accounts of it on the very spot, and the following is the result of my investigation. I will take Mr. Borlase's statements categorically:—

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* See Mr. Borlase's letter to the *Antiquary*, July 27, 1872.

† Letter to the *Antiquary*, July 27, 1872.

‡ Mr. Borlase mentions a possible third example, in his "Nænia," p. 26. A fallen cromlech, which may have possibly belonged to the "tripod class," is to be found near Helman Tor, in the parish of Lanlivery.

(1) Lanyon Quoit "always *was*, as it is now, a free-standing dolmen."

(1) I humbly submit that Lanyon Quoit could not possibly have been always as it is now, from the fact of its having fallen, during a violent storm in 1815, whilst a comparison of its plan, as it now is in its restored state, and as it is given by Dr. Borlase, shows that the stones have been moved. The supporters were originally parallel, and are now at different angles to one another.

(2) "A tripod dolmen consisting of three slim pillars supporting on their summits a horizontal stone."

(2) I leave it to my readers to judge from the accompanying representation (from a photograph) of the cromlech whether, from the flat nature of the component stones, the supporters have not more or less the character of slabs rather than that columnar shape necessary for the so-called "Table stone proper:" and whether the *three slim pillars* would not have been more accurately described as *stout stone slabs*. The good Rector of Ludgvan, more than a hundred years ago, more aptly described these Cornish monuments.* "Three or four large flags or thin stones capped with a much larger one, which go by the British name of cromlêhs;" and again, "In several parts of Cornwall we find a large flat stone in a horizontal position (or near it) supported by other flat stones fixed on their edges and fastened in the ground." He never mentions pillars or columnar supports.

Mr. Borlase omits to mention the *fourth* slab (D) which is prostrate to the north (see plate), and the *fifth* and *sixth* flat stones (E and F) (possibly one broken in two) which lie imbedded in the soil at the foot of the south supporter, in which position they were apparently placed by the restorers in 1824 to prop up the upright slab †

(3) Two drawings of it in its pristine condition by Canon Rogers, 1797, and Dr. Borlase, 1747, "agree in representing the slimness of the pillars, their distance apart, and great height of monument, features which render it not unlike a gigantic three-legged milking-stool."

Dr. Borlase's drawing shows *four* upright slabs, although the fourth does not apparently touch the capstone. I think that the supporters A, B, C, may be identified with those in Dr. Borlase's drawing with tolerable certainty, and D, now prostrate, was the fourth upright. that E and F were once also upright is highly probable.

(4) Then, as now, there was no mound about it. It stood on a low bank of earth and the area had been often disturbed by treasure-seekers."

(4) Dr. Borlase says "this cromlêh stands on a low bank of earth not two feet higher than the adjacent soil, about 20 feet wide and 70 feet long." The cromlech stands as much in as on the long mound which, according to the above measurements, would contain at least 2,000 cubic feet of earth, besides the *many rough stones* "not the natural furniture of the place," which Dr. Borlase also mentions. It bears every appearance of having formed the base of a long barrow.

(5) "No houses are near it which could have received the stones of a denuded mound."

(5) A good road with rough stone walls on each side of it, which runs within a few yards of the cromlech, would well account for a portion of a denuded mound or cairn whose stones would be well adapted for building the walls and metalling the road.

(6) "It is difficult to see how a kist-vaen or septum of any kind could have been formed beneath the cap-stone. Had a wall of *small* stones been built from pillar to pillar the height of the superincumbent mound must have forced them inwards, a catastrophe which the "dolmen-builders" were always careful to avoid."

(6) Mr. Borlase must have had experience in his researches among the underground bee-hive caves to know how extensively microlithic dry masonry can be so built up as to resist any outside pressure of a superincumbent mound.

(7) "Had *large* stones placed on edge formed the walls of the kist, how is it they are *all* removed, while every other cromlech in the district retains them?"

(7) In "Nænia Cornubiæ," p. 43, Mr. Borlase writes, with regard to Lower Lanyon Cromlech, "Two stones are all that now remain, viz. the covering stone and one of the supporters; the others having been split up and carried away for building."

(8) "My strongest proof is yet to come. The interment was *not in the kist at all*. A grave had received the body six feet under the natural surface of the surrounding soil, and within the area described by the structure. This being the case, of what use could an enclosed kist have been, or why should the cenotaph be covered in at all?"

(8) Dr. Borlase discovered a pit within the area of the kist-vaen of Mulfra Quoit; and Mr. Borlase himself relates in his Nænia "a small pit seems to have been sunk in the centre" of Chywoone cromlech which he acknowledges was buried in a tumulus. This method of interment would therefore seem common to these three structures.

(9) "On the southern side of the structure, and so near it that a mound over the monument must have inevitably covered it up, stands a little circular ring cairn of the ordinary type, in the centre of which I found the remains of an inner ring which, though now rifled, had doubtless contained an interment."

(9) Dr. Borlase mentions with regard to the long low bank above-mentioned "at the south end, has (*sic*) many rough stones, some pitched on end, in no order; yet not the natural furniture of the surface, but designedly put there; though by the remains, it is difficult to say what their original position was."

Should Mr. Borlase's recognition of the confused aggregation of stones as a ring cairn be correct, it is by no means inconsistent with the theory that a mound once enveloped the cromlech and (as Mr. Borlase suggests would be the case) included the ring cairn in its area.

A parallel case occurs at Moustoir Carnac in Brittany, a plan and section of which, after M. Galles, is given in Fergusson's work, p. 358, and which I have personally examined. Here we find a true dolmen, *two* ring cairns, and a kist within one large long tumulus or barrow.

From my own inspection, I agree with the older Borlase, that "nothing is to be absolutely concluded, there having happened so many disturbances," but I have little doubts that whatever it was it formed some part of a structure in connection with and belonging to the cromlech.

Whilst comparing Cornish cromlechs with French dolmens, a comparison should be made between Chywoone cromlech* and Mr. Fergusson's characteristic example at Grandmont† in Bas-Languedoc (woodcut No. 128), with regard to which he says, "The umbrella form is hardly such as would ever be used for a chamber in a tumulus, but as a pent-roof is singularly suitable for an open-air monument."

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* Antiquities, pp 159 and 223.

† The younger Borlase acknowledges that "several of the stones had been broken." "Nænia," p. 18.

* Nænia Cornubiæ, p 55.

† "Rude Stone Monuments," pp 343, 344. Figured in NATURE, vol. v. p. 387.

The Chywoone cromlech has a peculiar convex-shaped cap-stone or pent-roof; so much so, that "the Quoit itself, seen from a distance, looks much like a mushroom." Mr. Borlase calls it the most perfect and compact cromlech in Cornwall. On exploration, "it was first of all discovered that the building rested on the solid ground, and not on the surrounding tumulus in which it had been subsequently buried." . . . "The barrow or cairn, which in some places nearly reaches the top of the side stones on the exterior, is thirty-two feet in diameter, and was hedged round by a ring of upright stones." . . . "It was discovered that the interstices between the side stones had been carefully protected by smaller ones placed in such a manner as to make it impossible for any of the rubbish of the mound to find its way into the kist."

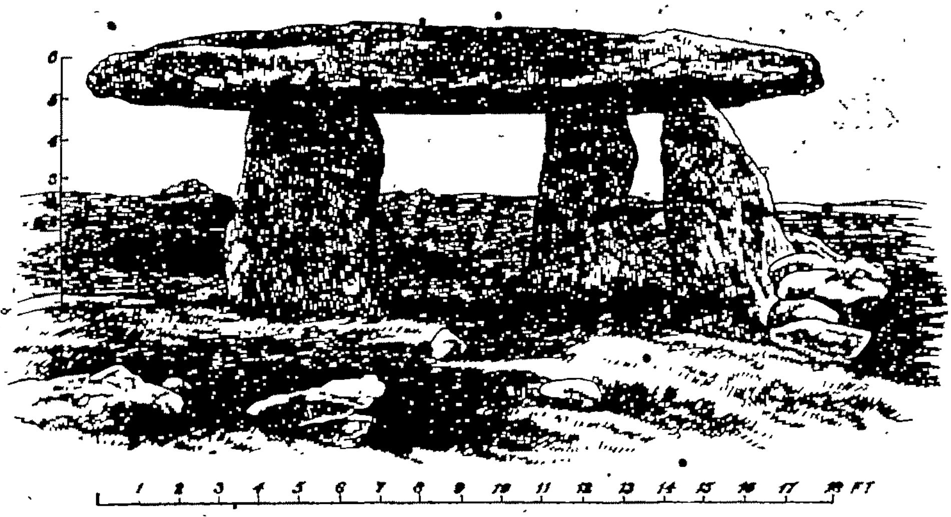

Fig. 1.—Restoration of sole remaining chamber of Lanyon Cromlech, showing fallen side slabs. View from the east.

Mr. Borlase remarks that "the *noscitur a socio* is a principle too lightly regarded by those on whom it forces a conclusion they do not like. In the case of antiquities it is, if judiciously used, extremely valuable." Applying this principle to the two Lanyon cromlechs, is it not just possible that some former owner of the upper cromlech has done what the late owner of the lower one did, viz.,* "remarking that the earth was rich, he thought it might be useful for a compost. Accordingly he sent his servants soon after to carry it off, when, having removed near a hundred cartloads, they observed the supporters of a cromlech."

After the above it is hardly necessary to allude to the Caerwynen cromlech, which has been re-erected in a gentleman's park, more as an ornamental monument than as an archæological record. It is noticeable that in its immediate vicinity is a heap of stones overgrown with thicket, which evidently had some connection with the structure, which was composed of more than four stones.

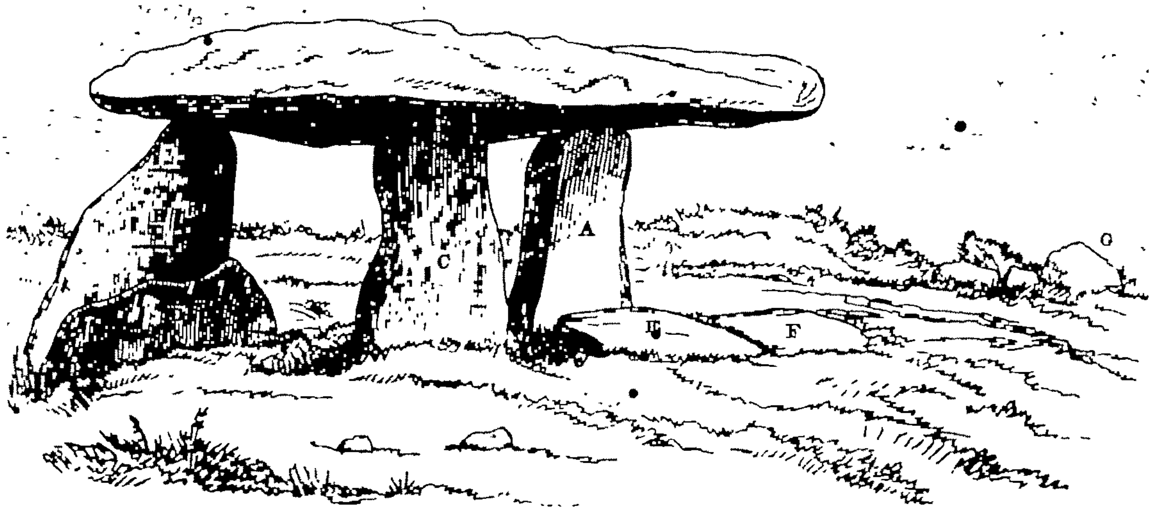

Fig. 2.—Sketch of Lanyon Quoit from the north-west.

In conclusion, it seems to me that the distinction between the dolmens proper and the kist-vaen cromlechs only adds to the difficulties surrounding the subject, and I fear that Mr. Borlase's letter will not tend to strengthen an already weak cause.

Pendennis Castle S. P. OLIVER

* "Nænia Cornubiæ," p. 43

NOTES FROM THE "CHALLENGER"

VI.

WE left Bermudas on Thursday, June 12, for the Azores. His Excellency Gen. Lefroy, C.B., F.R.S., Governor of the Island, with his private secretary, Capt. Trench and Capt. Aplin, R.N., Captain Superintendent of the Dockyard, and a party of ladies, came on board in the afternoon, and we bade farewell, with great regret, to the friends from whom we had received such unvaried kindness during our stay. At half-past five we steamed out of the Camber and passed among the reefs to Murray's Anchorage, on the north-east side of the island, where we anchored for the night. Next morning we proceeded through the narrows, and early in the forenoon, having seen the last of the treacherous and beautiful purple shadows in the bright green waters of Bermudas, we set all plain sail and stood on our course to Fayal. In the afternoon we got up steam and sounded, lat. 32° 37′ N., long. 64° 21′ W., in 1,500 fathoms, with the usual grey-white chalky bottom which surrounds the reefs.

Our position, at noon of the 15th, was lat. 33° 41′ N., long. 61° 28′ W., 1,610 miles from Fayal.

On the morning of the 16th we sounded in 2,575 fathoms, the bottom a reddish ooze, containing a large number of foraminifera. The bottom temperature was 1°·5 C. A small, rather heavy trawl, with a beam 11½ feet long, was put over in the morning, but when it was hauled in, about five in the afternoon, it was found that it had not reached the bottom. This was the first case of failure with the trawl. It was probably caused by the drift of the ship being somewhat greater than was supposed. The net contained a specimen of one of the singular and beautiful fishes belonging to the Sternoptychidæ, an aberrant family of the Physostomi, distinguished by having on some part of the body ranges of spots or glands producing a phosphorescent secretion. The surface of the body is, in most of the species, devoid of scales, but, in lieu of them, the surface of the skin is broken up into hexagonal or rectangular areæ, or separated from one another by dark lines, and covered with a brilliant silvery pigment, dashed with various shades of green or steel blue. We have taken, in all, four or five species of these fishes, all in the net, when dredging or trawling, at great depths. I do not think they come from the bottom, however. It seems more probable that they are caught in the net on its passage to the surface, possibly at a depth of two or three hundred fathoms, where there is reason to believe there is a considerable development of a peculiar pelagic fauna.

On Tuesday, the 17th, the trawl was lowered at seven in the morning, and in the forenoon a sounding was taken in 2,850 fathoms.

Several examples of a large and handsome species of the genus *Scalpellum* came up in the trawl, a few still adhering to some singular-looking concretionary masses which they brought up along with them. One of these lumps, to which a large example of the barnacle was attached, was irregular in form, about three centimetres in length, and two in width. The surface was mammillated and finely granulated, and of a dark-brown colour, almost black. A fracture showed a semi-crystalline structure, the same dark-brown material arranged in an obscurely radiating manner from the centre, and mixed with a small quantity of a fragment of greyish-white clayey matter. This nodule was examined by Mr. Buchanan, and found to consist, like the nodules dredged in 2,435 fathoms at Station 16, 700 miles to the east of Sombrero, almost entirely of peroxide of manganese. Some other concretionary lumps were of a grey colour, but all of them contained a certain proportion of pyrolusite, and they seemed to be gradually changing into nodules of pyrolusite by some process of alteration or substitution. This is undoubtedly very singular, and it is difficult to conceive what can be the source of so widespread a formation of manganese. It is, of course, a matter of great difficulty to make anything like accurate analyses on ship-board. Mr. Buchanan is giving his careful attention to the whole subject of the chemical composition of the sea-bed, and I hope that the determination of the composition of a number of samples, when a favourable opportunity occurs, will throw additional light upon this and a number of other obscure points connected with the chemistry of modern geological formations.

Scalpellum regium, n. sp. (Fig. 1), is by far the largest of the known living species of the genus. The extreme length of a full-sized specimen of the female is 60 mm., of which 40 mm. are occupied by the capitulum, and 20 mm. by the peduncle. The capitulum is much compressed, 25 mm. in width from the occludent margin of the scutum to the back of the carina. The valves are 14 in number; they are thick and strong, with the lines of growth strongly marked, and they fit very closely to one another, in most cases slightly overlapping. When living, the capitulum is covered with a pale-brown epidermis, with scattered hairs of the same colour.

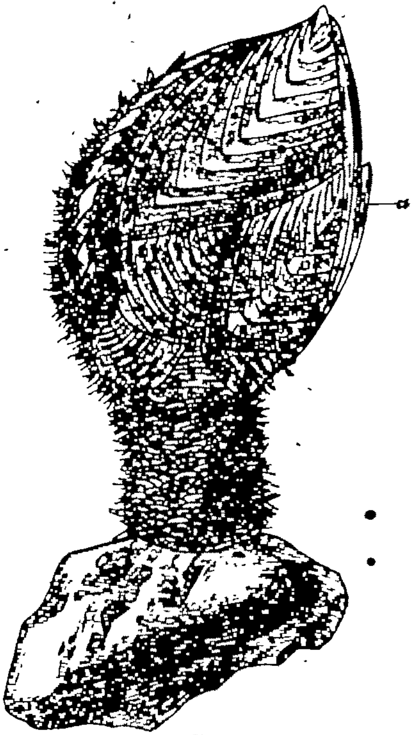

FIG. 1.

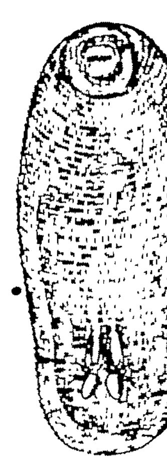

FIG. 2.

FIG. 1.—Scalpellum regium, Wy. Thomson *a*, Males lodged within the edge of the scutum. FIG. 2.—Male of Scalpellum regium.

The scuta are slightly convex, nearly once and a half as long as broad. The upper angle is considerably prolonged upwards, and, as in most fossil species, the centre of calcification is at the apex. A defined line runs downwards and backwards from the apex to the angle between the lateral and nasal margins. The occludent margin is almost straight. There is no depression for the adductor muscle, and there is no trace of notches or grooves along the occludent margin for the reception of the males; the interior of this valve is quite smooth. The terga are large, almost elliptical in shape, the centre of calcification at the upper angle. The carina is a handsome plate, very uniformly arched, with the umbo placed at the apex. Two lateral ridges, and a slight median ridge run from the umbo to the basal margin. The lower part of the valve widens out rapidly, and the whole is deeply concave. The rostrum, as in *Scalpellum vulgare*, is very minute, entirely hidden during life by the investing membrane. The upper latera are triangular, the upper angle curving rather gracefully forwards; the umbo of growth is apical.

The rostral latera are long transverse plates lying beneath the basal margins of the scuta. The carinal latera are large and triangular, with the apex curved forwards very much like the upper latera, and the inframedian latera are very small, but in form and direction of growth nearly the same.

The peduncle is round in section and strong, and covered with a felting of light-brown hair. The scales of the peduncle are imbricated and remarkably large, somewhat as in *S. ornatum* Darwin. About three, or at most four scales, pass entirely round the peduncle. The base of attachment is very small, the lower part of the peduncle contracting rapidly. Some of the specimens taken were attached to the lumps of clay and manganese concretions, but rather feebly, and several of them were free, and showed no appearance of having been attached. There is no doubt, however, that they had all been more or less securely fixed, and had been pulled from their places of attachment by the trawl. On one lump of clay there were one mature specimen and two or three young ones, some of these only lately attached. The detailed anatomy of this species will be given hereafter, but the structure of the soft parts is much the same as in *Scalpellum vulgare*.

In two specimens dissected there was no trace of a testis or of an intromittent organ, while the ovaries were well developed; I conclude, therefore, that the large attached examples are females, corresponding, in this respect, with the species otherwise also most nearly allied, *S. ornatum*.

In almost all the specimens which were procured by us, several males, in number varying from five to nine, were attached within the occludent margins of the scuta, not imbedded in the chitinous border of the valve, or even in any way in contact with the shell, but in a fold of the body-sac quite free from the valve. They were ranged in rows, sometimes stretching—as in one case where there were seven males on one side—along the whole of the middle two-thirds of the edge of the tergum.

The male of *Scalpellum regium* (Fig. 2) is the simplest in structure of these parasitic males which has yet been observed. It is oval and sac-like, about 2 mm. in length by ·9 mm. in extreme width. There is an opening at the upper extremity which usually appears narrow, like a slit, and this is surrounded by a dark, well-defined, slightly raised ring. The antennæ are placed near the posterior extremity of the sac, and resemble closely in form those of *S. vulgare*. The whole of this sac, with the exception of a small bald patch near the point of attachment, is covered with fine chitinous hairs arranged in transverse rings. There is not the slightest rudiment of a valve, and I could detect no trace of a jointed thorax, although several specimens were rendered very transparent by boiling in caustic potash. There seems to be no œsophagus nor stomach, and the whole of the posterior two-thirds of the body in the mature specimens was filled with a lobulated mass of sperm-cells. Under the border of the mantle of one female there were the dead and withered remains of five males, and in most cases one or two of the males were not fully developed; several appeared to be mature, and one or two were dead, empty, dark-coloured chitine sacs.

On Wednesday, June 18, we resumed our course with a fine breeze, force 5 to 7, from the south-east. In this part of our voyage we were greatly struck with the absence of the higher forms of animal life. Not a sea-bird was to be seen, with the exception of a little flock of Mother Carey's chickens, here apparently always *Thalassidroma wilsoni*, which kept playing round the ship, on the watch for food, every now and then concentrating upon some peculiarly rich store of offal as it passed astern, and staying by it while the ship went on for a quarter of a mile, fluttering above the water and daintily touching it with their feet as they stooped and picked up the floating crumbs, and then rising and scattering in the air to overtake us and resume their watch.

The sea itself in the bright weather, usually under a light breeze, was singularly beautiful—of a splendid indigo-blue of varying shades as it passed from sunlight into shadow, flecked with curling white crests; but it was very solitary: day after day went by without a single creature (shark, porpoise, dolphin, or turtle) being visible. Some gulf-weed passed from time to time, and bunches of a species of *Fucus*, either *F. nodosus* or a very nearly allied form, evidently living and growing, and participating in the wandering and pelagic habits of *Sargassum*. The floating islands of the gulf-weed, with which we have become familiar as we have now nearly made the circuit of the "Sargasso Sea," are usually from a couple of feet to two or three yards in diameter, sometimes much larger; we have seen, on one or two occasions, fields several acres in extent, and such expanses are probably more frequent nearer the centre of its area of distribution.

They consist of a single layer of feathery bunches of the weed *Sargassum bacciferum*, not matted together, but floating nearly free of one another, only sufficiently entangled for the mass to keep together. Each tuft has a central brown thread-like branching stem studded with round air-vesicles on short stalks, most of those near the centre dead, and coated with a beautiful netted white polyzoon. After a time vesicles so encrusted break off, and where there is much gulf-weed the sea is studded with these little separate white balls. A short way from the centre, towards the ends of the branches, the serrated willow-like leaves of the plant begin, at first brown and rigid, but becoming, farther on in the branch, paler, more delicate, and more active in their vitality. The young fresh leaves and air-vesicles are usually ornamented with the stalked vases of a *Campanularia*. The general colour of the mass of weed is thus olive in all its shades, but the golden olive of the young and growing branches greatly predominates. This colour is, however, greatly broken up by the delicate branching of the weed, blotched with the vivid white of the encrusting polyzoon, and riddled by reflections from the bright blue water gleaming through the spaces in the network. The general effect of a number of such fields and patches of weed, in abrupt and yet most harmonious contrast with the leaves of intense indigo which separate them, is very pleasing.

These floating islands have inhabitants peculiar to them, and I know of no more perfect example of protective resemblance than is shown in the gulf-weed fauna. Animals drifting about on the surface of the sea with such scanty cover as the single broken layer of the seaweed, must be exposed to exceptional danger from the sharp sea-birds hovering above them, and from the hungry fishes searching for prey beneath, but one and all of these creatures imitate in such an extraordinary way, both in form and colouring, their floating habitat, and consequently one another, that we can well imagine their deceiving both the birds and the fishes. Among the most curious of the gulf-weed animals is the grotesque little fish, probably *Antennarius marmoratus*, which finds its nearest English ally in the "fishing frog" (*Lophius piscatorius*), often thrown up on the coast of Britain, and conspicuous for the disproportionate size of its head and jaws, and for its general ugliness and rapacity. None of the examples of the gulf-weed *Antennarius* which we have found are more than 50 mm. in length, and we are still uncertain whether such individuals have attained their full size. It is this little fish which constructs the singular nests of gulf-weed bound in a bundle with cords of a viscid secretion, which have been already mentioned as abundant in the path of the gulf-stream.

Scillæa pelagica, one of the shell-less mollusca, is also a frequent inhabitant of the gulf-weed. A little short tailed crab (*Nautilograpsus minutus*) swarms on the weed and on every floating object, and it is odd to see how the little creature usually corresponds in colour with whatever it may happen to inhabit. Mr. Murray, who has the general superintendence of our surface work, brings in curious stories of the habits of these little crabs. We observe that although every floating thing upon the surface is covered with them, they are rarely met swimming free, and that whenever they are dislodged and removed a little way from their resting place, they immediately make the most vigorous efforts to regain it. The other day he amused himself teasing a crab which had established itself on the crest of a *Physalia*. Again and again he picked it off and put it on the surface at some distance, but it always turned at once to the *Physalia* and struck out, and never rested until it had clambered up into its former quarters.

On Thursday, the 19th, we sounded in 2,750 fathoms in a grey mud containing many foraminifera. Position of the ship at noon, lat. 35° 29′ N., long. 50° 53′ W.

The wind now gradually freshened, and for the next three days we went on our course with a fine breeze, force from 4 to 7, from the southward, sounding daily at a depth of about 2,700 fathoms, with a bottom of reddish grey ooze. On Tuesday the 24th the trawl was put over in 2,175 fathoms, lat. 38° 3′ N., long. 39° 19′ W., about 500 miles from the Açores. As in most of the deep trawls on grey mud, a number of the zoœcia of delicate branching polyzoa were entangled in the net. One of these on this occasion was very remarkable from the extreme length (4 to 5 mm.) of the pedicels on which its avicularia were placed. Another very elegant species was distinguished by the peculiar sculpture of the cells, reminding one of those of some of the more highly ornamented *Lepraliæ*.

WYVILLE THOMSON

(*To be continued.*)

THE FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE second session of the French Association was opened at Lyons last Thursday, by an inaugural address from the President, M. de Quatrefages, who pointed out the almost inconceivable advance of Science during the past century, and the importance of Science in education.

In speaking of scientific education, the President said that the devotees of literature accused Science of stifling sentiment and imagination; she kills, say they, the ideal and stunts intelligence by imprisoning it within the limits of reality; she is incompatible with poetry. The men who speak thus have never read Kepler the astronomer, Pascal the geometer, Linnæus the naturalist, Buffon the zoologist, Humboldt the universal *savant*. What! says the President, Science stifle sentiment, imagination, she who brings us every hour into the presence of wonders! She lower intelligence, who touches on all the infinities! When *littérateurs* and poets know Science better, they will come and draw from her living fountain. Like Byron of our time, like Homer of yore, they will borrow from her striking imagery, descriptions whose grandeur will be doubled by their truth. Homer was a *savant* for his time. He knew the geography, the anatomy of his era; we find in his verses the names of islands and capes, technical terms like *clavicle* and *scapula*. None the less he wrote the Iliad.

No, the study of Science will never suppress the genius of an inspired poet, of a true painter, of a great sculptor. But she will bring more light to the path of an erring soul. She will perhaps transform into a wise man, or at least into a citizen useful to himself and others, one who without her would only have been one of those pretended incomprehensible geniuses, destined to perish of misery, of impotency, and of pride. While fully admitting the important place of literature in education, he would wish to see children initiated at an early age into the facts, the ideas, the methods of Science.

Governments, such as they have hitherto been, have almost always acted as if they had no need for the men who study Nature and her forces. But when any critical or important event occurs, then it is found necessary to appeal to them. Of whom are the juries of International Exhibitions composed? No doubt each State sends its worthy merchants, its tried chiefs of industry, its eminent agriculturists, but it also, and above all, sends its men of science. At these important times peoples are comparing their real strength, and each feels that it is for its honour in the present and its prospects in the future that the truth should appear; and to enlighten them, whether it be concerning cannons or silk-manufactures, telescopes or crystals, jewellery or hardware, it is felt that Science is indispensable, and men of science are appealed to.

But once the Exposition is closed, the State leaves the men of science to return to their studies. I wish, said M. de Quatrefages, it kept them in the service of their country. These men whom we ask to understand and judge of wonders would certainly be able to show how to produce them. When Science is everywhere, it would certainly not be useless to Government to have it in their power to be enlightened at any time on scientific questions. Although less pressing, less imperious than in the days of peril, the wants of agriculture, of industry, of commerce, like those of the army and navy, do not change their nature. Why wait the necessity for appealing to the *savants*?

A day will come when every great Administration will have its Consulting Committee, composed almost exclusively of men of science, and then many mistakes will be avoided, and many forces utilised which are at present lost. But in order that such an institution should be born and developed, it is necessary that the function of Science be universally comprehended and accepted. To attain this result is one of the chief aims of the French Association.

CHRISTOPHER HANSTEEN

ON the 11th of April last, Hansteen died at Christiania at the advanced age of 88, having been born on the 26th September, 1784. On leaving the cathedral school of Christiania, where he received his early education, he entered the University of Copenhagen in 1802, as a student of law, which, however, he soon abandoned for the more congenial study of mathematics. In 1806, he began his work as a public instructor in the capacity of mathematical tutor in the gymnasium of Fredricksburg, in the island of Zealand, and there he began also his life work as an original investigator by instituting researches into terrestrial magnetism. He first acquired distinction by taking the prize which had been offered for the best essay on this subject, by the Royal Society of Science of Copenhagen; and shortly thereafter, viz. in 1814, was appointed to the chair of Astronomy in the University of Christiania, which had been recently founded by Frederick VI. of Norway.

His great work, entitled "Untersuchungen über den Magnetismus der Erde," was published in 1819, at the expense of the King. This work was illustrated with an Atlas of Maps, and was the most satisfactory collection of observations on the variations of the needle, and was besides distinguished for its broad philosophical generalisations. In the further prosecution of his physical researches, he made his well-known journey into Siberia as far as Kiachta and Irkutsk, accompanied by Erman and Due, the expenses of this journey being liberally defrayed by the Norwegian Government. The establishment, on the recommendation of Humboldt, of the ten magnetical and meteorological observatories, by the Emperor of Russia, was one of the most valuable fruits of this journey.

Among Hansteen's contributions to our knowledge of magnetism, may be mentioned the establishment by him of a period of 11·1 years as the length of the periodicity of the magnetic declination—a cycle which has recently assumed such remarkable significance in connecting astronomical with meteorological and other terrestrial phenomena.

Soon after his return from Siberia, the Government voted the necessary sum for building an astronomical and meteorological observatory at Christiania, which was erected under Hansteen's direction. This observatory has done much good work, of which the meteorological department deserves very special commendation. The trigonometrical and topographical survey of Norway, which was begun in 1837, was conducted under Hansteen's superintendence.

In 1856, the completion of his fifty years public services was celebrated, and a medal was struck in commemoration of the event. Shortly after this he ceased to lecture publicly, and in 1861 retired from public duty.

THE NOTORNIS OF LORD HOWE'S ISLAND

THE last number of the *Ibis* (July 1873, pl. x.) contains a representation of a very interesting bird, about which, though discovered and described in the last century, naturalists have for a long time been doubting. This is the species said to be first mentioned by Callam in 1783 (Voy. Bot. Bay), and subsequently figured in the works of John White (Journ. Voy. New South Wales, p. 238, App.) and Governor Phillip (Voy. Bot. Bay, p. 273, pl.), and designated by Latham (Ind. Orn. ii. p. 768) *Gallinula alba*. No specimens are known to have been brought to Europe for upwards of *eighty* years, and only two are believed to exist in museums—one in that of Liverpool, which was figured by White, and the other in Vienna, now for the first time portrayed. The species is most likely extinct in Norfolk Island, but a passage in a pamphlet by Mr. Edward Hill, published at Sydney in 1870, seems to show that it may still exist in that of Lord Howe—though, if so, doubtless on the verge of extermination through the pigs, with which the island is said to be overrun, for the bird is believed to be unable to fly. Should any examples be still living, it would certainly be better that their remains should be placed in our museums, than that they should contribute to the formation of pork; and I write these lines that they may attract the attention of some Australian readers of NATURE, who may be disposed to do a good turn to the University of Cambridge.

This bird, which has been variously assigned to the genera *Gallinula* (moor-hen), *Fulica* (coot), and *Porphyrio*, is now referred to the genus *Notornis*, containing only one other species, the "Takahe" of New Zealand (*N. mantelli*)—itself nearly, or quite, extirpated. It was about the size of a barndoor-fowl, with the bill and legs red. The Viennese specimen seems to be entirely white; the example at Liverpool is mottled with purple, but not enough to gainsay the name of "White Bird," by which it seems to have been known both in Norfolk and Lord Howe's Islands. It would no doubt, if taken alive, be easily kept in confinement, and I need scarcely say how highly a living example would be valued by the Zoological Society; but this is perhaps more than can be reasonably hoped for, and, so far as I am concerned, I should be well content with a specimen in spirit, or a skin with all the bones accompanying it, for the Cambridge Museum.

I may perhaps be allowed to conclude by remarking that the history, and especially the distribution of the family of birds, to which the subject of this notice refers is indeed worthy of far more attention than they have hitherto received, and could that accomplished zoological writer who has lately in the columns of a sporting contemporary made the not very distant family of *Gruidæ* the theme of an admirable series of essays—far probably from being fully appreciated by his readers—be induced to employ his pen on the *Rallidæ*, the results would be of the greatest interest. The Rails—employing the word in a very wide sense—are cosmopolitan in the highest degree. Some of the best known genera have their representatives all over the world, occurring even in oceanic islands, where birds generally are so scarce—*Gallinula* and *Fulica*, for instance; and some at least of the former, when they get to such remote spots, seem to lose their volatile powers, though otherwise undergoing but little change, as witness the *G. nesiotis* of Tristan d'Acunha, made known a few years ago by Mr. Sclater, and a form still undescribed, of which three examples were obtained by my brother from Denis Island, an outlier of the Seychelles group (*Ibis*, 1867, p. 358). Then there is a genus equally flightless, which has lately been restored to light and knowledge, but, alas! too late for us to know it in the flesh. This is the *Aphanapteryx*, which survives only in a few bones, recovered from the mud of a Mauritian lake, and now in the Cambridge Museum, a painting at Vienna, and a few notices by early voyagers—a bird with a long bill and dishevelled plumage, almost, it would seem, like that of the *Apteryx*. In the opposite direction almost, as to structure, we have *Tribonyx*; but I should occupy far too much space were I now to dwell upon even the chief forms of the family. From whatever point of view it be regarded, it will be found one of the most interesting in the whole series of birds.

ALFRED NEWTON

ASTRONOMICAL ALMANACS *

II.

II.—*The "Connaissance des Temps," under the direction of the Academy of Sciences*

THE first to whom the Academy entrusted the editorship of these Ephemerides was Lieutaud.

The only real modification introduced into the volume was the substitution, for the table of refractions published by Lefebvre, of a table of the refractions of Cassini, giving the values of that quantity in minutes and seconds for all degrees of height, from 0° to 90°. The book was also somewhat increased in size. In 1707 Lieutaud introduced into the *Connaissance des Temps* a notice of the occultations of stars, the observation of which is of use in determining longitudes. Lieutaud edited the *Connaissance des Temps* till 1730, when it passed into the hands of a young academician, Louis Godin.

Godin, a pupil of Delisle, was born at Paris on February 28, 1704, and entered the Academy as *élève* at the age of 21 years. He was then known only by a keen desire for knowledge and a strong predilection for astronomy. On taking the direction of the *Connaissance des Temps*, he suppressed the aspects of the planets, which were useless, and introduced the right ascension of the sun for every day of the year; calculated this co-ordinate and the declination to a second, and added the eclipses of the satellites of Jupiter, so that the *Connaissance des Temps* contained from this time the announcement of the eclipses of the superior satellites.

In 1735 Godin set out for Peru for the purpose of measuring with Bouguer and La Condamine an arc of one degree of the meridian, and to Jean-Dominique Maraldi, grand-nephew of Cassini the elder, was committed the care of the *Connaissance des Temps*. He enriched the work with the configuration of the satellites of Jupiter for every day in the year, but he suppressed the notice of occultations, a great mistake, certainly; though perhaps these phenomena were of little service in his time. Having become a *pensionnaire* of the Academy in 1760, he resigned the editorship of the *Connaissance des Temps* to Joseph-Jerôme Le François de Lalande.

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* Continued from p. 314.

De Lalande, born at Bourg-en-Bresse, July 11, 1732, was sent at the age of 20 to Berlin, under the patronage of Le Monnier, his master, to take observations of the moon, which, combined with those which La Caille at that time effected at the Cape of Good Hope, were the means of giving the parallax of that planet. On his return he was presented to a place vacant for many years in the Academy, and shortly after, in 1760, he was entrusted with the editorship of the *Connaissance des Temps*. A distinguished astronomer, possessing a thorough knowledge of all the advances which had been made during later years in astronomical science, Lalande very much improved the work of which he had charge. We shall mention the most important of the changes which are due to him.

His first care was to take for the basis of his calculations new tables, more exact than those which Godin had continued to employ. He employed for the sun the tables of the Abbé of La Caille; for the moon, those of Tobie Mayer[*]; for the planets, the tables of Cassini; and for the eclipses of the satellites of Jupiter, those of the Swede Wargentin, of which he had published a new edition. The rising of the sun and the planets is calculated for the true noon of each day; but, says Lalande, "the *Connaissance des Temps* being intended mainly for astronomers, the positions of the moon are given for the instant of her passage across the meridian." The following year, however, "on account of the inconveniences attending such a mode of indication," this astronomer resolved to give the longitudes for midday and midnight of each day. Finally, in a short and well-written memoir appended to the *Connaissance des Temps*,[†] he investigated the different methods for finding the longitude at sea by a single observation of the moon. Some years later he restored the announcement of the occultation of stars.

In 1774, the *Connaissance* received from Jerôme Lalande a most important improvement, which was the means of making this work, hitherto almost exclusively intended for astronomers, of great use to mariners. But, before stating in what this modification consisted, some historical details are necessary concerning one who was the real pioneer, and at the same time one of the glories of French astronomy in the 18th century.

In 1737, the *savant* Fouchy presented to Cassini of Thury, son and successor of the first director of the Observatory of Paris, celebrated for his fine work on "The Size and the Figure of the Earth," a young deacon of 23 years, who, alone, without instruments and almost without books, had acquired a remarkable astronomical education. Cassini welcomed the *protégé* of Fouchy, lodged him at the Observatory, and allowed him to take part in his work. This young Abbé was Nicolas-Louis de la Caille, born on March 15, 1713, at Rumigny, near Rozoy, in Thiérache. J. D. Maraldi, grand-nephew of Cassini the first, and who also lived at the Observatory, became his friend, and a year after his arrival (1738), La Caille made along with him the geographical description of the coast of France, from Nantes to Bayonne; in 1739 La Caille took part in the work connected with the meridian of France.[‡] Shortly after, Dr. Robbes nominated him professor of mathematics at the Mazarin College. He instituted a small observatory where he made a very large number of observations of rare precision. In 1741, at the age of 27 years, La Caille entered the Academy of Sciences.

In 1744 the astronomer of the Mazarin College published the first volume of a series of Ephemerides, entitled "Ephémérides des monuments célestes depuis 1745 jusq'en 1754," in which he was the first to give—and Lalande afterwards imitated him in the *Connaissance des Temps* of 1760—the distance of the sun at the equinox, or, what amounts to the same thing, the right ascension of the sun in time.

Some years later, in 1749, La Caille proposed to the Academy that he should spend a year at the Cape of Good Hope, for the purpose of making an accurate catalogue of the stars of the southern sky, intended to replace the first rough sketch made in 1677, by Halley, at St. Helena; to measure the parallax of the moon, of Venus, and of Mars, by means of comparative observations made simultaneously in Europe; and finally to determine carefully the geographical position of the Cape of Good Hope.[*]

The proposal of La Caille was adopted, and the States-General of Holland having given their assent, La Caille set out in 1751, after having published the list of stars which he wished to be observed by the European astronomers, for the purpose of rendering his voyage fruitful in scientific results. We do not intend to recount all the incidents of this expedition. Let us, however, mention a fact which illustrates well the character of this astronomer, "reserved, modest, and disinterested." He received for his expedition, the purchase of instruments, and other expenses, for his maintenance and that of an artist, the sum of 10,000 livres; on his return, he found he had spent only 9,145 livres. He scrupulously carried back the balance to the royal treasury; the officials, surprised, would not accept it. "You require it," they said to him; "it will take it to remunerate you." Moreover, when he set out from the Cape, the minister had charged him to make maps of the Isles of France and of Bourbon, which were not comprised in the original plan, and "for which most others would have asked, and certainly obtained, a supplementary indemnity."[†]

The observations made during this expedition (1751 and 1752) by La Caille with his telescope of 26 inches focus, and an inch and a half aperture, were published by himself, and after his death, by Maraldi, in 1763, under the title, "Cœlum australe stelliferum, seu observationes ad construendum stellarum Australius catalogum institutæ, in Africa ad Caput Bonæ-spei, à Nicolao-Ludovice De La Caille."

A new edition of this catalogue was published in 1847, under the superintendence and at the expense of the British Association and the British Government, under the editorship of Messrs. Bailly and Henderson, the latter, at the time, Director of the Edinburgh Observatory.[‡]

But, besides, this voyage to the Cape of Good Hope had a most important result. During the two journeys, La Caille tested and compared all the methods employed till then to determine longitude at sea. Among these he noted that which the celebrated Halley had given in 1678, and which is based upon the observation of the distance of the moon from the sun or from a star. The experiments which he made in reference to it having convinced him of its excellence, he strongly recommended it on his return to France; and in his second volume of Ephemerides, which commenced in 1755, he proposed a Nautical Almanac, in which should be found, for every hour of the day, the distance of the moon from the sun and the stars. La Caille regretted that his other occupations would not permit him to compile this nautical Ephemerides himself. At a later time, in his treatise on navigation, he reverted to the same subject, and gave anew the sketch of his almanac, limiting himself to giving the distances every four hours for the meridian. His design was not followed. Lalande contented himself with analysing and discussing La Caille's method in the *Connaissance des Temps* for 1760. As to the French Marine, it was content to use "L'état du Ciel, calculé par Pingré et rapporté à l'usage des marins, 1754, 1755, 1756, et 1758." It was very different, however, in England.

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[*] "Tabularum motuum solis et lunæ et longitudinum methodus pro mota.'

[†] Lalande afterwards regularly followed the custom of accompanying the *Connaissance des Temps* with short astronomical memoirs, entitled "Additions to the *Connaissance des Temps*." This custom has continued to the present day

[‡] The work done by Cassini de Thury, Maraldi, and La Caille, was published by La Caille in 1744, and bore the name of Cassini de Thury.

[*] La Caille also purposed to observe the length of the seconds pendulum, the variation of the magnetic needle, and finally the length of a degree of the meridian at the Cape. This has since been measured under the equator, under the Polar Circle, and in various places in Europe; but we do not yet know the value of any degree in the southern hemisphere.

[†] In the accounts which he rendered on his return, La Caille has put down only *five sous* for his daily expenses, and as much for those of a mechanician who accompanied him.

[‡] The Association gave 500*l.* and the Government 1,000*l.* It is entitled "A Catalogue of 9,766 Stars in the Southern Hemisphere for the beginning of the year 1750, from the Observation of the Abbé de la Caille."

(*To be continued.*)

SOUTH AFRICAN MUSEUM

THE *Cape Argus* for July 10 contains the report of the curator, Mr. Roland Trimen, of the South African Museum, for the previous half year. Many valuable additions have been made to the museum during that time, but its efficiency is very seriously crippled through want of funds, mainly due, we are sorry to say, to the parsimony of Government. We regret to see that the number of subscribers has seriously diminished from what it originally was, but the success of so valuable an institution should in no way be dependent on the capricious revenue to be derived from such a source. Let us hope that recent changes in the *personnel* of the Government will lead to greater liberality for this and for other scientific purposes. We cannot do better than give a few extracts from an excellent leader in the *Argus* on the Curator's report.

"Now that strong efforts are being made to forward the interests of education in the Colony, those institutions that aid in the work should not be neglected. We do not at present refer to colleges and schools, for these, whenever education is discussed, come prominently before the popular mind, but our remarks are directed rather to such places as museums, whose work in higher education of the kind required in modern days is of considerable importance . . . It has often struck us as rather a reflection on Cape Town that there is no Society here for the discussion of natural science subjects, and though we are aware of some obstacles to the successful working of such a body, we see no reason why they should not be overcome. In the capital of every Colony of which we have any knowledge, a Society of the kind exists, and indeed in the Cape itself there are towns that, in this respect at least, are ahead of the metropolis.

"But though we have no Natural Science Society in Cape Town, we have what, all things considered, may be said to be an excellent Museum. . . . The Museum was founded under the enlightened influence of the then Governor, Sir George Grey, in 1855, and in 1857 was incorporated by Act of Parliament. Its first trustees were Mr. Rawson, the Colonial Secretary at the time, Sir Thomas Maclear, the then Astronomer Royal, and Dr. Pappe, the then Colonial Botanist. On Dr. Pappe's death Mr. C. A. Fairbridge was appointed a trustee, and upon the resignation of Mr. Rawson, on his departure from the Colony, his place was filled by Mr. Southey, now Lieut.-Governor of Griqualand West. It will be thus seen that the Museum has from the first been under the management of trustees alike of scientific acquirements and business ability. In its first curator, Mr. Layard, it was extremely fortunate, and it had the advantage of his enthusiastic labours for the lengthy period of fifteen years.

"But though it has had the advantage of excellent management, the development of the institution has been seriously hindered from want of funds, and it has not received, either from the Legislature or the public, that pecuniary support necessary to secure the services of efficient officers and to meet the thousand and one expenses of cases, glass, chemicals, and the appliances and apparatus required in carrying out the work of a museum. It is a wise policy on the part of the Legislature to vote grants of money to such institutions in proportion to the pecuniary support received from the public, and if Parliament is to be induced to make a larger grant to the Museum, the private subscription list must be extended. The small sum of one guinea represents the subscription for a year, and we are quite sure, when it is known how much the institution stands in want of funds, the list of subscribers will become larger.

"Strangers who visit the Museum and who know how such things are managed elsewhere must smile when told that its curator is a clerk in the Civil Service, whose time is chiefly occupied in doing the work of a subordinate officer in the Colonial Office. We say this without any intention of disparaging the gentleman referred to, for his attainments in one branch of Science at least are universally admitted; but we do say that, if the South African Museum is to be anything like worthy of the name, and if it is to continue efficiently to perform the work so well commenced by Mr. Layard, its curator should devote the whole of his time and attention to the duties of that office. Under existing circumstances, that, however, is not to be expected, as the salary is not sufficient to induce any qualified gentleman to give up other positions for the sake of applying himself entirely to the work of the Museum.

"There are other matters connected with this institution to which we might draw attention, but until more public support is given to the Museum it would be a waste of time to refer to them."

GEOLOGICAL MAP OF AUSTRALIA AND TASMANIA *

GEOLOGICAL surveys have been proceeding, to a greater or lesser extent, in all the Australian colonies for several years, and in Victoria the work has been prosecuted so systematically, and with such success, that the main features of the surface geology of the country are comparatively well ascertained and mapped out. The example in this respect set by Victoria has been followed to a very considerable extent by Queensland, and in a lesser degree by several of the other colonies. A geological map of Australia has, however, never been issued. Such a work would be invaluable, and the materials obtained are quite sufficient to justify an attempt being made to carry it out. Such an attempt is now being made by the Mining Department of Victoria. Some months since the Hon. A. Mackay, Minister of Mines, put himself in communication with the Governments of the other colonies with the view of obtaining from them all the information in their possession respecting the geological characteristics of the territories over which they presided. The application was readily acceded to, and a large mass of materials has been since placed at the disposal of the Mining Department of Victoria. Under the direction of Mr. R. Brough Smyth, F.G.S., Secretary for Mines, this has been thoroughly digested and arranged, and is now being embodied in a map, which, when completed, as it will be shortly, will show at a glance the result of all geological surveys made in Australia and Tasmania up to the present time. As the value of such a work necessarily depends upon the accuracy of the observations upon which it is based, it may be well, before attempting a brief description of its main features, to indicate the source from whence the materials used in its compilation have been derived. The geological sketch map of Victoria, exhibited by the Mining Department at the late Intercolonial Exhibition, and which contains the results of the latest surveys made in the colony, will be embodied in the general map. It was compiled by Mr. Brough Smyth from surveys made some years ago under the direction of Mr. A. R. C. Selwyn, at present director-general for the Geological Survey of Canada, but who formerly held a similar position in this colony, and from surveys made since by the officers of the mining department. It has been described "as the nearest approximation that can at present be made to a true representation of the rock masses which are exposed in this colony." The New South Wales Government have in preparation a geological map, which, it is expected, will be available for use before the general map is published.

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* From an article in the *Melbourne Argus*, July 7.

The Queensland Government has been keenly alive to the importance of mapping out the immense mineral districts of that colony, and for some years has kept a staff of geological surveyors actively employed in the work. The information thus collected has been embodied in a series of elaborately-coloured and beautifully-executed maps, which have proved of great service in the compilation of the general map of Australia. An excellent sketch map, covering a considerable portion of the colony, has been obtained from the Government of South Australia. It was compiled under the direction of Mr. A. B. Cooper. It is especially valuable, as it embraces a great part of the populated districts. The country north of Encounter Bay, the most extensive mineral district in the southern portion, was examined and reported on by Mr. Selwyn many years ago, and a sketch map prepared by him is being used in compiling the new map. The same district has been very recently reported upon by Prof. Ulrich, at the request of the Government, and his observations are proving of great assistance.

Thanks to the energy of Mr. C. Gould, a son of the eminent naturalist, the geological characteristics of Tasmania were very accurately delineated during the time he was geologist for the colony. An excellent map was published under his direction, and he voluntarily made a number of additions to it a short time ago, when he learnt that a copy was to be transmitted to Victoria to be used in the preparation of the general geological map of Australia.

A large portion of the vast territory of Western Australia has been surveyed by Mr. H. Y. L. Brown, Government surveyor, but who was once attached to the geological staff of Victoria. This gentleman has produced a very beautiful sketch map of the S.W. portion of the colony, which has been extensively used by the compiler of the new map. It thus appears that every care has been taken to obtain the most accurate information at present available.

An examination of the map discloses facts of interest not only to geological students but to the public at large. The value of the map to men engaged in mining is too palpable to call for comment, as it shows at a glance the formations in which the precious metals occur. In rocks belonging to the primary or palæozoic group, gold, tin, antimony, silver, lead, and copper may be confidently searched for. The secondary or mesozoic rocks contain coal, while tin is frequently found associated with granitic rocks. Persons engaged in pastoral and agricultural pursuits will also derive advantage by consulting this map. A very little geological knowledge will tell them that in districts where the principal rock masses belong to the tertiary period they may look for well-grassed plains suitable for pasture. In areas where the volcanic rocks abound, rich soil, well adapted for agricultural pursuits, may be expected. The slatey ridges formed by the older silurian rocks, and the sparsely grassed mountains of granitic rock which abound in Western Australia, also convey a valuable lesson to the intelligent observer. One of the most prominent geological facts which the map discloses is, that a great metalliferous belt lies on each side of the main Cordillera from Cape Yorke to the southern point of Tasmania. It is composed chiefly of metamorphosed schists and granite rocks overlain in a considerable area by the newer palæozoic rocks and mesozoic coal-bearing strata. Another great belt appears to extend from Encounter Bay in South Australia towards the Gulf of Carpentaria. North of the 30th parallel of latitude the schists are overlain by tertiaries, and what Mr. Daintree considers to be rocks of the cretaceous age up to lat. 20° to 23°, where a large patch of metamorphic schist occurs. The whole tract west of the eastern metalliferous belt is occupied by tertiaries. Wide treeless plains, and what are called desert sandstones, abound. The vast tract of country known as Central Australia will have to be marked "unknown," as geological surveys have not yet been made of it. What is at present known of the geological character of the northern portion of South Australia will be mapped out. The Government of South Australia have furnished a very good map showing the palæozoic tract of Port Darwin, and from notes made by explorers the department has been able to lay down a large granitic tract also, as well as a large area covered with rocks of volcanic origin. The coal rocks are seen extending all along the coast from Port Curtis, in Queensland, in an almost unbroken line to Eden or Twofold Bay. They are especially prominent at Newcastle and Wollongong, in New South Wales. They again appear north of Corner Inlet, at Cape Otway, and can be traced in broken patches along the coast up to Glenelg, where they apparently terminate. Another interesting fact established by the new map is, that within the tertiary era connection has existed between Tasmania and the main land. There is a strict resemblance between the geology of Tasmania and the continent, and the chain of granite islands extending from Wilson's Promontory, the southernmost point of Australia, to Cape Portland, the northernmost point of Tasmania, have all their ridges capped with tertiaries, thus showing that within the tertiary period the island and the continent must have been connected. The main geological characteristic of Western Australia is the immense area occupied by granitic rocks, varied occasionally by patches of sandstone, especially on the southern coast line. A comparatively small part is occupied by a belt of metamorphic rocks to the east of Champion Bay. Volcanic rocks are also visible. A large granitic tract occurs in the basin of the Shaw River, east of Dampier's Archipelago. It appears that there has been a greater amount of denudation on the western side of the continent than on the eastern. Where the altitude is that of the Dividing Range, which varies from about 1,500 ft. to 7,000 ft., either granite, metamorphosed schists, or silurian rocks are found. Underneath the basalt or volcanic rocks in Queensland, as well as at Ballarat, the deep leads occur. It is curious to note that the deep leads of Queensland contain tin as well as gold. Wherever the dark red patch appears indicating granite, tin may be expected to be found. The extraordinary richness of the tin deposits of Queensland and New South Wales will probably cause the immense granitic tracts of Western Australia to be thoroughly explored. The middle belt of metamorphic schists which occurs in South Australia is as well known for its extensive copper mines as the eastern belt is for its gold.

The Mining department of Victoria has established a high reputation for the general excellence of the geological maps it has produced. The last effort will reflect equal credit upon the officers employed upon it. The rocks are shown in a descending order, and are easily recognised by the distinguishing colours with which they are tinted. A system of lettering the face of the map has also been adopted, which will facilitate the rapid identification of the rocks. In general appearance the map will more closely resemble those prepared in Germany or France than those compiled in England. As already mentioned, the responsible and onerous task of reducing the mass of materials obtained from so many different sources, and embodying the results of so many months of patient investigation, in the new map, has been performed by Mr. R. Brough Smyth. Mr. A. Everett, a draughtsman employed in the Mining department, has been entrusted with the duty of colouring the map, and Mr. R. Shepherd has performed the difficult work of colouring it on stone.

NOTES

SIR SAMUEL and Lady Baker arrived at Cairo, last Sunday. All was well.

THE twenty-second session of the American Association for the Advancement of Science commenced its meetings at Portland, Maine, on Wednesday, 20th inst. Prof. Lovering, of Cambridge, is president for the year.

THE discovery is announced, from America, of another small planet, No. 133, by Prof. Watson, of the Ann Arbor Observatory.

THE session of the Iron and Steel Institute at Liége was brought to a close on Thursday, on which evening the King of the Belgians gave the members a grand reception at Brussels. There was an interesting discussion on Wednesday morning between Mr. Bulgenbach and Mr. Bell at the Institute, on the subject of the construction of high furnaces. Papers were read relative to various technical matters, and the President read a paper upon the extension of commercial relations with China. In the afternoon more than 450 excursionists paid a visit to the factory of Messrs. Cockerill at Seraing. Several speeches were made, and the visitors, who were most cordially received, remained four hours. It has been decided that the Congress should meet in 1874 in Philadelphia, and in 1875 in England. A very interesting paper was read at one of the meetings by M. Julien Deby, C.E., "On the Rise and Progress of the Iron and Steel Industries in Belgium," in which he said:—"We are very ignorant of the state of things in this country prior to the arrival of Julius Cæsar. Archæological discoveries of quite recent date, still unpublished, seem to indicate that at the period of the great Roman conqueror's invasion Iron had already been made in Belgium, while it was yet unknown to the inhabitants of the British Islands. The oldest records we have consist in vast deposits of cinder which cover many acres of ground, and are situated at Niew Rhode, between Louvain and Aerschot, in Brabant, as well as at Tessenderloo, in the Antwerp campagne, where they generally occupy the top of the many ferruginous hillocks of that region. Along with these accumulations of iron cinder are found flint arrow-heads and fragments of coarse pottery, characteristic of the earliest dawn of civilisation, and which must have belonged to the old pre-historic workers of these deposits. At a later period, and during the Roman dominion, iron was produced in very many places in Belgium. Immense heaps of cinder are to this day scattered in many parts of the country, and several of these are being profitably worked in the neighbouring blast furnaces."

THE meetings of the British Archæological Association at Sheffield were brought to a close on Saturday. The time has been spent by the members in visiting most of the places of archæological interest in the district during the day, and in listening to papers read in relation to the places visited, as well as on other subjects. On Wednesday night, at a *conversazione* in the Cutlers' Hall, Mr. R. N. Phillips read a paper on the "Manufacture of Hardware by Celts and Romans," illustrated by fine specimens in bronze of various degrees of advancement, a baked clay melting-pot, and a bronze ingot. He adduced evidence of mining and smelting by Romans, and stated their wood-smelted iron to be of unequalled malleability. He suggested that the Romans held Britain for the sake of its mineral wealth; their extensive beds of scoriæ in the Forest of Dean were still so rich in iron-stone that they were being re-smelted. Mr. T. Morgan read a paper on the "Earliest Tribes of Yorkshire," and Mr. Alfred Wallis one on the "Pre-historic Remains on the Derbyshire Borders."

AT the meeting of the Somersetshire Archæological and Natural History Society held at Wells last week Dr. Beddoe gave a brief sketch of the ethnological history of the county, and showed its bearings upon the physical aspect of the population at the present day. We learn from the paper that the people of the eastern half of the county have, on the whole, broader heads, lighter hair, and darker eyes than those of the western half. In all these respects the eastern men approach more to the ordinary English, the western to the Irish, standard. The mixed blooded inhabitants of the towns appear to be lighter as to both eyes and hair than the people of either division. The fair and handsome Frisian type is pretty common in the north of the county. In the hilly south-eastern region about Wincanton dark complexions and dark or even black hair attest the late and imperfect Saxonisation of the country; the same may be said of the Quantocks. About Minehead and Dunster, perhaps from the less fixity of the population induced by seafaring, there is more evidence of mixture of blood; and in Exmoor and in some villages of Mendip the narrow skull, prominent jaws, and bony frame of the Gaelic type and the Turanian oblique eye and pyramidal skull crop up.

DR. BELL PETTIGREW, F.R.S., has been appointed Lecturer on Physiology at the School of Medicine, Surgeons' Hall, Edinburgh.

THE Secretaries of Section C (Geology) of the British Association request the attention of authors to the rule requiring the early transmission of papers. In order that the work of the Organising Committee may be completed in time, all papers and reports, accompanied by abstracts, should be forwarded to the General Secretaries not later than September 4.

WE are indebted to Mr. G. Gore, F.R.S., for a copy of a reprint of an able article of his on the "National Importance of Scientific Research," which appeared in a recent number of the *Westminster Review*. We are glad to have the opportunity of drawing attention to Mr. Gore's paper, as it forcibly expresses the view we have so persistently advocated in our own columns. Mr. Gore, after showing that the pursuit of pure Science is rarely rewarded in this country, points out that it is the duty of the State to provide and pay for pure scientific research, for the following reasons:—"Because the results of such labour are indispensable to national welfare and progress; because the results are of immense value to the nation, and especially to the Government; because nearly the whole pecuniary benefit of it goes to the nation, and scarcely any to the discoverer; because research cannot be efficiently provided for by means of voluntary effort; and because there appears to be scarcely any other way (except by application of University revenues) in which discoverers can be satisfactorily paid for their labour." At present, as the writer states, the men paid the highest are not those who discover knowledge, but those who use and apply it. The reason for this apathy of the public as regards scientific work is, as Mr. Gore shows, clearly traceable to a widespread and lamentable ignorance of the nature and value of scientific inquiry. To diffuse natural knowledge among all classes of society is therefore a great duty at the present time.

The Philadelphians are hard at work preparing for their Centennial Exhibition to be held in 1876. 200*l*. each for the ten best designs for an appropriate building had been offered, and forty plans have now been sent in. The Centennial Commission having in charge the inauguration and conduct of the Great Exhibition, have already made most commendable progress. Committees from their number, having in charge special departments of the vast scheme, are in constant session, and the general outline of the work seems to have been fully developed. The site for the buildings used for the occasion has already been selected in Philadelphia's beautiful park, and the formal transfer of the ground by the city authorities to the control of the Centennial Commissioners took place, with the imposing ceremonies befitting the occasion, on July 4. The decoration of the ground for the purpose, the planting of shade trees, &c., will be taken in hand at once.

Among the appropriations made by the State of New York for the State Cabinet of Natural History are the following enumerations:—Hall of Natural History, cleaning, repairs, &c., 3,000 dols.; for the increase of the zoological collection, 1,000 dols.; assisting in arranging duplicate fossils and minerals for distribution, 1,500 dols.; salary of botanist, 1,500 dols.; for the use of the Cabinet of Natural History, 10,000 dols., making an aggregate of 17,000 dols. The Board of Regents of the University receive 6,500 dols.

The offer of free lodging in the Rudolphinum during the Exhibition at Vienna has been responded to by no fewer than 2,412 teachers. Of these 418 have been selected, viz.:—207 Austrians, 99 Germans, 36 Italians, 20 Englishmen, 14 Dutchmen, 13 Swedes, 12 Danes, 10 Swiss, 7 Russians, 3 Belgians, and 2 Spaniards.

The Committee appointed by the Birmingham Natural History and Microscopical Society to carry out the proposed Marine Excursion have, as nearly as possible, completed all the necessary arrangements. A yacht has been hired for six days, commencing Sept. 1, for a very moderate sum. Mr. A. W. Wills has made a large-sized dredge, which he has kindly presented to the Society. The small dredges belonging to the President and Mr. Wills will also be available for the excursion. With the view of rendering the dredging operations scientifically interesting and valuable, it is proposed to use a Miller-Casella thermometer with copper case, similar to those supplied for the *Porcupine* and *Lightning* expedition. Dredging operations, and the management of the yacht, will be entirely under the direction of the President and Mr. Wills, who will determine the hours of sailing and returning, the places to be visited, &c. &c. In addition to those made in the yacht, excursions to places of interest in the neighbourhood will be planned at intervals during the expedition. Very satisfactory arrangements have been made as to accommodation. The proposed excursion is an experiment which, if successful, may be repeated on a larger scale at some future time.

The United States screw steamer *Juniata*, of 828 tons burden, left New York on the 24th of June, bound to Greenland, on her mission of rescue to the crew of the *Polaris*. She is in charge of Commander Braine, with a picked crew, and has been fitted out with every appliance needed for the success of her object. She reached St. Johns, Newfoundland, on June 30, and immediately went into the dock for the purpose of being properly sheathed with iron, and otherwise strengthened and refitted. As soon as this was completed she left for Disco, on July 9, where, or at Upernavik, she will wait until the arrival of her consort, the *Tigress*. The *Tigress*, it will be remembered, is the Newfoundland sealing steamer which rescued a part of the crew of the *Polaris* from the ice, and was purchased by the Secretary of the Navy as a relief vessel for the remainder of the party, as being better fitted for this end than any vessel that could be properly prepared in time for departure during the present summer. She reached New York on June 28, and was immediately examined by proper officers of the navy, who decided at once what alterations and repairs to put upon her. The *Tigress* is 165 ft. in length, has 28 ft. breadth of beam, and 16 ft. depth of hold, draws 13 ft. of water, and has a capacity of 463 tons. She has been placed under Commander Greer, lately of the Naval Academy, and is accompanied by Captain Tyson, late of the *Polaris*, as ice-master. The *Tigress* left Brooklyn on July 14, and arrived at St. Johns on July 23, where, like the *Juniata*, she will take in additional supplies, and then proceed northward. She is prepared to remain two years in the North if necessary, although it is hoped that she will return during the present season, convoying the *Polaris*.

The second annual report of the Board of Commissioners of the Department of Public Parks in New York, is partly devoted to the condition of the Menagerie in Central Park, which has increased considerably in size during the last year. A catalogue is appended of the animals contained in the collection, which is on exactly the same plan as Mr. Sclater's carefully constructed List of Animals in the Zoological Society's Gardens in Regent's Park.

In Part V. of Dr. Brown-Sequard's new "Archives of Scientific and Practical Medicine," there is an excellent analysis of some of the recent researches on the localisation of the cerebral functions, including an account of the experiments of Nothnagel, Gudden, and others. We hope next week to be able to give an abstract of the paper.

The death of the Rev. Peter John de Smet, of the Society of Jesus, is announced as having taken place at St. Louis on May 23—an event which is worthy to be noted in a scientific point of view. Although not himself a special student of natural science, numerous collections made at his request and under his direction, and transmitted to museums at home and abroad, have borne witness to his tastes; and it is even stated that he has left behind him a manuscript record of his life, in which are embraced important notes of the habits and customs of the Indian tribes of the West, and of the physical condition and natural history of the regions inhabited by them.

The Fourth Part of the illustrated work by Mr. Hermann Strecker, of Reading, Pennsylvania, upon the Lepidoptera has just been published, and contains figures and descriptions of quite a large number of species, illustrated by one plate. Among other species is included a new butterfly (*Satyrus hoffmanni*), obtained by Dr. Hoffmann at Owen's Lake, in Nevada.

The *Journal of the Society of Arts* for August 22 contains a report on steel as represented at the International Exhibition, by Mr. William Baker.

A letter appears in the *Times* of Tuesday, from Mr. Richard Potter, one of the party from Mr. Leigh Smith's Arctic Expedition, by the Spitzbergen route. It is dated Trenerenberg Bay, July 4, and says:—"The *Polhem* came in here last night, and is going away again to-day. She is going home in about three weeks, I believe. We fell in with the *Samson* two days ago. We have been up to the Seven Islands, lat. 80° 50′, but there is too much ice to go farther North at present. Prof. Nordenskiold and the other men who tried to get North in boats could not get farther than 80° 35′ lat., and then, finding the ice too rough for sledging, crossed the north-east land, and returned by Hinlopen Straits. They must have had a bad time of it, as there were snowstorms fifty out of sixty days. The bay where we are now is where Parry left the *Hecla* when he went North on sledges. It is anything but a fertile place, as the low ground is all one great swamp, and there is a lot of snow on the ground still. We are going to stop here to take in water, and to get the provisions and coals out of the *Samson*."

THE additions to the Zoological Society's Gardens during the last week include a Naked-footed Owl (*Athene noctua*), European, an Egyptian Vulture (*Neophron percnopterus*), and two Buzzards (*Buteo tachardus*), from Africa, presented by Mr. S. G. Reid and Lieut. Denison; a Golden Eagle (*Aquila chrysaetus*), European, presented by Mr. A. W. Tait; a Paradoxure (*Paradoxurus typus*) from India, presented by Mr. A. F. Adey; a Mantchurian Crane (*Grus montignesia*) from N. China; a Wild Pig (*Sus scrofa*) from N. Africa; three Common Guillemots (*Uria troile*), British; a White-backed Piping Crow (*Gymnorhina leuconota*) from Australia, deposited, and four Gambel's Partridges (*Callipepla gambelii*) hatched in the Gardens.

SCIENTIFIC SERIALS

Der Naturforscher for July 1873, contains, among other interesting matter, an account of observations by Herr Nageli, among plants in Alpine regions, as to the production of closely-related plant forms. He is led to conclude, (in opposition to the common view), that association is more favourable to the formation of species, than isolation. There are also botanical papers on the assimilation of air-plants under water, and the opening and closing of flowers. In physics and chemistry we have M. Amagat's recent important experiments on the expansion and compressibility of gases, and those of Troost and Hautefeuille on isomeric and allotropic transformations; a notice of M. Bichat's investigation of the influence of aggregate state on magnetic rotatory power, &c. M. Bichat has ascertained a decrease of this power as temperature rises, and entire disappearance of it in the state of vapour. Some striking facts with regard to the meteorological differences between northern and southern hemispheres are from a paper by Prof. Dove to the Berlin Academy. In physiology there are notes on the place of decomposition of albumen in animal bodies, and on the significance of common salt in the animal economy. Astronomy and technology are also represented, and there is a good selection of *Kleinere Mittheilungen*.

THE current number of the *Ibis* commences with an article on the "Ornithology of Sardinia," by Mr. A. B. Brook, which is one of a series on that subject. The part before us includes the Woodpeckers, their allies, the Swifts, and some Passerine birds, among which are *Melizophilus sardus*, *Bradypterus cetti*, and the Corvine birds. Mr. R. Swinhoe describes the habits and plumage of the Rosy Ibis of China and Japan (*Ibis nippon*). He also notes points in its visceral anatomy, comparing them with the corresponding structures in the common Heron, in order to show that the affinities supposed by some to exist between the two birds are but slight. An editorial note verifies the conclusion that the Ibis and Spoonbill are intimately related, and differs justly from the author's conjecture that the former bird is related to *Tantalus*, which is a true Stork.—Mr. J. H. Gurney gives a tenth additional list of birds from Natal, including several species from the rich collection of Mr. R. B. Sharpe. Mr. J. E. Harting contributes a paper on *Charadrius pecuarius* of Temminck, in which it is shown that this bird is the smaller of the two allied species inhabiting Africa, but not found in St. Helena, and that the St. Helena species, till now unnamed, is distinct (*Aegialites sanctæ helenæ*, Harting). Vieillot's name, *Ch. varius*, must also take precedence of Temminck's *Ch. pecuarius* An illustration is given of each of the birds referred to.—Messrs. Salvin and Elliot, in continuation of their notes on the *Trochilidæ*, discuss the genera *Pygmornis*, *Glaucis*, and *Threnetes*, separating the first into three groups, from the second removing *G. dohrnii* to the genus Grypus, as already suspected by Mr. Gould, and adding *Glaucis ruckeri* to the third. The same ornithologists help to clear the synonomy of *Lophornis gouldi* by naming *L. regina* of Gould, *L. stictolophus*.—Mr. T. Ayres continues his notes on birds in the republic of Trans-Vaal, and Mr. G. N. Lawrence on the Cuckoos of the genus *Neomorphus* defines precisely *N. geoffroyi*, *N. salvini*, *N. rufipennis*, and *N. pucherani*, showing that the specific validity of the last-named has been questioned by several distinguished ornithologists; though some time ago, Mr. Sclater, on seeing the type-specimen, was convinced of its being an excellent species.—Mr. Salvin figures the typical specimen of *Fulica alba* of White, showing that it is evidently of the genus *Notornis*, as pointed out by Herr von Pelzeln.—The Viscount Walden, P.Z.S., describes, as the last paper, a collection of birds from the Andaman Islands, made by Lieut. R. W. Ramsay; figuring *Centrococcyx andamanensis*, *Kittacincla albiventris*, *Sturnia andamanensis* and *Janthænas columboides*, also entering into detail with reference to *Spilornis elgini*.

SOCIETIES AND ACADEMIES

RIGA

Society of Naturalists, March 5.—Dr. Petzholdt concluded a series of five lectures on Turkestan, having described the fauna and flora, ethnographical features, dwellings, manners and customs, state of agriculture, mining and manufacture, &c. He commends the mode of treating silkworms as superior to that in Europe, and thinks the system of irrigation more perfect than in any other land not having scientific appliances. The Russian portion of Tasckkent, it is stated, has now a good chemical laboratory.

The *Correspondenzblatt* (No. 6) contains a note on uncommon forms of hair-growth, with reference to two Russian peasants exhibited before the Society in December.

March 19.—Herr Berg gave an account of his excursion to Kurland, and the plants and mollusca he met with.

March 26.—Dr. Nauck described an electrical experiment. A funnel with leather bag at the end is placed in a long glass cylinder, and has mercury poured into it. The liquid streams through the pores against the glass sides, and runs down. The lower part of the cylinder and the mercury in it are found positively electric, while the upper part and the funnel with its mercury are negative. The limit between positive and negative, after some variation, divides the cylinder into two parts, of which the lower is double the upper.

April 2.—Dr. Schell reported on the present arrangement of the meteorological station of Riga, and on observations of the water-mark at Riga and at Duna mouth in 1872.

BOOKS RECEIVED

FOREIGN.—Remarks on Synonyms of European Spiders: Prof T. Thorell (Upsala).—Lehrbuch der Physik, Dritte Lieferung: Dr Paul Reis (Leipzig).

ENGLISH.—Gateway to the Polynia, a Voyage to Spitzbergen, from the Journal of J C Wells, R.N (H S King & Co.)—Sound and Music Sedley Taylor, M A. (Macmillan & Co)—Echoes from distant Footfalls: Rev J. Boyes (Hodder & Stoughton).—Man a special Creation: William Sharpe, M D (R Hardwicke)—Introduction to Physical Measurements Dr F Kohlrausch (J & A. Churchill)—Mitchell's Manual of Practical Assaying, Edited by Wm Crookes (Longmans & Co)—Descriptive Sociology. Classified and arranged by Herbert Spencer (Williams & Norgate)—Introductory Text Book of Geology: David Page, LL D. (W. Blackwood & Sons).—Advanced Text Book of Physical Geography. David Page, LL D. (W. Blackwood & Sons)—Half-hours in the Green Lanes J E. Taylor, F Z.S (R. Hardwicke)—The African Sketch Book: Winwood Reade (Smith, Elder & Co)—Lacerda's Journey to Cazembe in 1798, Translated by Capt R. F. Burton (J Murray)—Elements of Mineralogy: James Nicol, F.R S E (A & C Black)—Harveian Oration, 1873: G Rolleston, M D, F R.S (Macmillan & Co.)—Researches in Zoology, 2nd edition: John Blackwall, F L S (J Van Voorst)

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THURSDAY, SEPTEMBER 4, 1873

THE TESTIMONIAL TO MR. COLE

AS was to be expected, the subscriptions for the well-deserved testimonial to Mr. Cole, to which we have already referred, have so far been thoroughly satisfactory, upwards of 2,000*l.* having already been subscribed. Among the names of the subscribers will be noticed the names of men eminent in nearly every department of human activity. Thus we see Dr. De La Rue, Mr. Brassey, Mr. Baines, M.P., Messrs. Clowes and Son, Elkington and Co., Prof. Ella, Mr. C. J. Freake, Lord Ronald L. Gower, Sir Francis Grant, Earl Granville, Messrs. S. C. Hall, Hawkshaw, Hawksley, Lord Houghton, Messrs. H. A. Hunt, C.B., Jackson and Graham, John Kelk, Longmans, J. E. Millais, Lord C. Paget, Sir A. de Rothschild, Sir Titus Salt, Duke of Sutherland, Messrs. G. Trollope and Sons, Sir Richard Wallace, Dr. J. F. Watson, Marquis of Westminster, Sir Joseph Whitworth, &c. &c. We may well hope that ere the list be closed many more names will be added, and such a sum subscribed as will render possible a testimonial worthy of the services performed by Mr. Cole to all the best interests of this country.

The earliest work which can be considered to have a connection with Science undertaken by Mr. Cole, was the reform of the Patent Laws, which he advocated in 1850, afterwards inducing the Society of Arts to take up the subject. He wrote three Reports, and the principles which he laid down have been generally adopted as the basis of the present law. He particularly insisted upon the principle of a moderate fee at the first registration of an invention, such payment to increase at the option of the inventor in after years. He denounced all "taxes on inventions," as such, and public opinion is now beginning to go with him. Successive Governments have received hundreds of thousands of pounds from this source, and still withhold all proper aid to the encouragement of Science. There is a spice of sarcasm in the adage which has been worked in Sgraffito on the back wall of the new Science Schools, "Scientia non habet inimicum nisi ignorantem."

In 1852 Mr. Cole reformed, or we may almost say, established, the system of Art Schools, making it possible for every locality to have its Art School if it pleased. In 1853 the Department of Art was made Department of Science and Art, and Dr. Playfair was appointed to organise the Science division; but he shortly afterwards resigned his post, and became Professor of Chemistry at Edinburgh. Mr. Cole then became sole Secretary for Science and Art. The late Marquis of Salisbury was the Lord President, and doubtless to the great interest which this nobleman took in all matters appertaining to Science is to be ascribed some of the success with which Mr. Lowe was enabled to ventilate and carry out his views. Captain Donnelly, R.E., was invited to enter the Department, and through the instrumentality of Lord Salisbury, Mr. Cole, and Captain Donnelly the present Government system of scientific instruction throughout the country, one of the things of which England has the greatest reason to be proud, was evolved; and through the admirable harmony existing between Major Donnelly and Mr. Cole the work has been brought to its present flourishing condition. In 1856 there were 16 Science schools, in 1872 there were 1,238. This is one part of the work which Mr. Cole has done for English Science, and we blush to think that it has not been appreciated by men of Science as it ought to be and as it will be appreciated.

The Report which has just been issued by the Science and Art Department as to the attendance in the various classes connected with it, and the number of visitors to the various museums during 1872, will give some idea of the magnitude of the work accomplished by Mr. Cole.

The number of persons who have during the year 1872 attended the Schools and classes of Science and Art in connection with the Science and Art Department is as follows: viz. 36,783 attending Science Schools and Classes in 1872, as against 38,015 in 1871, and 244,134 receiving instruction in Art, showing an increase on the previous year of 31,633, or nearly 15 per cent. At the Royal School of Mines there were 20 regular and 148 occasional, students; at the Royal College of Chemistry, 212 students; at the Metallurgical Laboratory, 30; at the Royal School of Naval Architecture there were 35. At the Royal College of Science for Ireland there were 20 associate or regular students, and 19 occasional students. The lectures delivered in the lecture theatre of the South Kensington Museum were attended by 11,958 persons, or 2,927 more than in 1871. The evening lectures to working men at the Royal School of Mines were attended by 2,400 persons; and 186 Science teachers attended the special course of lectures provided for their instruction in the new Science Schools at South Kensington. The various courses of lectures delivered in connection with the Department in Dublin were attended by 2,577 persons; and at the evening popular lectures, which were given in the Edinburgh Museum of Science and Art during the Session of 1871-2, there was an attendance of 1,416. The total number of persons, therefore, who received direct instruction as students, or by means of lectures, in connection with the Science and Art Department in 1872, is nearly 299,000, showing an increase as compared with the number in the previous year of 88,000 or 10 per cent. The museums and collections under the superintendence of the Department in London, Dublin, and Edinburgh, were last year visited by upwards of 2,922,000 persons, showing the very considerable increase of 1,141,000, or about 63 per cent. on the number in 1871. The returns received of the number of visitors at the Local Art and Industrial Exhibitions, to which objects were contributed from the South Kensington Museum, show an attendance of upwards of 574,000. The total number of separate attendances during the year 1872, as shown by the returns of the different Institutions and Exhibitions, in connection with the Department, has been upwards of 3,795,000. This total, compared with that of the previous year, presents an increase of 1,117,000, or 53 per cent., not including the number of visitors at local exhibitions, which was exceptionally augmented last year by the attendance of 420,000 at the Dublin Exhibition of Art and Industry, and is necessarily liable to much fluctuation from year to year.

We regret extremely to see that part of the great work done by Mr. Cole, in establishing the South Kensington Museum, runs some risk of being undone by the unintelligent intermeddling of Government. It would appear from statements recently made in the House of Commons that arrangements were being made for transferring the management of the South Kensington, Bethnal Green, and similar institutions to the trustees of the British Museum. It is difficult for an outsider to see what Government means by contemplating such a step; we believe no better means could be taken to cripple the efficiency of such institutions than by giving them over to the irresponsible management of the unpaid trustees of the British Museum, who have at present much work on their hands, which is the subject of constant Parliamentary inquiry. We cannot conceive that Mr. Cole would approve of any such step, a step which, we repeat, would be sure to mar the great work which, with untiring labour, all-conquering zeal, and advanced intelligence, he has accomplished. Report indeed has reached us that a National Committee is being formed to urge upon Mr. Gladstone's re-constituted Government the necessity of putting the British Museum, the National Gallery, and Institutions supported by Parliamentary funds, and now Trustee-muddled, under the direct control of a responsible Minister.

Sir Joseph Whitworth consulted Mr. Cole upon the establishment of Scholarships for Mechanical Science, to take place after his death. Mr. Cole recommended him to establish them during his life, so that he might have the enjoyment of watching the progress of them. Sir Joseph followed this recommendation, and presented the country with 3,000*l.* a year for these Scholarships.

Mr. Cole is now devoting special attention to the application of Science to Productive Industry in the yearly International Exhibitions, and we trust that he may long be spared to reap the honour which is his due and to help on the work of which he has laid the foundation.

The erection in Exhibition Road of the handsome Science Schools, one of the few buildings devoted to Science of which the country may be justly proud, which Mr. Cole has at length successfully achieved, is due solely to the persistency of his efforts, rendered more and more pertinacious by the obstinacy and penuriousness of the Treasury, which in the most niggardly spirit is still starving the work and preventing its proper development, simply because, we presume, it is a scientific work; and it was the intention of the recent Chancellor, Mr. Lowe, that in this particular England should be distanced by the smallest Continental or American state. It is fair to add that Mr. Cole was supported in this particular direction by the Duke of Buckingham, the Duke of Marlborough, and the Marquis of Ripon, who have successively been Lord Presidents since 1866.

ADVANCED TEXT-BOOK OF PHYSICAL GEOGRAPHY

Advanced Text-Book of Physical Geography. By David Page, LL.D., F.G.S., Professor of Geology in the College of Physical Science, Newcastle. Second and Enlarged Ed. (Edinburgh and London: Blackwood, 1873.)

PHYSICAL Geography is one of those branches of knowledge which, without being a science in itself, makes use of many of the Sciences to explain and illustrate the facts and phenomena with which it deals. So far as it is confined to the mere knowledge of facts and description of natural phenomena, no special acquaintance with any science is required; but when it comes to deal with the causes of phenomena and the deductions from geographical facts, it is essential that the teacher should himself possess a good general knowledge of several branches of modern Science. In particular it is necessary that he should clearly grasp the main principles of Physics, that he should have a good acquaintance with the distribution of animals and plants, and so much familiarity with arithmetic and mathematics as to be able to avoid making statements which are palpably incorrect.

After a careful examination of the present volume, we are forced to conclude that the author is, on all the above-mentioned points, unfitted to teach this particular subject. It is with much regret that we say this, having expected something very different, not only from the popularity of Prof. Page as an author and a teacher, but also from the criticism of one of our first literary periodicals (used as an advertisement), that the work is "a thoroughly good text-book of Physical Geography." In order to justify this difference of opinion from so high an authority, it will be necessary to point out what are the most prominent errors and defects in the volume. Some of these defects may, it is true, be mere oversights; but most persons will be of opinion that, in the second edition of an educational work, the plea of "oversight" can hardly be allowed.

In the second chapter—on the figure, motion, and dimensions of the earth—we find a series of curious misconceptions, blunders, or obscurities. At page 19 we have the globe "revolving and rotating in obedience to the laws of gravitation and attraction," and in the next page these words are again used as implying distinct "forces." On page 21 occurs the following:—"But day and night are of unequal and varying length according to the *seasons*; and these seasonal successions are caused by the facts—first, that the orbit or path of the earth's revolution round the sun is not a perfect circle, but an ellipse; and second, that in performing this revolution her axis is not perpendicular, but inclined at an angle of 66° 27½′ to the plane of her orbit." This is simply absurd. The ellipticity of the earth's orbit has nothing whatever to do with the fact of there being seasons, which would occur exactly the same were the orbit a perfect circle. The actual effect of the elliptic orbit in slightly modifying the length and severity of winter in the two hemispheres, and which is of some importance as being an element in explanation of the cause of the glacial epoch, is never so much as alluded to. In a recent public examination some of the competitors gave this very account of the seasons, and received few or no marks in consequence. They had probably got up the subject from Dr. Page's volume. Three pages further we have a table of certain dimensions of the planets. This has no particular bearing on physical geography, but as it is given it should have been correct. It is, however, full of gross blunders, which can be detected by observation alone. We have in three columns—the diameter in miles, the cubic contents in miles, and the volume, earth being taken as 1. Now the "solid contents" and the "volume" being the same dimension expressed in different ways, must be proportionate in any two planets; yet we have Mercury, volume 0·06, solid contents 10·195; Venus, volume 0·96, solid contents 223·521, so that while the volume of Venus is 16 times that of Mercury, its solid content is 22 times! Again: Earth, volume 1·00, solid content 260·775; Mars, volume 0·14, solid content 48·723, the earth being over 7 times the volume of Mars, but only 5⅓ times its solid content. Almost any other two planets come out equally wrong. Again, from the diameters given the solid contents can be easily calculated, but here again is frequent error; and to add to the confusion, in at least two cases the diameters are seriously wrong (4,980 miles instead of 4,100 for Mars, for instance), so that it is very difficult to understand where so many mistakes could have come from. On the next page we have a contradiction as to the earth's internal structure. It is first stated positively that "the interior of the earth cannot be composed of the same materials that constitute its outer portion," and lower down, that "either the interior of the earth is composed of materials differing altogether from those known at the surface, or the compression must be counteracted," &c. At page 27 we have the atmosphere described as "mainly composed of two gases, nitrogen and oxygen—79 parts of the former to 21 of the latter—with a small percentage of carbonic acid and other extraneous impurities." Considering the importance of the carbonic acid gas in the atmosphere, it is hardly instructive to class it as an "extraneous impurity."

Passing over the mere description of the earth's surface, parts of which are very well done, we find other objectionable matter as soon as we have to deal with the explanation of phenomena. A mountain range is said at p. 75 to be "not a simple upheaval, the result of one paroxysmal outburst, but the work of innumerable volcanoes and earthquakes operating through ages and subsequently escarped and chiselled by rains, frosts," &c. Here gradual elevation without volcanoes or earthquakes, and possibly from altogether different causes, is ignored. On the next page, speaking of circumdenudation, we have:—"A mountain may thus consist of stratified rocks and be wholly unconnected with any forces of upheaval or ejection from below." Here ignoring that the strata must be upheaved before they can be circumdenuded. These are perhaps slight matters, but we think an introductory work should not adduce the almost exploded theory of Elie de Beaumont on the parallelism of mountain chains of the same age, "even when in opposite hemispheres," as if it were generally admitted, or Prof. Hopkins' explanation of central mountains with diverging spurs as the result of an upheaving force acting on a point, without stating that a very different explanation of the facts is adopted by most modern geologists.

When we come to the subject of the ocean, involving many nice problems in physics, our author is again altogether at fault. It seems hardly credible that he should not know the difference between salt and fresh water as regards the point of maximum density, on which much of the theory of oceanic circulation and temperature depends; yet such seems to be the case. At p. 123 we are told that "at 40° Fahr. water is at its minimum volume and maximum density," and again in the same page—"Its maximum density or minimum volume at 39¼, its expansion as ice to one-ninth of its bulk at 32° for fresh water and at 28¼ or less for salt water." Again, at p. 131 we have—"As already mentioned, water acquires its minimum volume or greatest density at a temperature of 40°, and becomes lighter as it rises above or falls below this temperature. Owing to this property a perpetual interchange or circulation is kept up among the waters of the ocean," proving that sea-water also is supposed by the writer to have this property, instead of increasing in density down to about 27⅓°, as it actually does. Yet the author quotes Maury, who published this correction of the old notion in 1861, and the papers of Dr. Carpenter, who repeatedly refers to this fact as a most important one. Again, at p. 136 we have the obsolete theory of Sir James Ross as to deep-sea temperatures given in full, with a remark that it has recently "been materially interfered with" by the experiment of Drs. Carpenter and Wyville Thomson; but, without, apparently, any acquaintance with the whole of the facts established by those gentlemen, as shown by again referring to the temperature of the bottom of the ocean as being 39° Fahr., "that of its maximum density."

It is perhaps a small matter that, in describing the Nile valley, Capt. Speke's account is quoted at length (p. 181), and the Victoria Nyanza given as the source, the Albert Nyanza not being once mentioned, or any allusion whatever made to the fact that Sir Samuel Baker claims it to be the true source of the Nile; but it is of great importance that the student should be impressed with clear and accurate ideas as to the cause of winds. Yet we find here the old school-book notion of a vaccum and an inrush to fill it up. "As air is expanded by heat and contracted by cold the warmer and lighter volumes will ascend, and the colder and denser rush in from all sides to supply the vacancy" (p. 205). "The air of the torrid zone becomes rarefied and ascends, while the colder and denser air sets in from either side to supply the deficiency" (p. 213). And the same words are repeated at p. 243. But every physicist knows that there is no "vacancy" and no "deficiency" in the case, but merely a disturbance of equilibrium; and unless this is clearly comprehended the causes and effects of atmospheric currents can never be understood. On the subject of light and heat the ideas of the author appear to be still more confused. At p. 205 he says—"As the atmosphere is the medium through which the sun's heat is conveyed to and disseminated over the earth, so also it is the medium of his light-giving rays." This sentence will certainly convey to the learner the false notion that the atmosphere is in some way essential to the "conveyance" of light and heat from the sun to the earth; and this is further dilated upon in the following vague and unintelligible, if not erroneous sentence:—"Heat and light are alike indispensable to plants and animals, and, from the peculiar constitution of the atmosphere, as regards its varying density, moisture, &c., both are reflected and diffused so as to become most available to vegetable and animal life." The learner must be very acute who can obtain any definite information from such oracular teaching as this. Again (at p. 207) we have a total misconception as to the cause of the decrease of temperature at increasing elevations—"The heat that falls on the land being partly absorbed and partly radiated into the atmosphere, the lowest aërial strata or those nearest the influence of this radiation will be warmer than those at higher elevations." But it is a thoroughly well-established fact that the atmosphere is scarcely at all warmed by radiant heat, except when charged with vapour, but almost wholly by contact with the heated earth, and that the diminution of temperature upwards is due to the cooling by expansion of the air which rises from below, and to its greater diathermacy, owing to the comparatively small amount of vapour at great elevations. In the whole of this part of the book there is no allusion to the effect of atmospheric vapour in checking radiation, so that the learner is left without a clue to the comprehension of some of the most important and interesting facts in climatology.

The latter division of the volume treats of the distribution of life, but it deals chiefly in vague generalities, and shows little acquaintance with the large amount of research which has of late years been bestowed on this subject. The distribution of plants is illustrated by means of the eight zones, from equatorial to polar; and there is no hint to the student that this is not a natural system or that there are any other causes than climate, soil, and altitude that determine the flora of a region. Here, too, we are not free from absurd errors, such as rhododendron and azalea being given as characteristic of the "American Arctic zone," while "box, saxifrage, and gum" (!) are said to grow up to 4,200 ft. on the Pyrenees, and "rice and wheat" in "those provinces subject to the influence of tropical seasons!" (p. 257). Animal life is treated in an equally loose and obsolete fashion. We find such terms as "homoiozoic zones" and "latitudinal distribution" repeated *ad nauseam*, but in illustration of these the student is told that the opossum is peculiar to the north temperate zone, and the kangaroo to the southern, apparently in complete ignorance that opossums abound all through tropical South America, while kangaroos inhabit tropical Australia and equatorial New Guinea, as well as the more temperate regions. "The eagle and falcon" are also given as peculiar to the temperate zone, while "the wolf" is said to be peculiarly arctic (p. 261). We are next informed that—"it has been attempted to arrange the earth's surface into certain zoological kingdoms and provinces, but it must be confessed with much less precision and certainty than in the case of the vegetable world"—which is exactly the reverse of the fact,—and then we have the now obsolete arrangement of Edward Forbes put forth, without a word about the labours of Sclater, Günther, Murray, Blyth, Blandford, Huxley, and others, who have established what all agree are natural zoological divisions of the earth (which has not yet been done in botany), although they may still differ as to the comparative rank of those divisions. We are not therefore much surprised when (at p. 263) we are told that in the Moluccas and Timor "there is a great abundance of carnivora and other orders of animals (!)" or that we have (at p. 269) the entirely novel assertion that "on the introduction of some new exotic, animals hitherto unknown in that locality usually make their appearance." Having perhaps read or heard of Mr. Darwin's celebrated case of the heartsease, bees, mice, and cats ("Origin of Species," 6th ed., p. 57), Dr. Page holds forth as follows:—"Certain birds, for example, feed on certain insects, and these insects again find their chosen food in certain plants; remove the plants and you destroy the insects, and by the destruction of the insects you compel the birds to remove and find other habitats, or if these supplies cannot be found the birds are extirpated." Mr. Darwin gives a possible and very probable case founded on careful observation, but here we have a very improbable, if not impossible case, founded on imagination; because no birds feed on "certain"—that is definite species of—insects only, and comparatively few insects again are restricted to certain definite species of plants, so that there is no reason to believe that any insectivorous bird could ever be extirpated, or even rendered scarce, by the destruction of a single species of plant with the insects that feed upon it.

Next we come to the subject of mankind with the inevitable five races of Blumenbach, no notice whatever being taken of more modern classifications. Thus, the hill-tribes of India are left with the Caucasians, and the New Zealanders, Papuans, Australians, and Malays, are all jumbled together as forming one race. In the concluding chapter, which is a kind of summary of the whole work, we find it stated that the new world is characterised by more "uniformity of vegetable and animal life" than Europe, the exact contrary being the case; that "the vegetation of Africa is much less varied than that of Europe or Asia," which is equally untrue as regards Europe; the Cape of Good Hope alone equalling it in the number of families and genera of plants, while the difference between its northern and southern extremities is far greater than any corresponding difference in Europe; and, that the Polynesians are "utterly uncivilised." Having now gone through the book, we find that several classes of earth-knowledge have been totally omitted. The great subjects of terrestrial magnetism and atmospheric electricity are altogether ignored, while such phenomena as the rainbow, the blue sky, and meteoric stones, are never once mentioned.

The great and radical defects which have now been pointed out are not however the only ones, although they are by far the most important. The work is carelessly written, and the author seems not to have thought it worth while, even in a second edition, to correct errors, erase repetitions, or make sentences intelligible. A passage is repeated word for word about the middle and near the bottom of p. 27. "Contour" and "vertical relief" are defined in almost the same words three times over at pp. 62, 66 and 72. The two first lines on p. 21 are unintelligible, owing to some omitted words; and the second line of p. 28 is palpably ungrammatical. These, however, are small matters, and would not have been noticed had the author carried out with any approach to completeness and accuracy his somewhat lofty pretensions. He tells us that it is his object to "present an outline of the science in its higher bearings," to rise above mere external appearances, and seek to explain the causes that produce them, and that "he has endeavoured to embrace all that is important in recent discovery and hypothesis." The numerous quotations and references now given will enable the reader to judge how far the opinion expressed at the commencement of this article is well founded, and, if they agree with that opinion, they will feel some indignation that periodicals of high standing should (through ignorance or something worse), mislead the public so far as to tell them that this is "a thoroughly good text-book of Physical Geography." (!) This is the more to be regretted, as there are two well-known works to which the epithet is fairly applicable, and which are at least free from such erroneous facts and false or exploded theories as have been pointed out in Dr. Page's volume.

ALFRED R. WALLACE

OUR BOOK SHELF

Half-hours in the Green Lanes: a Book for a Country Stroll. By J. E. Taylor. (Hardwicke.)

THERE are two ways at least in which the first principles of Natural Science may be taught to the youthful mind, as well as to "intelligent people who have not had time to enter into the technicalities of scientific questions." One which, if we may judge from the number of elementary works on Physics in which it is adopted, has many arguments in its favour, consists in the careful and logical working out in detail of a few of the most important principles of the Science, together with the different steps by which they were arrived at; the knowledge of minutiæ being left for future observation and study, on the foundation supplied: and the other is little more than a compilation of disconnected facts, of unequal importance, arranged with an endeavour to make them impressive from their almost endless number, and strung together with teleological argument. The tenants of the "tarns and green lanes being the objects treated of, there is an expanded field for the 300 or so short pages, in which the fishes, molluscs, and reptiles of the former, as well as the birds, insects, and plants of the latter, are rapidly passed in review. Several excellent figures illustrate the work, Mr. Wood and Mr. Keulemans contributing to the ornithological section; however, we are surprised to see so many on subjects of comparatively little importance, as the 14 on the slight variations in the shape and marking of cycloid scales, and the 32 on the different species of snails. Turning to the letterpress, many of the descriptions will be found to be accurate and clear, and a few sufficiently long to enable the uninitiated to form a fair idea of the subject. Many however are so short and incomplete that but little can be made of them without extraneous assistance, and in some the carelessness in the choice of words adds to the difficulty, as where the Vapourer Moth (*Orgyia antiqua*) is said to derive its name "from the habit of the winged males rising and falling simultaneously in their flight." A fact is sometimes stretched to make a *simile*, as when we are inaccurately told that "the generic name of the Kingfisher (*Halcyon*) is derived from the ancient belief that when it was hatching its eggs, the water was always calm and still." The genus *Turdus* is more than once called *Tardus*, and several other mistakes show that the author's knowledge of the subject is not of the deepest, as when the hind wing of the Clifden Nonpareil (*Catocala fraxini*) is said to be black and red, and the wide geographical distribution of the Kingfisher is given as a reason for supposing that it has a comparatively high geological antiquity. Notwithstanding its faults, however, there are many points in this small work which will make it of more than ordinary interest to the general reader.

The Royal Readers. Nos. 1 to 6. (Nelson and Sons. London and Edinburgh.)

THE excellence of these reading books and their adaptation to the broader culture of the present day demand from us some notice. The editor of the series, who has done his work with unusual ability, tells us in the preface that his aim has been to cultivate the *love* of reading. So far as we are able to judge, this aim he has successfully carried out by presenting interesting subjects in an attractive way. Opening any one of these Readers, we are struck with the air of freshness and interest it possesses. An intelligent child, instead of closing the book with relief, is far more likely to leave it with regret. And added to the happy way in which the lessons have been prepared, the pages abound with capital woodcuts, some of which are of real beauty. There are none of the stereotyped cuts of stale children in old-fashioned dresses and hair in pig-tails, primly grouped at play, and supposed to illustrate the story of the goody-goody girl, or the naughty-naughty boy. Our children are mercifully spared from these haunting ghosts of our childhood and have their Royal Readers instead. But these books have a wider scope than mere reading lessons. In the fifth and sixth books we find a large amount of sound scientific knowledge conveyed in a course of lessons carefully prepared by the editor. Then there are articles on physical geography, the bed of the sea, the various ocean routes, and lessons on useful inventions, besides some other novel features which we have not room to detail. The employment of these reading books will certainly tend to create a love for healthy reading, and at the same time they seem likely to be of the highest service in training and furnishing the minds of children.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Atoms and Ether

I AM not enough of a metaphysician to say whether a substance which can be compressed and expanded *necessarily* contains void spaces.

If so, the idea of air, furnished to a beginner by instruction in "Boyle's Law," is self-contradictory; and any molecular theory afterwards developed in order to account for "Boyle's Law," may claim not only ingenuity but necessity in order to abate a crying grievance to all right-minded persons.

I do not myself believe in Prof. Challis's æther, but at the same time I do not believe in the power of the human mind to pronounce that a continuous medium capable of being compressed is an impossibility.

But, on the other hand, I am sure that a medium consisting of molecules is essentially viscous; that is, any motions on a large scale which exist in it are always being converted into molecular agitation, otherwise called heat, so that every molecular medium is the seat of the dissipation of energy, and is getting hotter at the expense of the motions which it transmits. Hence no perfect fluid can be molecular. So far as I can see, Prof. Challis intends his æther to be a perfect fluid, and therefore continuous (see p. 16 of his Essay), though he does not himself pronounce upon its intimate constitution.

Hansemann* makes his æther molecular, and in fact a gas with the molecules immensely diminished in size.

With regard to Mr. Mott's iron bar, when he pulls one end he diminishes, in some unknown way, the pressure between the particles of the iron, and allows the pressure of the æther on the other end to produce its effect.

N.B. This is only the language of a theory, and that theory not mine; nevertheless, I think it is consistent with itself.

Glenlair, Aug. 13 J. C. M.

Reflected Rainbows

I READ with great interest, in Prof. Tyndall's American lectures, a statement about the rainbow which appeared to me so extraordinary, that I determined to test it on the first opportunity.

The statement (I have not the book with me here, and give merely my recollection of the substance) is that, owing to the want of the necessary condition of parallelism the rays scattered from rain-drops cannot be so reflected as to show a rainbow by reflection from the surface of a lake.

Of course we all know that the same rainbow cannot be seen from two places at the same time, and therefore no one would expect to see the *same* rainbow directly and by reflection. It is also reasonable to suppose that, as a rainbow is often seen from one place and not from another, a rainbow may often be seen directly and not reflected, or *vice versâ*. The reference to the necessary condition of parallelism shows that it is something more than these obvious deductions from the laws of reflection to which Prof. Tyndall wishes to draw attention in the passage mentioned. Until I tried the experiment described below, I imagined him to mean that there was something about the direction or arrangement of the rays of light producing a rainbow, which prevented their forming a rainbow or anything like one, after reflection from the surface of still water. It is not always easy to arrange so as to have a rainbow and a still lake to experiment upon. I managed, however, to get satisfactory substitutes in the spray bow at the falls of the Rhine near this, and a small pool of water. I was greatly disappointed on looking into my pool, to see reflected not only the scenery of the falls, but also a very fine spray bow.

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* Die Atome und ihre Bewegungen, von Gustav Hansemann. E. H Mayer: Coln, 1871.)

What then can Prof. Tyndall mean? How is this peculiarity of rainbows to be observed? I have tried it in the only way of which I could think, but am now inclined to believe that I must have mistaken Prof. Tyndall's meaning.

Schaffhausen, Aug. 23 Z. X. Y.

The Origin of Nerve Force

ONE at least of the "obvious difficulties" which your correspondent, Mr. Henry R. Proctor, finds in my hypothesis as to the origin of Nerve Force, would scarcely have existed if he had directed his attention to a sentence in my article (NATURE, July 31), which runs thus: "In what are termed hot-blooded animals, that is, in mammals and birds, the difference of temperature between the surface and the interior is considerable under all natural circumstances, and in them there is a regulating action of the skin by which they maintain a uniform internal temperature, *always hotter than the surface*, whatever that of the external medium may be." The correctness of this proposition as regards the human being is now a physiological fact, as many observers from different starting points have arrived at the same conclusion; among others, my proof of it has appeared in the "Journal of Anatomy and Physiology" (vol. vi. November 1871). When the temperature of the atmosphere is above 70° F. the amount of perspiration is always proportionate to the temperature, and is sufficient to maintain the depths of the body at 98° or so. Below 70° the same condition results from the influence of cold on the cutaneous vessels, they contracting in proportion to the degree of cold, and so modifying the radiating and conducting power of the body surface. There is never therefore any reversal of the current, or a temperature at which it is *nil*.

Your correspondent's third paragraph contains an assumption, as great and not so reasonable as my own. Why should we have to assume that the body has to be kept at a constant temperature of 98° or so? There is no *a priori* reason in its favour. It may be said that the chemical changes which occur, being dependent on the properties of albumen, fibrin, &c., could not be continued under other circumstances. That, however, is only a shifting of the ground of argument, for it is much more reasonable to suppose that the properties of the animal tissues are the result and not the cause of the conditions under which they have been brought into existence.

I may mention that the physiological phenomena attending the immersion of the body in air and water of different temperatures are of quite a different character; they are scarcely comparable, and can be shown not to depend to any extent on the different conducting powers of the media, or their different specific heats. Immersion of the nude body in air of 30° is not rapidly fatal, even if the temperature is not kept up by violent exercise; and I cannot understand "immersion in water at 30°."

If the comparative coldness of the brain were the effect of absorption of heat in the building up of its elaborate texture, we should expect to find a similar condition in the muscles, which are also of very complicate construction. Such, however, is not the case, and therefore another explanation has to be found, which my hypothesis supplies.

Aug. 26 A. H. GARROD

The Flight of Birds

I HAVE just read with great interest, in NATURE of Aug. 21, Capt. J. Herschel's account (elicited by Mr. Guthrie's letter, vol. viii. p. 86) of his ocular and telescopic observations of Indian kites at rest in mid-air, and I am tempted to offer an explanation which occurs to me of the way in which that aërial balance may be maintained.

If there was no quiver of the wings perceptible "at an apparent distance of ten or twelve feet,"—if the very tips of the wings "looked as steady as those of a stuffed specimen,"—then certainly the theory of self-support by muscular action must be abandoned, and the problem is reduced to one in which we have only to consider the weight and shape of the bird with outspread wings and the velocity and direction of the wind.

If the direction of the wind is slanting upwards with moderate velocity, it is conceivable that a bird, facing the wind, with outspread wings in a plane inclined between the horizontal and the direction of the wind, might remain at rest, from the following considerations:—

If the air were at rest, the bird, with the plane of its wings inclined a little downwards and forwards, would not fall vertically, but would slide obliquely forwards down the air, like a returning boomerang, or an inclined sheet of paper let fall, and would reach the earth at some point far from the vertical. But suppose, instead of the air being at rest, there were a slant upward current of air meeting and balancing the slant fall of the bird: then the bird would remain motionless in mid-air.

Capt. Herschel rejects (perhaps too hastily) the notion of "slants of wind," and asks "what becomes of the horizontal force" of the wind. Surely its effect would be to balance the horizontally resolved portion of the bird's slant fall, just as the vertically resolved portion of the slant current of wind would balance the vertically resolved portion of the slant fall.

Different degrees of inclination and force of the wind might be met (within limits) by different degrees of slope and spread of the wings.

I must confess this is only theory. We want more observations, as keen and careful as Capt. Herschel's, to ascertain the force and direction of the wind attending this arrest of motion in mid-air. Slant currents are common enough on a small scale among house-walls, and on a larger scale we may see how the wind pounces down on a land-locked water, or presses up a mountain side. In a steady wind, the shapes of hill and valley must cause certain regular currents variously inclined to the horizontal, and some of these, I suppose, the eagles find and use. On the lee side of a hill (as in the case given by Captain Herschel) there would be a current rising from the eddy to join the main course of the wind. The conditions described by Mr. Guthrie were just such as would throw the wind into upward slanting currents.

We should want a well-balanced weather-cock with a double vane (one plate in a *horizontal*, the other in a vertical plane), to tell the *vertical* as well as the horizontal deviation of the wind.

Dacre Park, Lee, S.E., Aug. 24 HUBERT AIRY

Mallet-Palmieri's "Vesuvius"

MY absence in Spain during the months of March and April prevented my having seen NATURE for the 20th March, and left me until a few days since in blissful ignorance that it contained a lengthy critique by Mr. Mallet on my review (NATURE, Feb. 6) of his translation of "Palmieri's "Incendio Vesuviano." This accounts for my silence, as, had it not been the case, a reply from me would certainly have appeared at the time.

For, being "the reviewer reviewed," I suppose I am indebted to my habit of not taking advantage of a reviewer's privilege, but of signing my name in full, since I do not find that Mr. Mallet vouchsafed a reply to any other review of his book, not even to that contained in the *Geological Magazine* for March, which, as the organ of British Geological opinion, might be expected to have the preference over mine, even if its reviewer had not incurred special claims on Mr. Mallet's attention, by having handled his production in a vastly less tender manner than I had done.

In comparing the two translations of Palmieri's little pamphlet, I give preference to that in German by the eminent mineral chemist Rammelsberg, if for no other reasons, for its cheapness, and because the translator puts forth the work of the Italian professor entirely on its own merits as one which did not require to be heralded by any elaborate preface to make it take with the public, and also because it seems somewhat unfair to see the worthy Professor's excellent observations made a vehicle for introducing the public to what, although entitled "an introductory sketch of the present state of our knowledge of terrestrial vulcanicity," &c., is far from being such, and in greater part but a one-sided exposition of Mr. Mallet's own views of what he terms vulcanicity and vulcanology, and which, to quote one of his reviewers, "has really no connection with Palmieri's report."

It is unnecessary here to occupy space in splitting hairs over the exact definition of words, such as theory, hypothesis, force, &c., being quite content to assume that the readers of NATURE fully understood the sense in which I employed them; but when the abstracts published in the "Proceedings of the Royal Society" are generally admitted to be faithful reports of the main features of the Memoirs read before the Society, and here it is not a question of details, I ask any rational individual whether Mr. Mallet, merely because he considers the abstract of his paper as "most meagre and incomplete," is justified in using such words (p. 382) as "Mr. Forbes commences with an important error as to a matter of fact, by referring to 'Mr. Mallet's Dynamical Theory of Volcanic Energy,' as published in the Proceedings of the Royal Society for 1872."

When an author commits himself to print, he should also be prepared for the consequences; yet the tone of Mr. Mallet's critique evidently indicates extreme irritation in finding his views commented upon before his communication to the Royal Society is published in full, explaining in other words, that before this is done nothing is known about them—a state of things eminently suggestive, both that the scientific men who, he is pleased to inform us, have already expressed themselves in his favour, may, after all, have been somewhat hasty in so doing, and also that Mr. Mallet would have been more wise if he had withheld his self-laudatory sketch until the publication of the evidence in favour of his views had afforded the scientific world the opportunity of forming a mature judgment as to their soundness.

Volcanic rocks, or rather rock species, are commonly arranged under the two classes, Trachytic and Pyroxenic, names proposed by Bunsen as the equivalents of acid and basic, and it is hardly necessary to observe that when the mineralogical and chemical natures of rocks are to be compared, some such classification must be taken into account, since it would be as absurd to liken a trachytic to a pyroxenic rock as chalk to cheese; it must also be remembered that the same volcanic cone may emit lavas of both these classes, a fact observed by the Scientific Commission at the eruption of Santorin, when in exactly similar manner to many ancient outbursts, the trachytic preceded the subsequent and more abundant pyroxenic lavas. As regards the mineralogical and chemical constitution of unaltered volcanic rocks, nothing is more certain than that from whatever part of the world they proceed, they are essentially made up of a very limited number of mineral species, always the same, and the application of the microscope to petrology has now proved this to be the case also, when they are of so compact a texture as not to admit of their constituent minerals being distinguished by the naked eye. The examination of any large collection of volcanic rocks cannot fail to impress the observer with the wonderful similarity of the various rock specimens from one volcano to corresponding ones from others situated at the greatest distances; and ample evidence of this may be seen in the writer's extensive collection, the result of many years' labours in the volcanic districts of Europe, America, Australasia, and Africa, and in which, for example, specimens may be seen of trachytes from Auvergne, the Rhine, or the Andes, undistinguishable from one another when placed side by side, other lavas from Otaheite (where, however, Pele's hair is not found, as mentioned by Mr. Mallet), to all appearance identical with those from Etna, both of which volcanic districts he has had good opportunities for studying.

Pele's hair, from Hawaii, in the Sandwich Islands, so called from its having been blown by the action of the winds over the surface of the molten lava into hairlike filaments resembling spun glass, is simply pyroxene, a mineral which, next to felspar, is the most common constituent of the lavas of all volcanos.

When, however, Mr. Mallet asks, "Are the ancient basalts and trachytes identical with the modern ones or with each other in different localities?" the answer to the first question is simply no, for the results of modern petrological inquiry tend to show, although no sharp line can be drawn, that the volcanic rocks which made their appearance in the successive stages through which our globe has passed, although more or less characteristic of the epoch, were analogous to, but not absolutely identical with, those which either preceded or succeeded them; and to the second question the reply is, that they are identical in mineralogical and chemical constitution, and often even approximate closely in percentage composition.

The well-known researches of Bunsen on the volcanic rocks of Iceland, followed up by those of Abich on those of the Caucasus, showed the simplicity and identity in chemical constitution of volcanic rocks, and the later results of trustworthy chemical analyses, not of fragments chipped off at random, but of such as represent the mass of the unaltered rock itself, are every day bringing forward more complete evidence of this being the case; this is, without doubt, well understood by Professor Palmieri, for the very words cited by Mr. Mallet, in which he mentions that "two specimens of the same lava appear indeed to have their constituents in different proportions" are qualified by stating that the observatory did not possess the means of arriving "at any conclusion" on this point, and by expressing the hope that Prof. Fuchs, who had specially devoted himself to this subject, would, by employing "well selected and sufficiently large specimens," obtain satisfactory results. Not only do all chemists and mineralogists know that there may exist a considerable difference in the percentage composition of mineral species which are of identical chemical constitution, but in answer to Mr. Mallet's questions as to iron blast-furnace slags, every scientific metallurgist will admit that the basis of good smelting, necessitates the production of slags having a constant and definite chemical constitution, and that not only should the slag from every two tappings be identical in this respect, but that so long as the furnace works properly, and the same materials are charged into its mouth, the same slag will also flow from its hearth; many years' practical experience in the management of blast-furnaces, and the numerous analyses of slags which I have made, some of which will be found published as far back as 1846 in the British Association reports on the crystalline slags, have not only fully satisfied me on this point, but shown me examples of iron blast-furnaces, which, from their having been constantly fed with precisely the same ore, fuel, and flux, have not only for successive tappings, but for years, produced slags, not only identical in chemical constitution, but in which the percentage of the constituent silica and bases have only varied within extremely small limits.

When Mr. Mallet, however, asks such questions as whether the *crystalline* minerals of volcanic rocks are identical, and furnishes in his critique the most ample evidence of his confounding chemical constitution with percentage composition, ignoring altogether the laws of isomorphism and the substitution of bases, I believe mineralogists will absolve me from taking up more space in discussing further these questions. The implication of being unacquainted with the works of von Waltershausen, Senft, Blum, and my good friend Zirkel, which have been in my hands, I might almost say warm from the press, is easily disposed of, as numerous references will be found to them in my published papers; curiously enough, not long back I referred in a paper to opinions of von Waltershausen which are diametrically opposed to those held by Mr. Mallet; in the *Geological Magazine* for 1867, p. 227, references will be found to the other three works. If Mr. Mallet's knowledge of recent petrology is based upon "Blum's Handbuch der Lithologie," which he recommends "above all," I would remind him that this work, although a very excellent one when it was written in 1859, is now quite antiquated, this branch of mineralogical science being then, as it were, only in its cradle as compared to the great advances which have been made during the last eight or ten years: "von Waltershausen vulkanische Gesteine" appeared still earlier, in 1853.

I would remark that neither in this communication, nor in my review, was it the intention to take into consideration Mr. Mallet's theory of volcanic energy, and it was only alluded to because, in his introductory sketch, he so altogether overlooked those explanations which, notwithstanding his reply, will still be demanded by chemists, mineralogists, and geologists, before they can accept his views; I still object most strongly to the tone and style of his introductory sketch, and I am not alone in doing so.

Thornton Cottage, Aug 8. DAVID FORBES

Explosion of Chlorine and Hydrogen

SOME time ago, being desirous of showing a class the explosion of chlorine and hydrogen by artificial light, I devised a simple method which was perfectly successful. Equal volumes of the two gases, prepared separately by the usual methods, were mixed in a stout test tube and confined by a greased cork. This was placed upright on a little wooden stand, and kept in its place by a brass clip. About an inch of magnesium ribbon was suspended in a small tin shade by means of a wire clip. The magnesium being placed near the tube and lighted, the gases united with a report, jerking the cork to the ceiling, but in no case breaking the tube. W.

A NEW BUBALE, FROM ABYSSINIA

THE British Museum has just received a series of skins of a new Bubale from Abyssinia called Tora. It is like the Hartibeest for having a white patch on the rump, and white inside the ears, but it is without any black on the face or on the outer side of the limbs. It is of a bright pale bay colour, with black tuft on the tail, and the horns are much more slender than in the Hartibeest. I propose to call it *Alcephalus tora*.

J. E. GREY

FROM AMERICA TO ENGLAND BY BALLOON

THERE appears every likelihood that before the end of the year a feat will be attempted which seems to have been first seriously proposed thirty years ago by Prof. Wise, an American aëronaut, who is now making preparations to cross the Atlantic to England in a monster balloon. The American correspondent of the *Standard* has given full details of the elaborate construction of this balloon, and states the reasons which inspire Prof. Wise with unhesitating confidence that he will be able successfully to accomplish his aërial voyage.

The balloon, when completed, will be 160 ft. high, and the globe will be over 100 ft. in diameter. It will be able to lift from the ground, including its own weight, 14,000 pounds, and will have a net carrying capacity for passengers and ballast of 6,900 pounds. It will contain 600,000 cubic feet of illuminating gas, though only 400,000 feet will be put into it to allow for expansion in the higher regions of the atmosphere. The other details of construction are most elaborate, and every precaution seems to be taken to insure success and to provide for the safety of the four persons who are bold enough to risk their lives to gratify their curiosity and endeavour to increase the sum of human knowledge. The four voyagers will be Prof. Wise, Mr. Donaldson, an agent of the *Daily Graphic*, and a skilled mariner—for a copper-fastened cedar lifeboat, 22 ft. long and 4½ ft. beam, forms part of the appurtenances.

The hypothesis on which the enterprise is projected, is that there is a prevailing east-going current of air at an attainable elevation, in which a balloon can pass eastward from the American continent to Europe. The current is believed to be half-a-mile or more above the surface of the earth, and to move at the rate of from 50 to 150 miles an hour. It was a knowledge of this current that made Mr. Charles Green, the celebrated English aëronaut, say, in 1840, that he should start from America rather than from England to traverse the Atlantic in a balloon. The cause of the current is less definitely known than the fact. A French *savant* attributes it to "a decrease of participation in the rapidity of the rotary motion of the earth." Prof. Wise believes that this upper current of air, in the temperate zones, moves from west to east, because of the mingling of the south-west and north-west trade-winds in their circuits, in accordance with the laws of temperature and the aërial motion of the earth. The two currents, he believes, slide over each other, and the balloonist who knows his business can strike such a point as will carry him eastward, as it were, between them. That is to say, the zone lying between the 35th and 36th parallels is "a nodal zone," in which the south-west and north-west winds induce an intermediate current which moves nearly due east. In this highway the motion is about a hundred miles an hour.

The theory of the east-going current seems to be pretty well admitted. The direct experience which bears most strongly upon it is limited. There are three memorable balloon trips which are noteworthy. The current seems to set persistently eastward, deflected slightly towards the north by the rush of equatorial air towards the north. Prof. Wise, in 1859, in his trip from St. Louis to Jefferson county, in the State of New York, found the current almost due east; he travelled in balloon 1,156 miles in 19 hours. The speed here was only 61 miles an hour; but this can be accounted for. The great balloon voyage made by Nadar from Paris to Hanover was almost due eastward. This journey of 600 miles was made in about six hours—about a hundred miles an hour, although it was over the uneven surface of the Continent, diversified by hill, vale, stream, and so on. In the trip of Mr. Green, from London to Wellburg, in Nassau, the journey was about 600 miles, and was performed at the rate of about a hundred miles an hour, and there were the British Channel and other irregularities in the way of smooth sailing.

On the other hand, however, Mr. Glaisher in his experiments, in consequence of what Mr. Green had stated with regard to the constant prevalence of a current from the west, paid special attention to this point, and in his reports to the British Association in 1863 and 1864,[*] collected together the different directions in which the balloon had moved at different heights in his several ascents. From these it appears that the direction of the wind was quite as capricious at heights exceeding 5,000 ft. as it is on the surface of the earth. In Mr. Glaisher's winter ascents he did generally meet with a current from the south-west, certainly; but the number of such ascents was not great, and they were not to sufficient elevations to afford very trustworthy results. It is certain, however, that if there existed over England anything like a current of air constant in direction, it must have manifested itself distinctly in the course of Mr. Glaisher's thirty ascents, in all of which the direction of the wind at different elevations was a subject of careful observation.

Again, Prof. Newton of Yale College has written a letter to a recent number of the *Daily Graphic*, in which, from the observed behaviour of the luminous trains sometimes left by the brighter meteors at from forty to seventy miles high, he draws certain inferences which do not seem altogether favourable to Prof. Wise's theories. What these inferences are will be seen from the conclusion of his letter:—

"We have, then, at the *bottom* of the atmosphere, inconstant winds. We have just above us strata of air moving in diverse directions, for the lower clouds may move one way, the upper clouds another, while at the surface the winds may perhaps blow in a third. At two islands at short distances from each other we often have different winds.

"Again, we have for air near the top of the atmosphere, at least so high up that the density is exceedingly small, this fact, that lines (usually inclined to the horizon) only five or ten miles long almost always have their ends in air that is moving in different directions.

"Between the highest cloud and the lowest meteor trains lies an unknown region. It may be that here are uniform westerly winds. In the absence of direct observation neither this nor the contrary may be asserted. But it seems to me more rational to suppose that the complex system of currents at the bottom of the atmosphere is in direct connection with that at the top, and that there is a like complex system of currents and winds throughout the intermediate space. Of course, the general drifting of the air in the temperate zone to the east is unquestioned.

Prof. Joseph Henry, of the Smithsonian Institution, who has had thirty years of observation in this direction, says :—

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[*] British Association Reports, 1863, p. 507, and 1864, p. 313.

"'All the observations that have been made in the motions of the atmosphere, as well as the deductions from theoretical considerations, lead to the conclusion that the resultant motion of the air around the whole earth, within the temperate zones, especially about the middle of them, is from west to east.' Prof. Watson, the distinguished astronomer of the Michigan University, writes, 'I beg to say that there ought to be a strong current of air moving eastward in the upper regions, and that the experience of aëronauts goes to show that what the theory predicts actually exists. It seems to me quite possible to make an aërial voyage to Europe, and with great rapidity.' William H. Wahl, secretary of the Franklin Institute at Philadelphia, writes, 'I believe that, generally, Prof. Wise's proposition, concerning the existence of the elevated easterly current, is correct, and the same view is entertained upon theoretical grounds by meteorologists.' To the same effect writes Prof. Brocklesby, of Trinity College, author of 'Elements of Meteorology,' a work recognised as the best elementary text-book on the subject."

Still Prof. Henry is by no means enthusiastically in favour of seeing the dangerous voyage undertaken; he speaks of it as at the best extremely hazardous, and would prefer that some one else in whom he is less interested than he is in Prof. Wise would undertake the risk. His letter to Mr. Wise, in which he thus speaks, is worth quoting for its meteorological value. He says—

"I have no doubt of the fact that, if your balloon can be sustained in the air sufficiently long, a voyage might be made across the Atlantic; but this is the point which, it would appear to me, from my partial knowledge of what has been accomplished in the art of ballooning, is yet to be satisfactorily established. No one, however, has had more experience in the art than yourself, and you ought not to venture on the hazardous voyage without the fullest assurance that the balloon can be sustained at the requisite elevation for, say, ten days.

"I think it probable that over the ocean at a considerable elevation, the tendency to meet adverse currents will be less than over the land; on the other hand, however, there will be a chance of meeting a cyclone, which might carry you around a circle of several thousand miles, and throw you back over the coast of the United States, since you would be most likely to meet the northern portion of the great whirl, which would be moving in the western direction, the only possible escape from which would be by ascending to a very high elevation. The higher temperature of the Gulf Stream tends to produce an ascent of air above it during the colder months of the year, but in summer this effect would scarcely be perceptible.

"Your remark in regard to the greater velocity of the easterly motion of the balloon at night is in accordance with meteorological principles, since at this period the unequal heating of the earth by the direct rays of the sun does not take place, and hence adverse currents are not as frequent. The cooling of the atmosphere in that part of the earth which is in the shadow will tend to produce at the surface of the earth, after sunset, a westerly current, while at a certain elevation above the earth, the current would at the same time be in an opposite direction. In the morning, just before and after sunrise, the current at the surface of the earth, produced by the cooling, would be eastward, while that in the atmosphere above would be westward."

There can be no doubt that this daring expedition, whether it descends without mishap on the shores of Europe, or comes to grief in the middle of the Atlantic, will add something to our knowledge of the atmosphere; but many will no doubt think that all the knowledge that will be acquired by this sensational and hazardous method might be acquired by safer and more ordinary methods. We certainly, with all our heart, wish the enterprise complete success; but we think it very pertinent to refer to some remarks on the project in *La Nature* by the experienced balloonist, M. G. Tissandier. After referring to the theory of the easterly current in the atmosphere, M. Tissandier says, "We leave to the aéronaut all the responsibility of this hypothesis, which appears to us to be based upon vague conjectures; we should have a little more confidence in the resources which he expects to find above the Gulf Stream. This warm river, which traverses the extent of the Atlantic, should draw along with it a current of air, which the aërial navigator might take advantage of.

"We do not doubt the good faith of the aëronaut, who has already proved himself to be possessed of boldness and courage, but we believe he has not maturely considered the problem he proposes to solve. To go from New York to England, the aeronaut must travel a space of about 5,500 kilometres. Suppose that exceptional good fortune favours him, that a favourable wind, of mean intensity, having a speed of ten metres per second, blew regularly from west to east, without deviation, he must necessarily sojourn in the atmosphere six or seven days at the least, since the distance traversed in twenty-four hours will be, according to our hypothesis, 864 kilometres. But can an aërostat, no matter how voluminous it may be, constructed under existing conditions, and notwithstanding its complete impermeability, remain in the atmosphere for seven days? To this we reply, with the utmost confidence, in the negative. In fact, when a balloon quits the earth, as it rises a part of the enclosed gas is at once expelled by the dilatation due to the diminished pressure of the atmosphere; but the aérostat soon plunges into regions where the temperature is much lower than that of the strata of terrestrial air which it has left. The cold contracts the gas, the balloon loses its ascending power and descends. To maintain it at the level it has reached, it is necessary to diminish the weight, and the aéronaut throws out ballast. If he pass a first night at great altitude, it is certain that he will be thus obliged almost continually to lighten his craft. Next morning, as the sun rises, the bright burning rays heat the gas contained in the aërostat. The balloon, which had partly collapsed during the night, begins to fill out, the loose material stretches like the head of a drum, and it mounts into the higher regions of the atmosphere. It is now that the aëronaut will feel the want of a portion of the ballast he was obliged to cast away during the night. If the sun is hot, the balloon will rise so high that it will be necessary to moderate its ascent by letting off some of the gas. During the second night the reverse process takes place. This time the aëronaut has no longer the same resources as before; the ballast, which is his life, is being continually exhausted. I willingly admit he may have sufficient for the second and even for the third night; but will he have enough for the sixth and seventh night, if the differences of temperature of day and night are considerable, as is probable? The moment will soon come when the sacks of sand will be empty; the balloon will descend without any means being able to hold it back. But instead of encountering a hospitable soil, it strikes against the crest of the waves. The anchor instead of biting, will plunge in vain in the waters; if the wind is violent, in spite of their life-boat, the voyagers may be prepared for a most horrible fate. The aerostat will be piteously raised by the wind, and the terrified train will shoot from wave to wave over the surface of the ocean. Unusually clever will be the men carried along by such a force, if they could manage to find the means of detaching the life-boat."

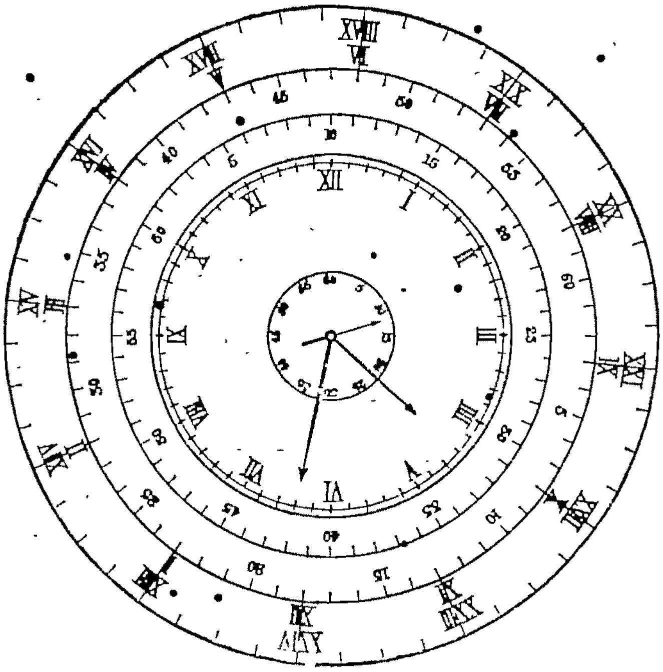
It is certainly true that it would be very difficult to sustain a balloon at a considerable elevation for six days (if the height of the balloon is a matter of indifference, the guide rope as used by Green would be quite sufficient to answer this purpose, even with an ordinary balloon), but we think the management of the balloon may be very well left to Prof. Wise, whose opinion on all practical points of aërostation is probably of more value than that of any other man living. Of all the persons who have devoted themselves professionally to ballooning as a source of income, Prof. Wise is certainly the ablest, and his work on Aëronautics shows him to be possessed of considerable scientific claims. The project could not, therefore, be in better hands; and considering the originality and boldness displayed by Prof. Wise in several of his very numerous ascents, there is every reason to believe that nothing will be left undone to bring it to a successful issue. In all the technical matters relating to the balloon, therefore, Prof. Wise may be well trusted to take the best course; and with regard to the meteorological questions involved by consulting not only American meteorologists but also Mr. Glaisher and other gentlemen who have studied the question of the winds in relation to aërostation, it is clear that he intends to leave no stone unturned to obtain the best information attainable, and, at all events, merit success.

MAYNE'S SIDEREAL DIAL

THIS instrument consists of two moveable circles, which may be made of brass or pasteboard, placed in a common watch-case. The lower and outer one shows the hours doubled up to XXIV., and divided into quarters. The upper one, which is also inner, shows the sixty minutes, 5, 10, &c. This circle is a narrow one, and works on the plain inmost rim of the lower one, so as to admit of the hours being seen outside the minutes.

Each circle being set to show at the top of the case, where the XII. of the watch comes, the "Sidereal Time at Mean Noon" (given in the Nautical Almanack for each day in the year), the watch is placed in the case, and will continue to show the sidereal time corresponding to mean time approximately for six hours, after which interval the minute circle should be *put on* one minute to ensure greater exactness.

This will be found a near enough approximation for the amateur observer, using an equatorial instrument, and this simple method will be found to save an infinite amount of trouble *in finding objects whose R.A. is recorded in a catalogue to those who, like the inventor, are unprovided with a sidereal clock.*



Mr. Norman Lockyer has suggested as an improvement, the use of a watch with the *seconds'* hand in the centre; this would necessitate a third, and still inner circle for the sixty seconds, by which, indeed, subject to an hourly correction of, say ten seconds being *put on*, the dial would be rendered accurate enough for rough transit observations; and this circle and *seconds'* hand have been added to the original design in the woodcut, where the dial is set to V. (¾) 47·10, the Sidereal Time at Mean Noon for the 18th June, 1873, the hands of the watch representing IV. (½) 32·12, which gives the corresponding Sidereal Time X. 19·22 (or applying the last-named correction, say 45 seconds for 4½ hours), X. 20·7.

It is as well perhaps, though scarcely needful, to add (for no one would be likely to make a mistake of 12 hours) that as the dial in the Example also reads XVII. (¾) 47·10, and as the mean time by the watch may be A.M. or P.M., the observer should bear in mind which half of the 24 hours, both astronomical and mean, he is working in.

The third or *seconds* circle is not indispensable, as the seconds hand, even in the ordinary position, can be made to fulfil its object, by setting it at noon to the Sidereal Second on the meridian; thus, in the Example, it would be set to 10, instead of to Zero, when the dial is set at noon, the correction for the equivalent of the lapsed interval being applied subsequently as required. But this involves altering the watch, which is objectionable; the use of the third, or *seconds'* circle, is therefore recommended, for although the seconds' hand, as placed in most watches, would not actually *point* to the Sidereal second, it is easy to refer the *position* of the mean second to the corresponding part of the watch's face, where the third circle can be read off at once.

ASHTON MAYNE, Captain,
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65, Cornhill, London, E.C.

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE *

IV.

The Imperial Standard Yard

THE immediate superintendence of the construction of the new standard yard was entrusted, in the first instance, to Mr. Baily, who conducted all the preliminary investigations and experiments. After his death in August 1844, it was undertaken by Mr. Sheepshanks, by whom and under whose direction by far the largest proportion of the actual operations was carried out, and all the comparing operations of the several standards of length made, up to the period of his death in August 1855. By this time the work was so far completed that not a single additional comparison of line measures was required. The detailed account of the construction of the new standard yard, and its verified copies, was then undertaken by the Astronomer Royal, with the aid of the documents left by Mr. Baily and Mr. Sheepshanks; and the winding-up of the work of the Commission, and the distribution of the scientifically verified copies of the standards also devolved upon the Astronomer Royal, as the chairman. The magnitude of the operations may be estimated from the fact of the number of micrometer readings for all the comparisons exceeding two hundred thousand; and amongst the operations it was found necessary to construct an entirely new system of thermometers. It should not be forgotten that the scientific gentlemen who bestowed so much of their valuable time, attention, and labour, during several years upon the experiments and observations for the important object of the restoration of the national standard of length, declined to accept any pecuniary remuneration.

The length of the new standard yard was determined in a similar manner to the determination of the weight of the new standard pound, by taking the mean length of the most authoritative standards which constituted the best primary evidence of the lost standard yard.

This standard measure of length had been constructed by Bird, in 1760, under the directions of the Committee of the House of Commons on Weights and Measures, first appointed in 1758. Its length was taken from a similar yard which had been constructed by Bird in 1758. Each of these standard yards consisted of a solid brass bar 1·05 inch square in section, and 39·73 inches long. Near each end of the upper surface gold pins or studs 0·1 inch in diameter were inserted, and points or dots were marked upon the gold to determine the length of the yard. The comparing apparatus in use at that period consisted of a beam compass with two fine measuring points, which could be adjusted to the dots on the standard measures under comparison. But the result of numerous comparisons of this kind made from time to time previously to the destruction of the standard in 1834, had been to leave the edges of the holes indented and irregularly worn away, so that the original centre was very difficult to ascertain. Mr. Baily, who had made some comparisons with this standard yard in the early part of the year 1834, describes the holes as appearing, under a microscope, like the miniature crater of a volcano.

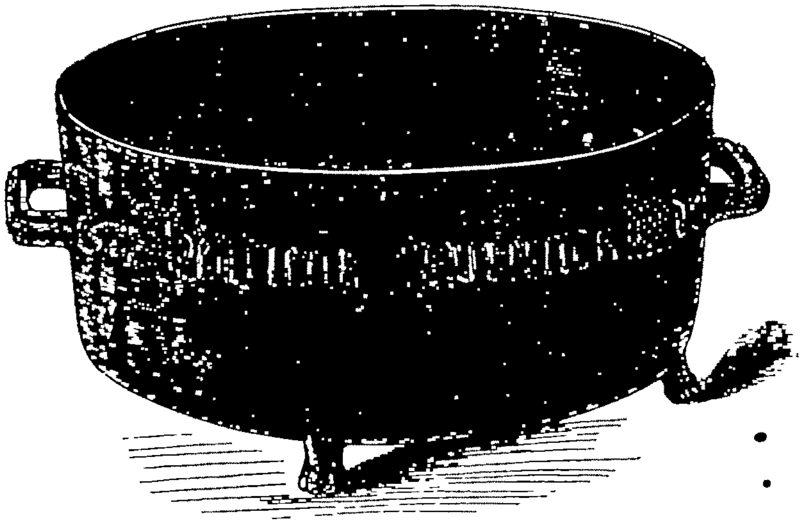

FIG. 8.—Standard Winchester Bushel of Henry VII ⅛ size.

The length of the standard yard of 1758, had been based upon that of the then existing Exchequer standard yard, which had been constructed in the reign of Queen Elizabeth in 1588, and upon the length of the Royal Society's standard yard, constructed as a scientific standard measure in 1742. It had been determined, upon comparison, to agree as nearly as possible with these two authoritative measures of a yard.

The two standard bars of 1758 and 1760 were found amongst the ruins of the Houses of Parliament, but they were too much injured to indicate the measure of a yard which had been marked upon them.

Bird's standard yard of 1760 had been left in the custody of the clerk of the House of Commons, and no legal authority was given to it as a standard of length until the passing of the Act 5 Geo. IV. C. 74, in 1824, already referred to. Meanwhile, other scientific standards of length had been constructed which may now be noticed.

In 1785, the first geodesical operations were begun, upon which the Ordnance Survey of the United Kingdom has since been founded, by General Roy's measurement of the base on Hounslow Heath. The standard used in the first instance for that purpose was that known as General Roy's scale, 42 inches in length, and constructed by Mr. Bird. This scale was based, not on the legal Exchequer Standard, but upon the Royal Society's scale, with which the whole length of the first 36 inches of General Roy's scale was compared, this constituting the *Ordnance yard*. Two standard yards of superior construction, belonging to the Ordnance Department, were placed at the disposal of the Standards Commission,

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* Continued from p. 349.

These were bars of iron, and line standards, the lines being marked on gold pins at mid-depth of the bar, notches being cut in it for that purpose. They had been compared with the imperial standard in 1834, and a statement of their comparison was published in 1847 in the account of the measurement of the base at Lough Foyle.

Towards the close of the century, some important scientific operations for the improvement of the standards were undertaken by Sir George Shuckburgh. In 1796, a new standard measure subdivided in fine lines, and since known as "Shuckburgh's scale," was constructed under his direction, by Mr. Troughton, together with a new comparing apparatus carrying micrometer microscopes. This is stated to have been the first occasion on which this mode of optical comparison was employed, being substituted for the beam compasses previously used. The Shuckburgh scale, which is now in the possession of the Royal Society, consists of a brass bar 67¼ inches long, 1·4 in. wide, and 0·42 in. thick. It is a scale of 5 feet, divided by lines into feet, inches, and tenths of inches, each inch being numbered. It was adopted by the Standards Commission of 1819 as the scientific standard of length, as distinguished from the legal standard of the Exchequer. The length of the yard was laid down on the Shuckburgh scale from Bird's standard, and it had also been accurately compared with each of the other standard yards previously mentioned, and their lengths had been transferred by beam compasses to the Shuckburgh bar.

In pursuance of the recommendation of the Royal Commission of Weights and Measures appointed in 1819, and of the Act of 1824, passed to carry their recommendations into effect, a new Exchequer standard yard for regulating commercial measures of length was constructed under Capt. Kater's superintendence. It was not, however, laid down from the legal standard yard, which, together with the legal standard pound, remained in the custody of the Clerk of the House of Commons, but from the length 1–36 in. of the Shuckburgh scale, which was considered by Capt. Kater to be identical with the imperial standard.

The official standard yard constructed for the Exchequer, under Capt. Kater's superintendence, in 1824, and intended for the verification of the local standard yards used by inspectors for comparing trade measures, consists of a slender brass rod with two wooden handles, as an auxiliary end measure, and a bed measure, being a bar of brass one inch square with rectangular steel terminations of the same width projecting above the surface of the bar. The distance between the interior faces of the steel terminations is intended to be equal to the length of the imperial yard. This yard bed and rod were used together from 1825 to 1870, for verifying all the local standard yards of similar though ruder construction. A standard yard, with the legal subdivisions marked upon it, and of improved construction, having a convenient comparing apparatus attached to it, has since been substituted, and is now used in the Standards Department.

Four other standard yards of more scientific character were also made under Capt. Kater's directions, and are now in the Standards Office. These bars of brass are of the same width and thickness as the Shuckburgh Scale, and have the length of the yard defined by fine points upon gold studs in the middle axis of the bar, the thickness of the bar being reduced at its extremities one-half with this object. All these standard yards were constructed by Dollond. By an ingenious contrivance the point at one end of the bar, not being placed exactly in the centre of the circular gold stud, was made susceptible of adjustment, by turning the stud round; and after final adjustment of each yard and repeated comparisons with the Shuckburgh Scale, no perceptible error could be detected in any of them. A similar standard measure made for the Royal Society in 1831 was considered by the Commission to be the most favourable type of Kater's yard.

Having thus described the principal standard yard measures then existing, we may return to the operations of the Standards Commission. For determining the true length of the new standard yard, a provisional standard yard was employed by Mr. Sheepshanks. This was a new brass bar, called by him "Brass 2," and was accurately compared by him with the standards deemed to be the most authoritative, and which had been *directly* compared with the lost standard, viz. Shuckburgh's scale, Kater's yard made for the Royal Society, and the two Ordnance yards. The results in terms of the lost imperial standard were as follows:—

In

Brass bar 2 = 36·000084 by comparison with Shuckburgh scale 0–36 in.
„ = 36·000280 „ „ 10–46 in.
„ = 36·000229 „ Kater's Yard of 1831.
„ = 36·000303 „ Ordnance Yard, No. 1.
„ = 36·000275 „ „ No. 2.

„ = 36·000234 by mean of all.

Mr. Sheepshanks preferred 36·00025, as being sufficiently near the truth, and in constructing the new standard, he assumed as the basis of his proceedings—

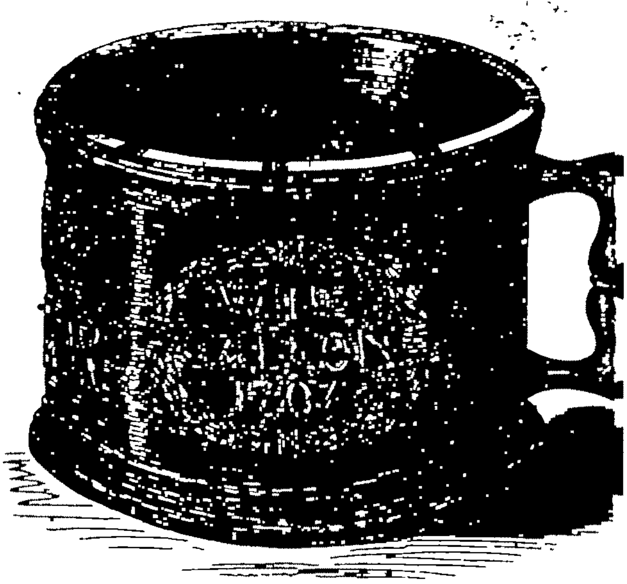

Fig. 9.—Standard Wine Gallon of Queen Anne, ¼ size.

Brass 2 = 36·00025 in. of lost imperial standard, at 62° Fahr., and this conclusion met with the assent of the Commission.

In the construction of the new standard of length, the following decisions were made by the Commission:—

1. The length of one yard to be the standard unit of length.

2. After considering whether the measure of length should be defined by the whole length of the bar, that is to say, an *end-standard*, or by the distance between either two points or two lines marked upon the bar, a *line-standard* was adopted in preference.

3. For the material of the bar, gun metal or bronze composed of

Copper	16 parts
Tin	2½ „
Zinc	1 „

was adopted after a series of experiments by Mr. Baily, and was recommended by him as containing the properties most essential for the construction of a standard intended to last through many ages, viz., almost perfect immunity from rust, with proved elasticity and rigidity.

The test bar of this alloy, when loaded at the centre with 5¼ cwt., broke without bending.

4. The form of the standard to be a solid bar 38 in. long, and 1 in. square in section. The measure of a yard to be defined by the distance between two fine lines perpendicular to the axis of the bar, marked upon gold studs at the bottom of cylindrical holes drilled from the upper surface to the mid-depth of the bar.

The gun-metal, or bronze, thus adopted for the new standard, has since been known as "Baily's metal," and this designation is engraved upon the Imperial standard yard.

In order to select the most perfect specimen for the new standard of length, 40 line-standard yards were constructed of Baily's metal, and one of these was finally selected as the Imperial standard, not only from its representing, with the greatest precision, the assumed length of the lost standard yard, but also from the clearness of its defining lines, and from its general good workmanship. Four of the remaining yards nearest in length to the new standard were selected as Parliamentary copies, and deposited in the same places as the Parliamentary copies of the standard pound already mentioned; and the rest were in like manner distributed amongst different countries and public institutions in this country.

Several other similar line-standard yards were also constructed for experimental purposes, being accurately verified by Mr. Sheepshanks, and were disposed of in like manner, viz.

The defining terminations of these end-bars consist of a plug of agate, slightly conical and shrunk into a similar conical hole at each end of the middle axis of the bar. The ends of the bars are ground and polished in a spherical form, the centre of the spherical surface being the middle of the bar.

All the numerous comparisons of the standard yards were made by Mr. Sheepshanks in one of the lower cellars at Somerset House, under the apartments of the Royal Astronomical Society, where the new micrometrical comparing apparatus constructed for the purpose by Messrs. Troughton and Simms, was fixed.

A full description of the comparing apparatus will be given under head V. of Weighing and Measuring Instruments, and their Use.

The Commission for restoration of the standards having terminated their labours, recommended in their final report that the new imperial standards of the yard and pound be deposited at the Exchequer Office, there to be preserved under such regulations as to Parliament might appear fitting. In expressing their adherence to the recommendation of the Committee of 1841 that no reference should be made to natural elements for the values represented by the standards of weight and measure, they also recommended that so much of the Act 5 Geo. IV. c. 74, as provided for the restoration of the standards in the manner therein provided be repealed, and that the standards should in no way be defined by reference to any natural basis, such as the length of a degree of the meridian on the earth's surface in an assigned latitude, or the length of a pendulum vibrating seconds in a specified place. They considered the ascertaining of the earth's dimensions and the length of the seconds pendulum in terms of the standard of length, and the determination of the weight of a certain volume of water in terms of the standard of weight, as scientific problems of the highest importance, to the solution of which they trusted that Her Majesty's Government would always give their most liberal assistance, but they did not urge them on the Government as connected with the conservation of standards.

These recommendations were carried into effect by the Act of 1855, 18 and 19 Vict. c. 72, for legalising and preserving the restored standards of length and weight, sec. 1 of which repealed the provisions of the Act of 1824 concerning the restoration of the standards by reference to the pendulum and to the weight of a cubic inch of water.

Under the provisions of the Act of 1855, the imperial standards were deposited in 1855, in the office of the Exchequer. On the consolidation of the ancient Office of the Exchequer with the Audit Office in 1866, and the creation of the Standards Department of the Board of Trade, under the Standards Act, 1866, 29 and 30 Vict. c. 82, the custody of the imperial standards was transferred to the Warden of the Standards, the head of the new Standards Department, and the imperial standards are now deposited in a fireproof iron chest in the strong room in the basement of the Standards Office, which has been specially adapted for their safe preservation. Provision is contained in the Act for the comparison once in every ten years of the three Parliamentary copies of the imperial standards deposited at the Royal Mint, in charge of the Royal Society, and in the Royal Observatory, Greenwich, respectively, with the imperial standards of length and weight, and with each other. Under this Act new scientific duties were also imposed upon the Standards Department, the Warden of the Standards being charged with conducting all such comparisons, verifications, and other operations with reference to standards of length, weight, or capacity, in aid of scientific researches or otherwise, as may be required.

In connection with the question of the derivation of a standard unit of length from a natural constant to be found in the ascertained dimensions of the earth, it may be added that Sir John Herschel has pointed out the fact of the length of the polar axis having been determined, from the combined results of all the scientific measurements of arcs of the meridian, to be equal to 500,482,296 inches of our imperial standard yard, and that if one five-hundred-millionth part of the polar axis were adopted as a new standard unit, to be called the "geometrical inch," it would differ from the imperial inch less than one-thousandth part of an inch; a difference so small as not to be measured by any ordinary method, and only by the aid of the nicest scientific instruments. For all "ordinary practical purposes," the geometrical inch would be identical with the imperial inch; whilst for high scientific measurements for astronomical purposes, it would connect by an unbroken numerical chain the small units with which mortals are conversant in their constructions and operations with the great features of nature, and more especially with those greater units in the measurements of the universe with which astronomy brings us in relation. It would also produce a more exact ratio between our units of length and weight, the avoirdupois ounce being nearly a "geometrical ounce," or one-thousandth part of the weight of a geometrical cubic foot of distilled water. That is to say, whilst the existing legal weight of a cubic foot of distilled water is 997·136 ounces, the weight of a geometrical cubic foot of water would be 998·1 ounces. And as the imperial half-pint is the measure of ten ounces of distilled water, the ratios of these units of length, weight, and capacity would thus be brought within such practical limits of precision as would meet every possible requirement of commercial exigency.

III.—*Derived Units and Multiples and Parts of Imperial Standard Units.*

THE IMPERIAL STANDARD GALLON AND BUSHEL.

With respect to measures of capacity, the sole unit of all imperial measures of capacity, established by the Act of 1824 is the standard gallon, containing 10 lbs. avoirdupois of distilled water, weighed against brass weights in air at the temperature of 62° Fahr., the barometer being at 30 inches. From the imperial standard gallon is derived the imperial bushel of 8 gallons, the standard of capacity for dry goods commonly sold by heaped measure, or incapable of being striken. Various units of measures of capacity had been previously established in this country at different periods. In Magna Charta, three such units are recited, "there shall be throughout our realm, one measure of wine, one measure of ale, and one measure of corn." Of these, the most ancient known was the Winchester corn bushel, of the capacity of about 2150·42 cubic inches, together with the Winchester corn gallon of 272¼ cubic inches. We have no record of any other standard measures of capacity being actually constructed, until the standard ale gallon of 282 cubic inches was added by Queen Elizabeth, and the standard wine gallon of 231 cubic inches by Queen Anne. All these old standard measures were discontinued as legal measures in 1824, and the new imperial standard gallon of 272·274 cubic inches, and the bushel of 2218·191 cubic inches, constructed and verified under Capt. Kater's superintendence, have since continued to be the standard units of imperial measure for liquids and for dry commodities.

The Exchequer standards of the imperial gallon and bushel formed part of the complete series of secondary standards constructed and accurately verified under Kater's superintendence, in 1824. These standards, together with other secondary standards, subsequently legalised, have served for regulating all the commercial weights and measures of Great Britain and her colonies and dependencies from 1824 up to the present time. The Exchequer standards were transferred to the Standards Department of the Board of Trade in pursuance of the Standards Act, 1866.

H. W. CHISHOLM

(*To be continued.*)

THE FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE session of this young Association which has just been concluded at Lyons appears to have been altogether successful, and according to the Reports read the Association is in an exceedingly prosperous condition, both as to number of members, income, and the carrying out of the scientific aims which it has in view. The number of members who attended the Lyons Congress was very satisfactory. The capital fund at the end of 1872 was 136,464 francs, and the income for 1873 is expected to be 24,000 francs. One of the aims of the Association is to give an impulse to Science in the provinces, and, as we recorded some time ago, the members of the Association resident about Bordeaux have formed a local association, and it is hoped a similar result will follow in the case of each town where the yearly meetings are held. The Association has received invitations for its next session from various French cities, and it has been decided to hold the meeting of 1874 at Lille. M. Wurtz was elected President for the ensuing year.

The accounts which have come to hand are mainly concerned with the work done in the Medical Section. Last week we gave a few extracts from the Presidential Address of M. De Quatrefages, and shortly we hope to be able to give a *résumé* of the work done in the various sections, as well as of the more important public lectures. Meantime we shall give a brief sketch of the general work which has been done.

In the general meetings, Dr. Blanc, an Indian military surgeon, read an important paper "on the means of arresting the propagation of cholera," founded on experiments made by himself. M. A. Gaudry, Professor at the Jardin des Plantes, Paris, gave a lecture on a botanical subject. Dr. Bertillon also gave a lecture on "Demography," *i.e.* the Natural History of Society. M. de Lesseps talked in a familiar and pleasant way of the proposed railway across Central America. M. F. Papillon read a paper on the connection between the Sciences and Metaphysics, and the Abbé Ducrost gave a lecture on the Prehistoric Station of Solutré.

The part of the Congress which is undoubtedly the most attractive consists in the excursions and the public lectures; the former interest strangers, and the latter, members. Besides the special excursions organised by certain sections and parties of members, there have been three general excursions—one to the prehistoric station of Solutré; a second to the sides of the plateau of Les Dombes; a third to the mines and furnaces of Voulte-sur-Rhône, in Ardèche, and a fourth, which set out last Friday and was to last for two days, to Geneva and the shores of its lake.

There have been three public lectures: the first was given by M. Karl Vogt, of Geneva, on Volcanoes; the second by M. Janssen, on the Physical Constitution of the Sun; and the third by M. Aimé Girard, on the Recent Progress of Industry.

NOTES

THE final arrangements for the Bradford meeting of the British Association are as follows:—The first General Meeting will be held on Wednesday, Sept. 17, at 8 P.M. precisely, when Dr. Carpenter, LL.D., F.R.S., &c., will resign the Chair, and the President-Elect, Prof. W. A. Williamson, F.R.S., will assume the presidency, and deliver an Address. On Thursday evening, Sept. 11, at 8 P.M., a Soiree; on Friday evening, Sept. 19. at 8.30 P.M., a discourse by Prof. W. C. Williamson, F.R.S., of Manchester, on Coal and Coal Plants; on Saturday evening, Sept. 20, a Lecture on Fuel to working men only, by Mr. Siemens, F.R.S.; on Monday evening, Sept. 22, at 8.30 P.M., a Discourse on Molecules, by Prof. Clerk Maxwell, F.R.S.; on Tuesday evening, Sept. 23, at 8 P.M., a Soiree; on Wednesday, Sept. 24, the concluding General Meeting will be held at 2.30 P.M., and in the evening a Grand Concert will be given in St. George's Hall, at 8 P.M. The excursions on Thursday, Sept. 25, will be to Harrogate, Ripon, Studley, Bolton Abbey, Gordale Scarr, Malham, Clapham Caves, Settle Caves, and Ingleboro'. Lists and prices of lodgings, and other general information will be given, on application at the Local Secretaries' Office, Bradford.

IT is said that a portion of the immense wealth of the late eccentric Duke of Brunswick is to be devoted to the founding of a Faculty of Medicine in Geneva.

THE King of Prussia has conferred on Prof. Helmholtz the Order of Merit for Science and Art.

THE October number of *Petermann's Mittheilungen* will contain an account of Professor Nordenskiold's Arctic Expedition during 1872-3, in the direction of Spitzbergen, which has not, geographically, been very successful. The steamer *Solhem* reached Tromso on August 6, and the following telegram of that date has been received from Prof. Nordenskiold:—"Just arrived here, all well. My resolution to undertake another ice-journey towards the north after the sledge-journey round North-east-land, has been rendered impracticable through want of provisions, which has compelled us to return. Instead of this we have undertaken extensive deep-sea dredgings as well as botanical, magnetic, and geological researches. I bring with me, besides other from various formations, very important collections of Miocene flora, as well as of two formations which belong to an older geological period hitherto altogether unknown in the Polar regions. These collections throw new light upon the prevailing flora and the climate of former periods, as well as upon the changes which these have undergone."

ACCORDING to the report of the Meteorological Department an earthquake occurred at Nottingham at ten minutes to seven o'clock on Friday morning last.

PROF. PALMIERI stated in the Neapolitan papers on Aug. 12, that, according to observations on Vesuvius, new earthquakes may be expected.

THE Berlin medical journals record the death, from cholera, on August 20, of Dr. Otto Obermeier. His death is supposed to have been the immediate consequence of his intense devotion to Science. Having too great confidence in his power of resisting infection, in consequence of having not taken fever during his investigations on that disease, he kept in his bedroom pathological specimens taken from persons who had died of cholera, and also portions of their excreta; and it is believed that in this way he became infected. According to one account, he injected some blood from cholera patients into his own vessels. He was so devoted to his inquiry that, after he had become aware of the condition in which he was, he made some microscopic examinations on his own blood. His death occurred after an illness of seven hours, in the thirty-first year of his age.

THE late Mr. John Stuart Mill has left property to the amount of 14,000*l.* Of this he has left to any one university in Great Britain or Ireland that shall be the first to open its degrees to women, 3,000*l.*; and to the same University a further sum of 3,000*l.* to endow scholarships for female students exclusively. His copyrights he bequeaths in trust to Mr. John Morley, to be applied in aid of some periodical publication which shall be open to the expression of all opinions, and which shall have all its articles signed with the names of the writers.

THE planet No. 131, discovered by Prof. Peters, has been named Vala.

No. 1,952 of the *Astronomische Nachrichten* contains the following ephemeris of the one discovered by M. W. Tempel at Milan on July 3 last:—

1873	R.A. h.	m	s	South Declination. °	′
Sept. 4	2	7	17	14	38
,, 8	2	9	0	15	26
,, 12	2	9	53	16	12
,, 16	2	10	15	16	54
,, 20	2	9	52	17	31
,, 24	2	8	49	18	6

This ephemeris is by Signor J. V. Schiaparelli; it is for 0 h. Milan time. The same number of the *Astronomische Nachrichten* contains a short article upon this comet by Mr. L. Schulhof, assistant at the Observatory of Vienna, wherein he says, "It does not admit of doubt that we have here to do with a periodic comet of short revolution, the exact calculation of the orbit of which I shall enter upon without delay."

TWO new comets have been recently discovered; the one by M. Henry, at Paris, the other by M. Borelly, at Marseilles.

M. STEPHAN, the Director of the Marseilles Observatory, has succeeded in re-finding Biorsen's comet. The correction of Mr. Plummer's ephemeris is as follows:—

$$\text{R.A.} + 2^{h}\ 7^{m}$$
$$\text{Dec.} - 15'\ 8''$$

WE understand that the Board of Examiners for the mining district of South Durham, Whitby, and Cleveland allow candidates for examination, under the New Mines Regulation Act, to count one or two years passed at the College of Physical Science in Newcastle the same as the like period served under indentures to a mining engineer, or as one or two years' experience in a mining office. This offers a very great advantage to young men who intend to devote themselves to the profession of mining engineering.

THE following candidates have been successful in obtaining Royal Exhibitions of 50*l.* per annum for three years and free admission to the courses of instruction under the Science and Art Department:—Royal School of Mines, Jermyn Street, London: William Hewitt, aged 21, teacher; C. S. Fleming, 20, assistant teacher; Samuel Barratt, 22, assistant teacher. Royal College of Science, Dublin: Henry Louis, 17, student; Robert H. Reilly, 18, student; Thomas Arnall, 22, rule-maker.

A SUM of 500*l.* having been placed at the disposal of the Council of the Society of Arts, through Sir William Bodkin, by a gentleman who does not wish his name to appear, for promoting, by means of prizes or otherwise, economy in the use of coal for domestic purposes, the Council have decided to offer the following prizes: Five prizes for carrying out the purpose of the donor, each prize to consist of the Society's medal and 50*l.* Testing-rooms will be provided, in which the various competing articles may be tested in succession, each competitor having allotted to him in turn a room and chimney, for a limited period, where he may fix his apparatus for the purpose of its being tested by the judges appointed by the Society of Arts, the same to be removed when directed by the judges; such fixing and removal to be at the cost of the competitor. The competing articles must be delivered at the London International Exhibition, South Kensington, on December 1, 1873, with a view to their being tested, and subsequently shown in the Exhibition of 1874.

THE last number of the *Society of Arts' Journal* contains a Report on Cooking Apparatus at the International Exhibition, by Mr. G. W. Yapp.

IT is stated that a new Literary Review will be published at the beginning of next year, covering the same ground as the *Athenæum*, the *Academy*, and *Notes and Queries*. It will supply a regular weekly account of English and foreign literature, science, and learning, the fine arts and archæology, music, and the drama. It is added that the proprietors have purchased all rights in the *Fortnightly Academy Journal*, and intend to make that the scientific and learned part of the new paper, though whether under the name of the *Academy* or some other name is not yet determined.

A LIFE of Claparède, with an admirable portrait, precedes his posthumous work, entitled "Recherches sur la structure des Annélides Sédentaires," which is published as the new volume of the "Memoires de la Société de Physique de Genève."

WE have received a little pamphlet containing a very interesting account, by Mr. J. Logan Lobley, of the excursion of the Geologists' Association to the Malvern district during the 21st and five following days of last month.

WE have received Reports of the meetings of the Eastbourne Natural History Society for April 18 and May 23. At the former meeting, a paper by Miss A. Woodhouse was read on *Odoxa moschatellina*, and the Rev. A. K. Cherill read one "On Mosses." At the latter meeting the following papers were read:—"The Orchidaceæ, with special reference to the species found near Eastbourne," by Miss Hall and Miss A. Woodhouse; *Ceratostoma Helvellæ*, by C. J. Muller; and "The Alluvial Beds of the Wish," by the Rev. E. S. Dewick, F.G.S.

A CORRESPONDENT writes us that he has just obtained a specimen of quartz with gold found at Wanlockhead, Dumfriesshire. It is a fragment of a detached mass of quartz which weighed about ten pounds, throughout which gold was diffused. Gold has long been collected from the sand of some of the rivulets at Wanlockhead and Leadhills, but no instance was before known of gold having been found in its matrix. The specimen which our correspondent has contains about as much gold as might be equal to the third or fourth of a sovereign, along with brown iron ochre diffused over one of the surfaces of the quartz.

A DESPATCH from Havana, dated August 19, states that late advices from Lima and Peru report a serious accident had occurred sixty miles from that city. A body of earth, estimated at 10,000,000 cubic yards, fell from a mountain side into a valley, severely injuring a number of persons, and damming up a river, the water of which had risen 109 feet above its usual height. Engineers were of opinion that the water would soon burst its barriers, when it would rush towards Lima, sweeping everything before it, and submerging the lower portion of the city. Several towns in Chili had been greatly damaged by earthquakes.

As the result of a recent careful study of the drug *Pareira brava*, Mr. Daniel Hanbury has discovered that, instead of its being obtained from *Cissampelos pareira*, of the natural order *Menispermaceæ*, the genuine *Pareira brava* is the stem and root of a plant which he has identified with *Chondrodendron tomentosum* of Ruiz and Pavon. The drug of English commerce, however, is mostly of larger size than the root of *Chondrodendron* and is of doubtful origin, the structure of the wood being also that of the order *Menispermaceæ*.

UNDER the title of "On Coal at Home and Abroad," Messrs. Longman have recently published in one volume the following three articles, contributed to the *Edinburgh Review* by the Rev. J. R. Leifchild:—1. Consumption and Cost of Coal; 2. On the Coalfields of North America and Great Britain; 3. Fatal Accidents in Coal Mines. The republication of these papers at the present time is very opportune; they will be found to contain a great deal of information on the all-important "Coal question," as well as many interesting details concerning the working of coal mines and the character and condition of the miners.

ZOOLOGISTS will find in Dr. Theodore Gill's "Synoptical Tables of Characters of the Mammalia," prepared for the Smithsonian Institution of Washington, an excellent, concise, and accurate description of the characters of the families of the Mammalia, in a form more scientific and manageable than any yet published, at the same time that the merits of the most modern suggestions are fully weighed. The biography of the subject is also exhaustively treated.

THE Brighton Aquarium is an institution which all biologists undoubtedly look to as one from which much valuable information may be obtained on points connected with the habits and peculiarities of the animals which it has such advantages in retaining. The communications made public by its "Consulting Naturalist," however, are of a character very different from what we should expect from one so favourably placed. A fresh arrival is thus announced:—"One of the funniest little 'cusses' ever turned out of Nature's workshop, in the shape of a seal, made a bow to the public in the Brighton Aquarium a few days ago." This is followed, later on, by a *quasi* advertisement of the concert given in the building, in which the seal is playfully made to do duty as the butt for pun and slang quotation.

THE additions to the Zoological Society's Gardens during the past week include two Persian Sheep (*Ovis aries*), presented by Mr. W. H. Shirley; a Diamond Snake (*Morelia spilotes*) from New South Wales, presented by Mr. H. Frieland; two Robben Island Snakes (*Coronella phocarum*), presented by the Rev. G. H. Fisk; two Chubb (*Leuciscus cephalus*) and a Barbel (*Barbus vulgaris*) presented by Mr. E. S. Wilson; two Ring-tailed Lemurs (*Lemur catta*) from Madagascar; a King Parakeet (*Aprosmictus scapulatus*) from New South Wales; a Black Cuckoo (*Eudynamys orientalis*) from India, purchased; a Weeper Capuchin (*Cebus capucinus* and a White-throated Sapajou (*C. hypoleucus*) from America, deposited.

A POSSIBLE NEW METHOD OF ELECTRICAL ILLUMINATION

IT will be in the recollection of the readers of this *Journal*, that, in April last, an analogy was pointed out between sunlight and the electric light, and that certain conditions were therein indicated as being most favourable to that particular development of light which would best bring out the separation of the power producing the light from the place of its manifestation. Those conditions were the employment of magneto-electricity, and the use of a closed incandescent conductor in an atmosphere which would not oxidise or otherwise affect the durability of the light-producing material. From the quotation from the Russian paper *Golos* which follows, it will be seen that the results anticipated are even now in the course of realisation, and all that practical men can do is, to wish the plan the success it seems to deserve, and to wait the result of the further exhibitions of its power in London and other places more accessible to the Western nations than St. Petersburg:—

"On Tuesday the 8-20 of May, a most interesting trial was made for the first time in public at the Admiralty House, St. Petersburg, under the auspices of Messrs. S. A. Kosloff and Co., the proprietors of the patent, of a new system of lighting by electricity, the invention of Mr. A. Ladiguin, of that town.

"Owing to the restricted space in the hall made use of on this occasion, the number of spectators was necessarily limited, but still they consisted of more than a hundred specialists from different countries, representatives of science, honourable visitors, and many reporters, who were all deeply interested, and unanimously decided that the trial was really successful.

"Up to the present time, as is well known, the electric light has been used only for lighthouses, as an electric sun illumination for signals, or on the stage, where a strong light may be required without regard to cost; but thus far it has been quite impossible to employ it for lighting streets or houses.

"By the old method the electric spark was passed between two points of charcoal, each attached to a copper wire connected with an electro-magnetic machine.

"The disadvantages attending this mode consisted in the facts that, for each light a separate machine was required, and that the light so obtained, although very powerful, was impossible to be regulated, besides being non-continuous, owing to the rapid consumption of the charcoal points from exposure to air.

"All these difficulties Mr. A. Ladiguin has tried and apparently overcome most successfully.

"By his newly-invented method, only one piece of charcoal or other bad conductor is required, which being attached to a wire connected with an electro-magnetic machine is placed in a glass tube, from which the air is exhausted, and replaced by a gas which will not at a high temperature combine chemically with the charcoal. This tube is then hermetically sealed, and the machine being set in motion by means of a small steam-engine, the charcoal becomes gradually and equally heated, and emits a soft, steady, and continuous light, which, by a most simple contrivance, can be strengthened or weakened at the option of those employing it; its duration being dependent solely on the electric current, which of course will last as long as the machine is kept in motion.

"Taking into consideration the fact that one machine, worked by a small three-horse power engine, is capable of lighting many hundreds of lanterns, it is evident what an enormous advantage and profit could be gained by the illumination of streets, private houses, public buildings, and mines with the new electric light. In the latter it must prove invaluable, as no explosion need ever be feared from it, and these lanterns will burn equally as well under water as in a room.

"Without mentioning the many advantages this mode of illumination has over gas, which by its unpleasant odour and evaporation is slowly poisoning thousands of human beings, and from which explosions are frequent, we can state that by calculations made, this electric light can be produced at a fifth of the cost of coal gas.

"We hope shortly to place before the public more complete particulars, as well as reports of further experiments which are proposed to take place in Vienna, Paris, and London.'

* From the *Journal of the Society of Arts*.

*GROWTH OR EVOLUTION OF STRUCTURE IN SEEDLING PLANTS**

THE continuous absorption of oxygen, and formation of carbonic acid, is an essential condition of evolution of structure, both in plants and in animals.

The above proposition in so far as it relates to animals will probably be admitted by all; the opposite opinion is, however, commonly held as regards plants; yet we propose to show that in these organisms, as in animals, growth as applied to evolution of structure, or organisation of material provided, is inseparably connected with oxidation.

The discussion of the proposition in question necessarily involves a preliminary view of the character of the gases exhaled from various plants. Commencing with the lower organisms as fungi, the uniform testimony is that these plants at all times expire carbonic acid, while it is chiefly in the higher plants, and especially in those which contain chlorophyll or green colouring matter, that carbonic acid is absorbed and oxygen exhaled. The inquiry then in reality narrows itself down to the examination of the growth of chlorophyll-forming plants.

Regarding these plants the statement is made and received, that they change their action according as they are examined in the light or in the dark, exhaling oxygen under the first condition, and carbonic acid under the second. Various explanations of this change of action have been given, that generally accepted accounting for it on the hypothesis of the absorption of carbonic acid by the roots, and its exhalation by the leaves when light is no longer present.

The change, on the contrary, appears to arise out of the fact that two essentially different operations have been confounded, viz: the actual growth or evolution of structures in the plant, and the decomposition of carbonic acid by the leaves under the influence of the light, to provide the gum or other materials that are to be organised. These two factors are separated by Prof. J. W. Draper in his discussion of the conditions of growth in plants. We propose to show that by adopting this proposition of two distinct operations in the higher plants, all the apparent discrepancies regarding the growth of these plants are explained.

The growth of seedlings in the dark offering conditions in which the act of growth or evolution of structure is accomplished without the collateral decomposition of carbonic acid, I arranged two series of experiments in which growth under this condition might be studied and compared with a similar growth in the light. That the experiments might continue over a sufficient period of time to furnish reliable comparative results, I selected peas as the subject of trial, since these seeds contain sufficient material to support the growth of seedlings for a couple of weeks.

To secure as far as possible uniformity of conditions between the dark and light series, and also to facilitate the separation, cleansing and weighing of the roots, each pea was planted in a glass cylinder, one inch in diameter and six inches long. These cylinders were loosely closed below by a cork, and filled to within half an inch of the top with fine earth or vegetable mould. They were then placed erect in a covered tin box or tube stand in such a manner that the lower end dipped into water contained in the box, while the whole of the cylinder except the top was kept in the dark. Thus the first condition for germination, viz., darkness, was secured; the second, warmth, was supplied by the external temperature, which varied from 70° to 80° F., while regularity and uniformity in the supply of moisture in both series was secured by having a box of cylinders or tubes for each and keeping the level of the water the same in both. The supply of oxygen was also equal and uniform, since the upper part of each tube presented a similar opening to the air.

Thus prepared, one box containing five cylinders was kept in a dark closet, while a second, similar in all respects, was placed in a window of an adjoining room, where it was exposed to direct sunlight five or six hours every day. To each tube a light wooden rod thirty inches in length was attached, and on this the growth of the seedling was marked every twelve hours. The hours selected were 7 A.M. and 7 P.M. I thus obtained the night and day, or dark and light growth of every seedling, as long as those in the dark grew. The seeds were planted on June 1st, and appeared above the ground on June 6th, when the measurements were commenced. In each series one seed failed to germinate; the record, consequently, is for four plants in each, and the history of the evolution of structures is as follows:

Evolution of Structure in the Dark.—In Table I. the seeds are designated as A, B, C, D, and each column shows the date on which leaves and lateral growths appeared. These constitute periods in the development of the plants, which are indicated by the number 1, 2, 3, 4, 5, 6. The weight of each seed is given in milligrammes.

Table I.—*Seedlings grown in the Dark*

		A.	B.	C.	D.
Weight of seed		431	466.	456.	500.
Period	1,	7th day.	7th day.	7th day.	7th day.
,,	2,	8th ,,	9th ,,	9th ,,	8th ,,
,,	3.	10th ,,	10th ,,	11th ,,	10th ,,
,,	4,	12th ,,	12th ,,	13th ,,	12th ,,
,,	5,	14th ,,	15th ,,	15th ,,	14th ,,
,,	6,	17th ,,	18th ,,	18th ,,	17th ,,

A glance at the above shows the uniformity as regards time with which the structures were evolved in each plant. It also indicates for each plant an equality in the number of periods of evolution, viz., 6, notwithstanding the difference in the weights of the seeds, and suggests that the power of evolution of structure in seedlings resides in the germ alone.

The character of the evolution in the six periods shows a steady improvement or progression.

In the first, the growth consists in the formation close to the stem of two partially developed pale yellow leaves.

The second period is similar to the first, except that the leaves are a little larger.

The third presents a pair of small yellow leaves close to the main stem, from between which a lateral stem or twig about one inch long projects, and bears at its extremity a second pair of imperfectly developed yellow leaves, from between which a small tendril about a sixteenth of an inch long is given off.

The fourth resembles the third, the lateral twig being longer, and the tendril three times as long as in the third.

The fifth is like the fourth, except that the tendril bifurcates.

The sixth is similar to the fifth, except that the tendril trifurcates.

Stem, leaves, twigs, tendrils of various degrees of complexity, all are evolved by the force pre-existing in the germ without the assistance of light.

Evolution of Structure in the Light.

Table II.—*Seedlings grown in the Light*

		E.	F.	G.	H.
Weight of seed.		288.	426.	462.	544.
Period	1,	—	6th day.	—	6th day
,,	2,	7th day.	7th ,,	7th day.	7th ,,
,,	3.	8th ,,	8th ,,	8th ,,	9th ,,
,,	4,	12th ,,	9th ,,	10th ,,	10th ,,
,,	5,	15th ,,	11th ,,	14th ,,	12th ,,
,,	6,	— ,,	13th ,,	— ,,	14th ,,

Table II. was obtained in the same manner as Table I, the columns representing the days on which lateral growths and leaves appeared. Though there is not the same uniformity as in Table 1, the periods are identical in both as regards the visible character of the evolution. Nothing appears in the second that did not pre-exist in the first, and in the case of the seeds E and G the evolution is even deficient as regards the first and the sixth periods.

While the general character of the evolution in both series is similar, certain minor differences exist. In II. the leaves and tendrils are many times larger than in I., and they, with the whole plant, are of a bright green colour, instead of the sickly pale yellow of I.; but the light has not developed any new structure; it has only perfected those which pre-existed, and converted other substances into chlorophyll which is not an organised body.

Not only did the plants in the two series present similarities in evolution of structure, but the average weight of dry plant in each was very nearly the same, for:

455 of seeds in the dark produced 184 of dry plant, while
455 ,, light ,, 215 ,, ,,

A comparison of the parts below the ground with those above (both being dried at 212° F.) shows that the proportion of root to total weight of plant was also nearly identical, being,

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* From Silliman's American Journal of Science and Art.

25 of root for 100 of plant in the dark, and
23 „ 100 „ light.

The close similarity in the evolution of visible structure in the light and in the dark, the small difference in the total weights of the plants grown in the same time in both series, and the close approximation in the proportional weight of root to plant, all justify the conclusion, that the growth in darkness and in light closely resemble each other, and that it is proper to reason as regards the nature of the action from the first to the second.

Another interesting fact which lends support to the opinion that the process of growth in seedlings, developed in the dark is very similar to that occurring in those grown in the light, is the character of the excrement thrown out by the roots. It is well known that many plants so poison the soil that the same plants cannot be made to grow therein until the poisonous excretions from the roots of the first crop have been destroyed by oxidation. In the case of peas this poisoning of the soil takes place in a very marked manner, and I have found that in the pots in which peas have been grown in the dark, the soil is so poisoned by the excrements from the roots that a second crop fails to sprout. Does it not follow, that since in the two series with which I experimented, the excrements from the roots possessed the same poisoning action, the processes in the plants from which these excrements arose must have been similar?

There remains an important argument concerning which nothing has thus far been said. It is to be derived from the consideration of the rate of growth in the light series during various periods of the day of twenty-four hours. If the evolution of structure in a plant in daylight is the result of the action of light, that evolution should occur entirely, or almost entirely during the day. If, on the contrary, it is independent of the light, it should go on at a uniform rate as in plants in the dark.

For the elucidation of this portion of the subject, I present the following tables; the first of which shows the growth by night, 7 P.M. to 7 A.M., of the seedlings in the dark series, compared with their growth by day, 7 A.M. to 7 P.M. The measurements were taken from the sixth to the twentieth of the month, the day on which growth ceased in the dark series:—

Table III.—*Seedlings grown in the dark*

	Night growth.	Day growth.
No. 1	12¼ inches.	14 inches.
„ 2	13¼ „	13 „
„ 3	11¾ „	11¾ „
„ 4	12½ „	11¼ „
Average,	15¾ „	12½ „

The total day growth and night growth under these circumstances are nearly equal, though there is a slight excess in favour of the night, amounting, as the table shows, to ¼ of an inch in 12 inches.

In Table IV. the growth of the light series is given in the same manner, by day and by night, for the same time, viz., to June 20. The thermometric and hygrometric conditions in both series were very similar, as indicated by the dry and wet bulb thermometers suspended in the vicinity of each set of tubes:—

Table IV.—*Seedlings grown in the light*

	Night growth.	Day growth.
No. 5	3½ inches	4 inches.
„ 6	8 „	7 „
„ 7	5¼ „	4½ „
„ 8	9½ „	8¼ „
Average,	6½ „	6 „

In the average, and throughout the table, with a single exception, not only is the uniformity in the rate of growth during the day and night shown, but the slight excess of night growth found in the series kept in the dark is likewise copied. We must therefore accept the conclusion that the act of growth or evolution of structure is independent of light, and that the manner of growth during the day is similar to that at night.

It will be noticed that the total average height attained in the light is only about half that in the dark series. The explanation of this we have already seen in the fact that in the former the leaves and tendrils were much larger than in the latter, while the dry weights were nearly the same. The material of the seed in the light series was consumed in extending these surfaces, while in the dark series it was spent in lengthening the stem.

Having established the continuous character of growth in seedlings, and the similarity of rate and nature of the process by night and by day, and admitting that at night plants throw off carbonic acid, it is not improbable that this carbonic acid arises, not from mechanical absorption by the roots, and vaporisation by the leaves, but as a direct result or concomitant of the act or process of evolution of structure.

To put the matter in the clearest form, let us first understand what growth is. It appears in all cases to consist in the evolution or production of cells from those already existing. According as the circumstances under which the cells are produced vary, so does the tissue ultimately produced vary. Cells formed in woody fibre become wood. Cells formed in muscle in their turn form muscles, but the starting point of the process in every instance is the formation of new cells.

If now we examine the evolution of cells under the simplest conditions, as, for example, in the fermentation that attends the manufacture of alcohol, we find that with the evolution of the torulæ cells carbonic acid is produced. The two results are intimately connected, and it is proper to suppose that since the carbonic acid has arisen along with the new cells, the latter operation must in some way involve a process of oxidation. Accepting the hypothesis that oxidation is attendant on these processes of cell growth under the simplest conditions, we pass to the examination of what occurs in the lowest forms of vegetable organisms found in the air.

The fungi, and indeed all plants that are not green, with a few exceptions, exhale carbonic acid and never exhale oxygen. In this case, in which cell production often occurs with such marvellous rapidity, the carbonic acid must have arisen as a consequent of the cell growth. It is improbable that it has been absorbed by roots and exhaled from the structures, either in these plants or in those produced during fermentation. In the latter there never are any roots, and in the former, even where roots are present, they bear a small proportion to the whole plant. The quantity of moisture exhaled by such growths is also insignificant, and out of proportion to the carbonic acid evolved. We must, therefore, in this case decline to accept the root-absorption hypothesis, and admit that the carbonic acid has arisen as a result of the cell growth in the plant.

Passing to the chlorophyll-bearing plants, we find that in the Phanerogamia it is only the green parts that at any time exhale oxygen, and then only under the influence of sunshine. The other parts of the plant above the ground, that are not green, viz., the stem, twigs, flowers, &c., are at all times, day and night, exhaling carbonic acid. The whole history of the plant, from the time the seed is planted, to its death, is a continuous story of oxidation, except when sunlight is falling on the leaves. The seed is put into the ground, and during germination oxygen is absorbed and carbonic acid exhaled. If the seedling be kept in the dark, oxygen is never exhaled, carbonic acid is, and the plant not only grows, but all visible structures except flowers are formed in a rudimentary condition. In the light the growth during the night time is attended by the evolution of carbonic acid, while during the day time the bark of the stem and branches is throwing off carbonic acid. When flowers and seeds form, the evolution of carbonic acid attending this highest act of which the plant is capable, is often greater than that produced at any time in many animals.

Everything in the history of plants, therefore, tends to show that the evolution of their structures is inseparably attended by the formation of carbonic acid; and it seems impossible, when we consider the evolution alone, to arrive at any other opinion than that already expressed—that, all living things, whether plant or animal, absorb oxygen and evolve carbonic acid, or some other oxidised substance, as an essential condition of the evolution of their structures.

J. C. DRAPER

SCIENTIFIC SERIALS

THE first number of the *Zeitschrift fur Ethnologie* for 1873, opens with an interesting paper by Dr. George Schweinfurth on the Monbutta Tribes of Central Africa, whose name and existence have hitherto been unknown to us. Dr. Orl and M. Jules Poncet had shown that there were important streams south of the Miam-Miam Territory, which took a westerly course, and that the banks of the most considerable of these rivers were occupied by a brown-skinned race differing widely from the contiguous negro tribes, both in colour and in civilisation. These are the Monbuttas, known also to the ivory-traders as the Guruguri, in allusion to their practice of boring their ears. Their country, which Dr. Schweinfurth visited in 1868, and where he remained for five weeks under the special protection of the king, Munsa, is a densely populated district lying between 3 and 4 N. lat., and 28 and 29 E. long., and bounded on the north by the Kibali, a copious stream which unites with the Gadda, and under the name of Uelle receives in its passage through the Miam-Miam country a number of other streams, that serve as feeders to Lake Tsad. The country of the Monbuttas, lying at an elevation of from 2,500 to 3,000 ft. above the level of the sea, consists of an ever-varying alternation of gently swelling hills and well-watered valleys, alike rich in palms and bananas, and every other form of luxuriant tropical vegetation. In this earthly paradise where Nature spares man the burden of labour, the people, although living under an organised system of government, and showing extraordinary skill in working metals and in other arts, are habitual cannibals. This is not from want of animal food, as elephants, buffaloes, antelopes and wild swine abound, but whatever the cause may be, the fact is undisputed that the cannibalism of the otherwise gentle Monbuttas exceeds that of any other known African nation, and is systematically gratified at the expense of the more degraded blacks living beyond their frontiers, whom they seize and carry away, driving their captives before them like a herd of sheep, and slaughtering them as they need them. The young children and the fattest individuals are kept for the royal kitchen, where the flesh is dried and prepared with capsicums and many savoury fruits for the king, Munsa, whose numerous wives have to take it in turn to cook for him. The power of the king is supreme, and it would appear that the land of the Monbuttas may rank as one of the most important monarchical states of Central Africa. In race the people seem to approximate to the Fulbe, and in language to the north equatorial African group. They recognise one supreme being, appear to have no outward symbols of worship, and practise circumcision.—Dr. P. Langerhans has a paper in this number on the anatomical features of interest belonging to a series of facial and cranial measurements, with the corresponding photographs, taken at Jerusalem from among the mixed population of Khurds, Armenians, and Negroes (from Dâr). As a contribution to human comparative anatomy the paper will be found useful.—Those interested in the study of the prehistoric remains of Holland and the Low Countries generally will find much serviceable matter in a paper by Dr. Friedel, who points out the distinctions between the Frisic-Germanic and the Celtic-Batavian remains, and passes in review the collections preserved in the various Dutch museums, of which that of Leyden is the most valuable in an ethnological point of view.

Poggendorff's Annalen der Physik und Chemie, No. 5, 1873.—This number contains several papers on electricity. Dr. Hermann Herwig investigates the influence of free electricity at the surface of electrodes, on electro-dynamic phenomena. His experiments were made with a delicate electro-dynamometer, in which the deflections of the bifilar and multiplier coils were compared, the electro-motive force and resistance being varied.—A paper by M. Edlund treats of the chemical action of the galvanic current and the distribution of free electricity on the surface of an electrode. The author applies his theory (of electricity being a phenomenon of the luminiferous ether), to the decomposition of water by a current, and institutes a comparison between what occurs in a ring-tube in which a gas is forced into circulation from a certain point, with the phenomena in a galvanic circuit. In another note the same author opposes von Bezold's explanation of "disjunction currents," which he thinks are due to an electro-motive force in the voltaic arc itself, not to a difference in tension between the electrodes.—M. Wullner describes experiments confirming his former assertion (questioned by Schuster) that nitrogen, in Geissler tubes, gives both a band and a line spectrum. A valuable series of experiments on heat consumption in the solution of salts, and the specific heats of salt solutions is detailed by Dr. Winkelmann, who here extends the previous work of Graham and Person on the subject.—There are also papers on the change of volume of solid substances through the formation of chemical combinations of the same aggregate state (W. Muller), on the pole-points of a magnet (Riecke), on the dynamical principle of Hamilton in thermo-dynamics (Szily), on a new mode of exhibiting metallic spectra (Edelmann), and one or two others.

The July and August numbers of the *American Naturalist* contain, among others, the following papers :—Dr. Elliott Coues discusses the relationship between the Prairie Wolf, or Coyoté (*Canis latrans*), and the common dog, taking a pointer as his type, which is much of the same size. The physiognomy of the former is said to be between that of a wolf and a fox, "but more doggy than either." Its affinities with the dog are shown to be extremely close.—Mr. T. M. Trippe gives reasons for instances of irregular migrations of birds, showing that some depend on human interference, and changes in climate, and others are as yet unexplained.—Prof. Verril describes a new species of Octopus (*O. bairdii*) from the bay of Fundy. It is somewhat related to *O. groenlandicus*, but differs in the hectocotylised arm being longer and otherwise different.—Alexander Agassiz, in a fully illustrated article, gives reasons in favour of the supposition that the pedicellariæ and spines of Echinodermata are only modifications of a single type form, to suit different purposes in the animal's economy.—Prof. W. J. Beal, on the phyllotaxy of cones, shows that the well-known laws of phyllotaxis are very general, nevertheless there are exceptions to them, well marked among some cones, as is proved by the author's examination of a very large number from the Norway spruce, in which [illegible] and [illegible] were the common fractions.—Mr. A. S. Packard, jun., treats of the distribution of Californian moths, bringing information on their peculiarities to bear on Prof. Gray's work on the distribution of Californian plants.—Dr. Theodore Gill has a paper on the status of Aristotle in systematic zoology, in which he gives very cogent reasons against that great philosopher having the knowledge of the principles of zoology which is ascribed to him by some. He concludes that "there is not the slightest evidence of any recognition of what is now understood by classification in any of the extant treatises of Aristotle on animals, and the systems framed to embody his generalisations have been constructed from isolated sentences wrested from their context, and simply reflect the framer's notions or his ideas as to what Aristotle might have supposed."—Prof. Bessey notes the sensitiveness of the stamens of *Portulaca* and *Claytonia*.

Mittheilungen der Naturforschenden Gesellschaft in Berne, 1872.—Prof. Dor has an article, in this number, on colour blindness. Various experiments are described; the method most preferred having been that of viewing spectral colours with a polarisation prism. The author rejects the Young-Helmholtz theory, which, as is known, supposes three colour-perceiving elements in the retina, those for perception of red, those for green, and those for violet (or blue). Among his objections are these : absence of anatomical proof ; distinct vision of many of the colour blind ; the spectrum as observed by two persons, brothers, who had no perception of red or violet, was of normal length ; all the pathologically colour blind suffered from atrophy of the optic nerve through cerebral or spinal injuries ; in these cases, the fibrous and cellular layer of the retina, and the optic nerve, to the brain, were atrophied, but not the rods and cones ; in retinal disease, on the other hand, the perception of colours *is not perverted, though diminished. He concludes that colour* blindness is a cerebral affection.—A note by Dr. Adolph Vogt treats of the drainage of towns, in view of a faulty state of things at Berne.—The action of Buss's new governor is discussed in a paper of some length by the inventor.—Dr. A. Forster communicates a note on the falling stars of November last, also meteorological observations at the Berne Observatory during 1872. From the curve of daily temperature variations at Berne it appears that these are sometimes considerable, *e.g.* 18·6° C. in 24 hours, a fact of significance for health.—We may further note, in this number, some contributions to local botany, by Dr. Wydler.

SOCIETIES AND ACADEMIES

BELGIUM

Royal Academy of Sciences, June 7.—M. Quetelet presented a note on the solar eclipse of May 26, 1873.—M. Montigny gave the results of a second series of experiments made on the spire of Antwerp Cathedral, in which he determined barometrically the heights at several points, in winds of different direction and velocity. His tables show a difference between the calculated height and the real height, the latter being greater for winds of the eastern semicircle, while the former is greater for westerly winds. In north and south winds, and those closely neighbouring, the heights measured both ways closely agreed. The differences between true and barometric altitude for the same gallery increase regularly, but in contrary directions, from the meridian to the azimuths east and west, when they each attain their maximum value. The height, barometrically measured, increased, as a rule, with the velocity of the wind. No connection was demonstrable between barometric height and inclination of wind. Observations at Namur and Brussels are compared with those at Antwerp, and show a cycle like that just described, only the regions to which the maximum and minimum (barometric) altitude correspond are, at these places, in the contrary direction to those at Antwerp.—M. Melsens communicated a paper on the effect of reducing alcoholic drinks to very low temperatures. A liquor like brandy may be cooled to—60° C. without being painfully cold to a person taking it. From the phenomenon of congelation in ordinary and sparkling wines, M. Melsens seeks to show how wines and beer also may be improved by application of cold.—M. Louis Henry described researches on the etherised derivatives of alcohols and of polyatomic acids; also on propargylic compounds.—M. de Selys Longchamps made a third addition to his "Synopsis of the Gomphines," of which he can now enumerate 188 species (seventeen of these being new), arranged in forty-three genera and subgenera.—M. Van Beneden gave a summary account of results from a voyage to Brazil and La Plata. His main object had been to study the fauna of the American coast, and specially of Rio. He describes the frequent formation of lagoons by the deposit of a transverse bar separating the water of a bay from that of the sea. Fresh water continually entering such lagoons, their saltness disappears, and an interesting question was, how had the original ocean fauna, here enclosed, been affected by the change of physical conditions. M. Van Beneden made various dredgings in the bay of Rio (in which the tidal change of sea-level is very small), and in these lagoons, and promises future communications on the subject. He mentions having found in some lower forms of Crustacea (*Lernanthropidæ* and *Clavellina*) a double circulatory system like that in Annelides. Besides the lacunar system, in which circulates a colourless liquid having white globules, there is a complicated system of vessels with proper walls, containing red blood without globules. There is no connection; the two liquids do not mix. The colouring matter is hæmoglobin. The branchiæ and trunk, alternately contracting and dilating, put the liquids in circulation. The author also mentions having dissected a lamantin (disinterred for his benefit), and a dolphin, and describes exceptional features in both. The paper gives several interesting zoological facts.

July 5.—M. Quetelet read a paper on the calculation of probabilities, applied to the science of man; reviewing recent progress of statistical science in this direction, and giving numerical results in the case of stature and mortality.—M. Van Beneden presented two coloured drawings of Cetacea captured at the Cape of Good Hope. He thinks zoologists have too little regarded the system of coloration in such animals, and his remarks bear chiefly on this.—M. L. Henry communicated a paper on diallylic compounds, being part of a series of researches on glyceric derivatives.—M. Swarts followed with a note on some properties of pyrocitric acids.—M. Spring communicated some facts with reference to the oxygenated compounds of sulphur.

PARIS

Academy of Sciences, Aug. 18.—M. Bertrand, president, in the chair.—The following papers were read:—Fourth note on guano, by M. Chevreul. The author has found that the crystallisable matter C, described in his late notes, is an ammonia salt, and that the other body insoluble in cold water is a very complex mixture containing acid. He gave no further details.—Direct demonstration of the fundamental principles of thermodynamics, by M. A. Ledieu.—On the movements of the *Phylloxera* from place to place, by MM. J. E. Planchon and J. Lichtenstein.—M. de Lesseps demanded the appointment of a Commission by the Academy to examine his project of a Central-Asian railway.—M. Daubrée communicated a letter from Mr. Nordenskiold, giving an account of the discovery, in recently fallen snow, of a carbonaceous snow containing metallic iron. This was first found at Stockholm; but the author, fearing that the powder might be due to the soot of the city, wrote to his brother, then in the centre of Finland, to collect snow there. The results were the same, and Mr. Nordenskiold has obtained sufficient for a quantitative analysis which he proposed to make during the coming winter.—Researches on secondary ascending currents, and their application (continuation), by M. G. Planté.—A description of the cryptograph, by M. Pélegrin.—On algebraic left-handed curves, by M. Piquet.—Experimental researches on explosives, by MM. Roux and Sarrau.—A new method of estimating ammonia, organic nitrogen, and nitric acid in waters, soils, and manures, by M. Pinggari. The author proposed to convert all nitrogenous bodies into ammonia and nitric and nitrous acids by acting on them with a mixture of argentic chloride and potassic hydrate, and then converting the oxidised nitrogen into ammonia by nascent hydrogen. He proposed to estimate the resulting ammonia by Nessler's process, except when below 0·00001 grm., when he proposes a special reagent, composed of two drops of phenol and 5 or 6 c.c. of hypochlorite of soda, which gives a fine blue-violet colouration to ammoniacal liquors.—On the hydrochlorate of terpene, and on the isomerism of the bodies having the formula $C_{10}H_{16}$, HCl, by M. Riban.—On the variations of hæmoglobin in the zoological series, by M. Quinquand.—On the variations of the urine under the influence of caffeine, coffee and tea, by M. Rabuteau.—On the zoological position, &c., of the parasitic Acarians known as *Hypopus*, by M. Mégnin.—On a deposit of silicified vegetables in the coal basin of the Loire, by M. Grand'Eury.—On the existence in the quaternary period of a large glacier in the mountains of Aubrac (Lozère) by M. G. Fabre.—Note on the meteors of November 27, 1872, by M. Ch. Dufour.—On the meteors of August 9 and 10, by M. F. Tisserand.—A note on the same subject, by M. Chapelas, concluded the business of the session.

August 25.—M. Bertrand in the chair.—The following papers were read:—On Zöllner's theory of solar scoriæ as being the cause of spots, by M. Faye. The author observed that this theory as recently developed in a communication to the Royal Saxon Academy agrees better with the known facts of the motions of the spots than does Secchi's eruption theory.—On the polar planimeter, by M. H. Resal.—On the thoracic and abdominal phosphorescent organs of the cocuyo of Cuba, by MM. Ch. Robin and A. Laboulbène. The systematic name of this insect is *Pyrophorus noctilucus* (*Elater noctilucus* L.) Direct demonstration of the fundamental principles of thermodynamics, part vii., by M. A. Ledieu.—On the rapidity of reproduction in the *Phylloxera*, by M. Lichtenstein.—On a principle of union in universal chemistry, as applicable to organic chemistry, by M. E. Martin.—A letter was received from M. Wolf announcing the discovery of two new comets by MM. Borelly and Paul Henry.—On the spectrum of comet III., 1873, by MM. Wolf and Rayet.—On the spectrum of the solar atmosphere, by M. G. Rayet. The author announces the discovery of the fact that the least refrangible of the two D lines is longer than the other, as he saw the former reversed when the latter was invisible.—Twelfth note on the effects of barometric changes on life, by M. P. Bert.—On hay-fever, by M. E. Decaisne. The author asserted that this disorder has no actual existence as a separate disease.—Experiments on the scolex of *Tænia mediocanellata*, by M. Saint-Cyr.—On the movements of the stamens in *Ruta*, by M. G. Carlet.

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THURSDAY, SEPTEMBER 11, 1873

THE ENDOWMENT OF RESEARCH

VI.

AMONG the difficulties which are likely to impede the ready realisation of the object to which attention has been drawn, there remains one which will always be most keenly felt by those who have devoted the most thought to the question. Beneath the word "Science" there lurks a distressing ambiguity, which, though it may not force itself upon the attention of the devoted students of any particular branch, is always arising when the general claims of scientific study come on for discussion. For our present purpose it is particularly important to attach that meaning to the word which, while best justified by usage, is also most calculated to conciliate good will from all quarters.

It will hardly be denied that the name primarily belongs to those sciences called by way of distinction "natural," in the name of which this journal is conducted, and which therefore it is needless to enumerate here; and that the name is thence transferred, by reason of analogies of varying degrees of strength, to those other branches of knowledge which either in their logical methods or positive results approximate to the standard of the physical sciences. Although it would be presumptuous to attempt to lay down with exactness the line which must somewhere exist between scientific and unscientific knowledge, it must yet be always necessary to treat with much suspicion the claims of mere erudition and of social theorising to be admitted to the honoured name. The old-fashioned reputation of the grammarian or the divine, and the modern popularity of practical reformers, are neither of them grounds on which to found a title to national endowment. The unprofitable studies for which the Universities were once famous have for centuries been abundantly rewarded, and the applause of a crowded congress is ever ready to acknowledge the merits of a novel speculation in Sociology. It is not unnatural that those who know by hard experience what Science really is should jealously uphold the dignity of their pursuits, and point with pride to the innumerable advantages which mankind within the last century has reaped from their labours: but, on the other hand, the warning is not unneeded at the present day that the field of the physical sciences is not equal in extent to that which all scientific knowledge can comprehend, and that the appeal to utility may be turned into a fallacious argument. Yet further, it may be urged that those among the sciences which most attract the public attention at the hands of an accomplished experimentalist, and of which the direct practical applications are manifest to all, are least in immediate want of support from national endowments. It is for the languishing departments of Science, which have not been popularised, and of which the results have not yet been turned to commercial value, that the advantages of endowment are most required. As soon as ever the main principle of these articles is publicly recognised, the more advanced and most useful are certain to obtain sufficient care, but it is for the more backward and the least profitable that the need of help is most urgent.

It may be reasonably expected that the Universities, as their traditions become modified under the influence of the public demands, will be disposed to accept the duty of endowing scientific research under the limitations above indicated. They can have no antecedent prejudice in favour of those branches of science which either attract the most spectators, or the greatest number of self-interested students. They have always refrained with anxiety both from bidding for popularity, and from preparing their pupils for the technical business of life. Their historic position also, and the peculiar responsibilities which they cannot but feel, will cause them to interpret Science in a liberal fashion. For these reasons it is confidently hoped that, while they cannot afford to disregard the paramount importance of the physical sciences, they will maintain the position to which other sciences more closely connected with their present curriculum have of late years grown. The former, on account of their rigorous methods, the positive character of their results, and the abundance of their possible applications must always hold the first place, and present a standard for the rest; but these others also, in so far as they are really matters of scientific treatment, are in their proper subordination equally fields for original research and proper objects for endowments. The example of the German Universities has familiarised our own seats of learning with the notion that the study of languages, of antiquities, and of history, are all capable of being pursued in the genuine scientific spirit, and may lead directly to the most important positive results. Abundant evidence has been given within the last few years to show that the primitive condition of mankind and the origin of civilisation are matters which may be revealed by Science. The metaphysical explanations of the last century have given way before the well-ordered facts and regular uniformities which modern inquirers have been able to discover and arrange. The products of the human mind, and the course of human action, when displayed in their simplest and most universal forms, have been proved to be proper subject-matter for Science, no less than the law of man's physical organism or the processes of external nature. The most advanced thinkers have no hesitation in saying that the origin of natural religion is capable of being disclosed by the same methods and with equal certainty as the origin of species, and that philology yields an instrument which can unfold the secrets of an unknown past as surely as the spectroscope reveals the composition of unknown worlds. Just as modern psychology has found it necessary to borrow a large portion of its materials from the kindred science of comparative physiology, so have the nascent sciences alluded to above been under a continual obligation to the methods of physical science, and especially those to which they are linked by means of the recognised science of ethnology.

By thus widely extending the meaning of the word Science, the intention has been to widen the area over which the endowments of original research may be extended, and to give an indication of the number of directions in which scientific investigation should be encouraged. As an indirect consequence it may be suggested that this aspect of the matter shows an easy method by which the don of the last generation, an acute critic merely in longs and shorts, and erudite only in Greek particles, may be changed, without any violent transformation, into the modern scientific student. It is not so much that the subject-matter of his studies will be different, for in this respect the reform must be gradual, and is already being carried out, but that the whole spirit of his pursuit must be altered along with the tenure of his office. The principle throughout advocated is not that money should be abstracted from the Universities for general scientific objects, but that the large funds which they cannot need for the purposes of teaching, should, for the future, be devoted, not as prizes to their successful examinees, but for the support of those engaged in original scientific work: and it is contended that this is the sole means by which they can justify their retention at all of this surplus, and by which also the main objects of the first givers can be carried out. It must also be noticed that this development of our scheme brings out into greater clearness the old position that the obligation of teaching would be merely an incumbrance on our new scientific fellow. Discoveries in Science, especially those of the most important kind, are made in such a tentative fashion, and are proved by such elaborate inferences, as to be quite incapable of being communicated orally to a class; nor indeed would it be desirable that researches, as soon as made, should be forthwith promulgated from the professor's chair. The growth of Science, whether physical or not, must in many cases be militant, and may be left much more profitably to the student, who is ever investigating with a single eye to the truth, than to the teacher, who must be always careful of the form in which his doctrine is to be conveyed.

We trust, therefore, that by this attempt to show that the meaning of scientific research is not so restricted as it has been some times represented; not only has the general thesis of these articles been strengthened, but also that new adherents may be conciliated in favour of a cause which promises to connect together the followers of physical science and those at the Universities who alone worthily maintain the dignity of their ancient studies; and it must always be understood that we have looked upon the Universities as representing local State action, and that the nation must do universally what we think should be done by the University authorities locally at Oxford and Cambridge. C.

EUROPEAN SPIDERS

Remarks on Synonyms of European Spiders. By T. Thorell, Ph.D., Junior Professor of Zoology in the University of Upsala. (Upsala, 1870-73, pp. 1-644.)

FEW branches, perhaps, of entomological science show the effects of independent and isolated labours more strikingly than Arachnology—limiting this term here to the order Araneidea (or Araneæ). The great works of N. Westring on Swedish Spiders, published in 1861, that of Mr. Blackwall on those of Great Britain and Ireland, published 1861-64, and the "Catalogue Synonymique des Araneides d'Europe," by M. Eugène Simon (included in his general work "Histoire Naturelle des Araignées," published 1864), are an instance representing very strongly the fruits of this isolated labour in the same branch of natural history science. These authors appear to have been, and indeed, it is believed, actually were—the two former at all events—totally ignorant of each other's existence. M. Simon, indeed, quotes Mr. Blackwall occasionally in his "Catalogue Synonymique," but his knowledge of that author's works was apparently confined to the scanty and often erroneous quotations in Baron Walckenaërs' "Insectes Aptéres." Mr. Blackwall then and M. Westring, each, in his own way and with the works of other authors more or less at their common command, plodded on for years in parallel paths. Both worked diligently and laboriously, at, for a very great part, as a glance at the map would suggest, identical objects; their labours at length resulting in the respective volumes above mentioned. So much as this however can hardly be said in regard to the third one of the works noted. The "Catalogue Synonymique" bears few marks of labour at the objects themselves which it enumerates, and is in fact a mere desk work, remarkable chiefly for the limited and often infelicitous use of the materials undoubtedly available at that epoch to any author professing to gather together and to harmonise the independent and scattered morsels of an extensive branch of natural science. The good work, however, done since, and being now daily done in Arachnology by M. Simon, will soon obliterate the remembrance of the comparative failure of the more ambitious efforts of his early years.

Towards these isolated works of Mr. Blackwall and Mr. Westring the minds of Arachnologists in more than one quarter appear soon to have been directed, with a view to bring their parallel lines together. Dr. T. Thorell—of the University of Upsala—a countryman and personal friend of Mr. Westring, and an accomplished scholar, was the first to move publicly in it: and bringing great ability and clearness of head to bear upon the subject, designed an almost exhaustive work on "European Spiders." Of this work, and under that title, was published in 1869-70, Part I. with the special title of "Review of the European Genera of Spiders, preceded by some Observations on Zoological Nomenclature." This portion of the proposed work appeared in the "Nova Acta Regiæ Societatis Scientiarum Upsaliensis," ser. iii. vol. vii. Fasc. i. et ii.; but owing to some unforeseen difficulty, and unfortunately for the external continuity of the two portions of the work, the second part, intended to treat upon the more special division of the subject, was published in 1870-73 as a separate work in a different form and with an independent title, being that given at the head of this notice, viz. "Remarks on Synonyms of European Spiders;" this is, however, as may be at once seen from the respective introductions to the two, although the title of the second does not allude to it, really Part II. of the originally designed work, "On European Spiders." It is thus evident that though the present notice is upon the second work, it could not be adequately considered without first remarking briefly upon the one which preceded.

Dr. Thorell's stated object ("European Spiders," p. 1) being to fix the nomenclature of the spiders described in the works of Blackwall and Westring, it was obviously necessary first to decide upon the genera recognised by them, and by those authors also to whom they refer; and for both this and the subsequent determination of the specific name to which each spider was entitled, it was, above all, important to lay down the rules by which the author proposed to be guided in his decision between perplexed and conflicting synonyms. It is no part of the object of this notice to criticise the rules thus laid down in Part I. pp. 3—18 of Dr. Thorell's work; suffice it to say that they are substantially those laid down by a committee of the British Association, Ann. N. H. 1, vol. xi. p. 239 *et seq.* Their general reasonableness is obvious, though in many cases their rigorous enforcement would savour of pedantry and lead to undesirable results. Another feature in Part I. of "European Spiders," that is the list of works upon Arachnology, with the name of the author and date of publication, is a useful and important one, and cannot fail materially to assist the general student as well as anyone desiring to test the justice of the author's decisions. This list is considerably enlarged in the portion of the work now under consideration, pp. 584—589.

Coming then thus to Dr. Thorell's "Remarks on Synonyms of European Spiders," we soon observe that whatever the difficulties may have been which beset the determination of the generic synonyms in Part I., these difficulties must have been immensely increased when the specific nomenclature came to be decided. The task was not merely to ascertain and fix the names of those species common to Blackwall and Westring, but those of all the authors quoted by them as synonymous, with their species, together with the names of many other species into the bargain, such as those which have been subsequently described by E. Ohlert ("Die Araneiden oder Echten Spinnen der Provinz Preussen," 1867), A. Menge "(Preussiche Spinnen," 1866—1872, not yet complete), and other authors. This laborious task Dr. Thorel has executed with exceeding care and minuteness of investigation; his plan has been first to take, by way of text, the species described by Mr. Westring, as being, with only four exceptions out of 308, known to himself from examination of the type specimens; then to determine by strict and careful comparison of figures and descriptions what species described by other authors appeared to be more or less certainly identical with those of Westring; and then which of these had priority in regard to time of publication, the date of publication being in each case placed in immediate conjunction with the names of the spider and author, and the usual reference to the name of the publication or where published. Some idea of the labour of comparison and discrimination of descriptions and figures may be obtained by the fact of the number of synonyms given of a single species, being in some instances as many as twenty-two. Each of these would form the subject of a separate investigation, independently of those, often numerous, synonyms quoted by each author cited, and which would frequently have to be investigated in a similar manner; and after all, when the frequent meagreness of descriptions and badness of figures are considered, it will be evident that the determination of synonyms must, without types of the species described for examination, be often little more than approximate guess-work; in fact it is not too much to say that the greatest care and pains bestowed upon figures and descriptions alone would give but very unsatisfactory results.

In the present instance Dr. Thorell has had the advantage (fully acknowledged in his work) of being able to examine and compare, not only the types (as before observed) of all but four of Westring's species, but also, with them, a very large proportion (nearly 250 out of 304) of those described by Mr. Blackwall, and many more described by other authors quoted in the "Synonyms." With these undeniable advantages the various considerations entering into each question of synonymic identity or distinction are detailed in a manner at once full and yet terse; and wherever a doubt has finally rested it is never slurred over or disguised.

It would be in vain to select special examples in proof of this; the details which follow the list of synonyms appended to each species, are, in almost every instance, of this thorough and honest character.

The first section of the work being occupied with the spiders described by Westring, forms by far the largest portion of the whole—pp. 1—407; for the complete determination of all the synonyms involved in this section of necessity cleared off a very large number of the species described by Mr. Blackwall. The consideration of the remainder of these forms section ii., and occupies pp. 414—470. This part ends with an exceedingly useful "List of the Spiders described and figured in Mr. Blackwall's 'History of the Spiders of Great Britain and Ireland,' together with a statement of the names believed to belong to each of them, the year in which the assumed specific name was published, and the work in which this publication took place; or instead of these last-mentioned particulars, a reference to the place in the present work where the species may have been more fully treated of." In this list, those species, about 67 in number, of which Dr. Thorell has not himself seen types, are marked with an asterisk. Section iii. contains "Synonymic remarks on some of the Spiders included in Simon's Catalogue Synonymique des Aranéides d'Europe." For reasons given in the introduction to this section, Dr. Thorell's remarks are confined to a small number of the species contained in Mr. Simon's Catalogue; in fact this catalogue, being a mere list of names, is used only as indicating some European species of general interest not contained in either Mr. Blackwall's or Westring's works.

In a work of the nature of that now under consideration, and occupying nearly three years in its appearance, it was inevitable that some errors should get in, as well as that modifications and additions should be necessitated in consequence of extended research and more accurate information obtained during that time; these, under the head of "Additions and Corrections," occupy pp. 544—582; and the remainder of the volume, pp. 582—607, is taken up with additions and corrections to that portion of the original design (mentioned at first), entitled, "On European Spiders, Part I." This is a very important, as well as interesting, *finale* to the whole, containing, as it does some further observations on nomenclature, with a disquisition on the present state of the question as to the exact functional use of the palpal organs of the male spider. Some remarks are also made upon the *fourth pair of spinners*, or inframammillary organ, discovered by Mr. Blackwall many years ago, and ascertained to be correlated (in the female spider only) with a peculiar comb-like row of bristles—calamistrum—on the metatarsi of the fourth pair of legs. Dr. Thorell appears somewhat to doubt Mr. Blackwall's position, that this organ is in all cases a true spinning apparatus; the better opinion would appear to be that it is so.

The work ends with some very valuable remarks on the general classification of the Araneidea, or (as Dr. Thorell, with good reason, prefers to call the order of spiders) Araneæ, pp. 597—607. Within this compass some recent works and suggestions on the systematic classification of spiders by Dr. Ludwig Koch, Rev. O. P. Cambridge, Anton Ausserer, and others are reviewed and criticised; the conclusion come to being that the new and highly remarkable forms brought to our knowledge by the researches of later years shows more than ever "that a fully satisfactory classification of the order of spiders is a thing not soon to be expected, and that a by no means inconsiderable number of forms cannot without great uncertainty, even if at all, be included under the hitherto received families and higher groups." Undoubtedly, towards this satisfactory classification, by whomsoever it may be finally effected, Dr. Thorell has done good work in the volume on "European Spiders," and that on their "Synonyms." The systematiser hardly exists yet who could say with truth that he had risen from a perusal of these volumes without considerable alteration, or, at least, modification, of his own previous views on the subject.

With so much to commend, in the work under review, it may perhaps appear invidious to notice what seem, to be a defect, at least in point of form. In the course of the minute and extensive investigation of specimens, descriptions, and figures necessary to arrive at a satisfactory determination of obscure synonyma, species here and there appeared to be new to Science, and others to require separation (under other names, and with a fresh description) from those with which they had before been confounded; these new and separate species Dr. Thorell has described in extended notes, *in loco*, in a smaller type, thus marring the continuity, and breaking in upon the expressed design of the work. Would not these descriptions have come in better, and have been more useful for study and reference, had they formed an appendix to the work?

Another defect (though its rectification might perhaps be said to have been a departure from the strict design of the work) appears to be that Dr. Thorell does not include in his volume *all* the spiders at present known to be indigenous to Europe; it details those described by Westring and Blackwall, with some others given in M. Simon's catalogue, as well as, incidentally, many more described by other authors; but still it leaves unnoticed other described species. It would have given the work a great additional value had there been a general list of all the (at that present time recorded) spiders of Europe in systematic order, or, at least, a supplementary one of all those species mentioned or detailed throughout the work, in addition to those of Blackwall and Westring. This is, however, as before hinted, rather a criticism upon the design than the execution of the work, though it seems to be invited by the author's having so far departed from his own original design as to include descriptions of new species, as well as notices of others besides those included in "Araneæ Suecicæ," "Spiders of Great Britain and Ireland," and the "Catalogue Synonymique."

It would be scarcely proper to conclude this notice of a scientific work written by a native of Sweden, without a remark upon its being written in English, and a well-deserved compliment upon the exceeding clearness and terseness of the style, and its generally happy accuracy of expression.

Dr. Thorell's own opinion—expressed in a note to page 583—and in which most English-writing naturalists will probably acquiesce—is that English will one day become the common scientific language of the world, not only because it "is far more widely diffused over every part of the earth than any other culture-language, and that already two of the greatest nations publish in it the results of their scientific labours, but because English, on account of its simple grammar, and as combining in nearly the same degree Teutonic and Romanic elements, is by most Europeans more easily acquired than any other language." The opinion, however (given in the same note *l. c.*), in regard to works written in little-understood languages, such as Russian, Polish, Bohemian, Finnish, or Magyar, will hardly be endorsed. Dr. Thorell would exclude works written in these or such like languages, from equal scientific weight with others written in French, English, German &c., *i.e.*, he would not apply to the former the rules, as to priority, applied to these latter. Now, however grateful it would be to Western naturalists to have all works on Natural Science published in languages with which they are ordinarily more or less familiar, yet it would be rather too hard upon other nations, to whom the love of natural history has come sooner than a general philological culture, to be excluded from equal scientific rights with their more advanced brethren in the West. It would seem quite as just, if not more so, that if a penalty is to be paid for ignorance of foreign tongues, it should fall rather upon those who, with whatever trouble and inconvenience, certainly might become acquainted with works on Science in any language, than upon those who, preferring to write in that tongue in which they can undoubtedly think most clearly and best express their thoughts, give the results of their scientific labours in the vernacular. By all means let us have, if possible, a common scientific language, but meantime, if it be so, we must put up with the occasional annoyance of finding that a genus or species which we had fondly imagined we were the first to describe, had already, perhaps long, been well described, and possibly figured, in some unheard-of work written in an outlandish tongue not understood of the Western Scientific World.

OUR BOOK SHELF

A History of the Birds of Europe. Parts 18, 19, 20. By H. E. Dresser, F.Z.S., &c. (Published by the Author at 11, Hanover Square.)

THIS fine work continues to appear with commendable regularity every month, and keeps up its high character both for fulness of information and beauty of illustration. In the numbers now noticed are several highly artistic plates, such as those which represent the White-shouldered and Imperial Eagles, the Great Black-headed Gull, the Common Crane, the White Stork, and the Great Bustard, which each form a perfect picture. We find full but not too lengthy articles on all these, as well as on the Black Grouse, the Curlew, and many smaller birds. An excellent plan is adopted, in the more characteristic and difficult European genera, of giving a list of all the known species, with notes of their distinguishing characters and geographical distribution. One of the most rare and interesting species figured (in Part 20) is the Teydean Chaffinch, a bird of a blue colour, and which is confined to the upper limits of the pine forests of the Peak of Teneriffe, and to the desolate plains above them, feeding on the seeds of the Retanca (a broom-like plant) and the *Adenocarpus frankenoides*, which characterise those regions, as well as on the seeds of *Pinus canariensis*.

A. R. W.

Lehrbuch der Physik, von Dr. Paul Reis (Dritte Lieferung). Leipzig: 1873.

THIS forms the concluding part of Dr. Reis's useful handbook of Physics. The subject of physiological optics is continued, followed by a description of optical instruments and the laws of the interference and polarisation of light. Heat is treated in the next part, but hardly so fully nor so well as light; radiant heat, for example, occupying less prominence than it deserves. Considerable space is devoted to the explanation of machines for the conversion of heat into motive power: thus we have some of the various forms of steam-engine described, together with a full account of Ericson's heat-engine and Lenoir's gas-engine. Magnetism follows heat, and then we come to static and dynamic electricity and the practical application of electricity. The book closes with a few chapters devoted to the physics of the heavens, or in other words a brief sketch of popular astronomy and meteorology. The principal defect of this handbook is the want of sufficient woodcuts to illustrate the apparatus referred to. The whole work exhibits the characteristic solidity and thoroughness of the German race, and is a marked contrast to some of the recent French popular text-books on Science, the profuse and beautiful illustrations in which almost supplant the letterpress. Let us flatter ourselves that in our nation these complementary races intermingle.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Tyndall and Forbes

IT will probably be considered necessary that Dr. Tyndall's pamphlet,* which first appeared as an article in the *Contemporary Review*, be answered at full length. That, however, cannot be decided for some time, as several of those concerned are abroad; but it may be well to let Dr. Tyndall know at once that there is no difficulty whatever in answering him, and that the answer will not lose force or point by a little delay. In the meantime I hope you will give me space to briefly notice a few of the more obvious inconsistencies of Dr. Tyndall's article.

1. Dr. Tyndall is astonished that the "blameless advent" of his "innocent" "modest" "unpretending" volume should be looked upon as reiterating charges made against Forbes. An extract or two will settle this point.

a. "Had he (Rendu) added to his other endowments the practical skill of a land-surveyor, he would now be regarded as the prince of glacialists."

"Professor Forbes, when he began his investigations, was acquainted with the labours of Rendu. In his earliest works upon the Alps he refers to those labours in terms of flattering recognition. But though as a matter of fact Rendu's ideas were there to prompt him, it would be too much to say that he needed their inspiration."

Put these two passages into straightforward English, instead of Dr. Tyndall's favourite style of insinuation, and they amount to this: that Forbes, having the accomplishments of a land-surveyor, and being acquainted with Rendu's work, put this and that together and appropriated the discovery.

b. Forbes had, in 1860, minutely informed Dr. Tyndall of the nature and amount of his knowledge of Rendu in 1842. It is not too much to say that Dr. Tyndall's sentence quoted above is utterly inconsistent with the plain statement of Forbes, and so implies a serious personal charge against the latter.

c. A similar serious charge is made, when Dr. Tyndall, knowing that Forbes asserted that it was at his suggestion that Agassiz employed a theodolite or a fixed telescope, and that this had never been denied, carefully states that "the same instrument was employed the same year by the late Principal Forbes upon the Mer de Glace," and that "we are now on the point of seeing such instruments introduced almost simultaneously by M. Agassiz on the glacier of the Unteraar, and by Prof. Forbes on the Mer de Glace."

2. Dr. Tyndall tells us that his work was originally commenced as a boy's book, but that "the incidents of the past year" (*i.e.* his controversy with Forbes) caused him to deviate from this intention. Have boys so altered since 1859 that such controversy has now become suitable for them when supplied in the "International Series"?

3. What I said with reference to the unpublished correspondence of Forbes was said without any special reference to Dr. Tyndall. It was simply my excuse to the reader for the very meagre use I had made of so extensive and valuable a collection.

But, even in this matter Dr. Tyndall's inconsistency is patent. He says that, longing for peace, he abstained from answering Forbes, not from inability to do so, but to avoid making Science the arena of personal controversy. Yet, in the same breath, he not only complains of my not publishing certain letters which he supposes to contain charges against himself, but (see §5 below) insinuates that I am acting from feelings of animosity!!

4. Dr. Tyndall's answer to one of Forbes' charges is certainly to some extent plausible. I can say no more till I have an opportunity of consulting Rendu, for it is quite obvious that it is possible by proper selection of portions of so vaguely-written a book to make him appear to say anything one chooses.

Dr. Tyndall's answer to the other charge is so obviously insufficient that I need not deal with it here.

But more than this:—no ever-so-complete defence of himself on one or two points is any reply to the overwhelming pamphlet of Forbes, every line of which in its calm truthfulness calls for an answer.

5. Dr. Tyndall refers to former controversy between us, and to its happy termination at a personal interview. Why Dr. Tyndall should bring before the public such matters as a private reconciliation, unless with the object of holding me up to scorn as the breaker of a solemn truce, I altogether fail to see. I need scarcely say that no one in his senses would enter into an agreement never in future to differ from another, nor to point out in his writings passages calculated to mislead. But the following, and other passages which I need not cite, are all so many half-mysterious insinuations (of the Tyndall kind) against me, and all tend towards the same implied accusations.

". . . the fire was not extinct: the anger of former combats, which I thought spent, was still potential, and my little book was but the finger which pulled the trigger of an already loaded gun."

I shall be obliged by Dr. Tyndall's pointing out to me a single expression, in that part of Forbes' Life which was written by me, which is calculated to give him the slightest offence:—with the one exception of a letter from Forbes, which was specially written for publication; and which, for Forbes' own sake, I would rather not have published.

No doubt he may be annoyed by my saying that little has since been added to the observations made by Forbes on glaciers. This is a matter of opinion. I do not think that Dr. Tyndall has made any addition of consequence to our knowledge of glaciers, and I am supported in this belief by many of the very highest authorities. But this is no charge against Dr. Tyndall.

6. When I saw the "Forms of Water, &c.," I added a brief and excessively temperate statement to what I had already written, and I republished Forbes' own defence of himself against Tyndall and Agassiz. Was I not bound to do something, and could I possibly have done less?

7. The rupture of the truce, or "peace," whatever that may be, was the work of Dr. Tyndall himself—partly by his "Forms of Water, &c." mainly by his article in the *Contemporary Review*. So far as I am personally concerned, the public has no right to know my feelings:—but, whatever they are, they are mingled with the satisfaction I experience in being once more free, as of old, to point out to the public the misleading passages and actual

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* Principal Forbes and his Biographers

errors in Dr. Tyndall's popular works; and to join the too thin ranks of those who, like Mr. Sedley Taylor, are not to be imposed upon by a popular reputation—but venture to think for themselves and to give the public the benefit of the result.

8. Opportunities for such public warning have never been wanting, but now they are so numerous that a long essay would be requisite to do justice to them all.

In the meantime, as an example or two, I may call attention to the way in which Sir Charles Wheatstone, and (by implication) Sir William Thomson, and others, some of whose splendid scientific labours have had the misfortune to become profitable in a pecuniary sense, are treated in Dr. Tyndall's "Lectures on Light," just published. The contrast between the utter contempt for money shown by their censor, and the (implied) opposite which is condemned as unworthy of scientific men, is brought out with all the flow of word-painting and righteous indignation which Dr. Tyndall so abundantly possesses. Besides, the monstrous doctrine is inculcated that men who devote themselves to practical applications are men incapable of original research.

9. But, to conclude for the present, I would simply call attention to the following passage, which comes from an author who in the same work treats of the relative merits of such giants as Young and Fresnel. What confidence can one have in the accuracy of any statement on a scientific matter made by the author of it?:—

"And here we may devote a moment to a question which has often been the subject of public discussion—whether, namely, a rainbow which spans a tranquil sheet of water is ever seen reflected in the water? Supposing you cut an arch out of pasteboard, of the apparent width of the rainbow, and paint upon it the colours of the bow; such a painted arch, spanning still water, would, if not too distant, undoubtedly be seen reflected in the water. The coloured rays from such an arch would be emitted in all directions, those striking the water at the proper angle, and reflected to the eye, giving the image of the arch. But the rays effective in the rainbow are emitted only in the direction fixed by the angle of 41°. Those rays, therefore, which are scattered from the drops upon the water, do not carry along with them the necessary condition of parallelism; and, hence, though the cloud on which the bow is painted may be reflected from the water, we can have no reflection of the bow itself."—"Lectures on Light," p. 25.

If Dr. Tyndall, with the assistance of his scientific advisers, fails to see the justice of my remark on this passage, perhaps you will permit me to make it the text of a little essay in a future number.

I have all along said, and still say, that I cordially recognise the services of Dr. Tyndall in popularising certain parts of Science. But his readers must be cautioned against accepting as correct great parts of what he has written. It is granted to very few men to do this useful work without thereby losing their claim to scientific authority. Dr. Tyndall has, in fact, martyred his scientific authority by deservedly winning distinction in the popular field. One learns too late that he cannot "make the best of both worlds"

I would request Dr. Tyndall for his own sake, not for mine, should he favour me with a reply, not to pick out one or two isolated passages of a letter, which absence from books may possibly have rendered slightly inaccurate—but to answer me, as he has *not* answered Forbes, in the full spirit and not in the partial letter.

P. G. TAIT

St. Andrews, Aug. 20

W. S. J. on Hegel

I RESPECTFULLY request admission, into an early number of NATURE, for an explanatory word or two, in reference to W. S. J.'s review of my poor book on Law, &c., in the valuable publication named, for July 24, 1873.

W. S. J.'s very first sentence speaks of the said little book as containing "a discussion of Hegel's opinions concerning gravitation and the differential calculus." In the first place, Hegel has nothing to say against either gravitation as a fact, or the differential calculus as an established method of indubitable scientific calculation: he would only attempt to philosophise both by placing metaphysical principles under them. Now this is part of Newton's own action, and he certainly would not object to any *attempt*, Hegel's or other, in the same direction. In the second place, I discuss no opinion of Hegel in this reference: I only attempt to expose erroneous opinions of Hegel's relative opinions. To this I strictly confine myself, and this goes much deeper than the reader may, at first, think.

On Law, whatever is said by W. S. J., concerns only the old difficulty of Hegel's *dialectic*; and perhaps the italicising of this word, together with my own intellectual deficiences, may be respectfully offered in explanation of as much. W. S. J. here, then, is evidently misintelligent himself, and, accordingly, only speaks so as to induce misintelligence in others. Nevertheless, it is worth saying that the reader may or may not gain from the particularity of satire in W. S. J.'s hands—so keen is it that it cows, and, again, so kindly that it disconcerts.

Mathematically, according to W. S. J., "the critical statement of the necessary outcome of Hegel's philosophy," reduces itself to this, that the principle in question is placed "in that in which the quantum has disappeared, and there remains the relation only as qualitative relation of quantity." W. S. J. has for this only the mildly-authoritative contempt of a duly-elevated position; and when it is said "What is called infinitely *little* is only qualitative, and is neither little nor great, nor quantitative at all," he at once squelches all by an "*on the contrary!*" Now all this contemned matter comes directly, not from Hegel, but from Newton; for the former, quoting from the latter, says:—

"These (N.'s increments and decrements) are not to be taken as particles of a definite magnitude (*particulæ finitæ*). Such were not themselves moments, but magnitudes, generated out of moments; what is to be understood, rather, is the principles or beginnings (elements) of finite magnitude:" that is, plainly, what is concerned lies "in that in which the quantum has disappeared as quantum, and there remains the relation only as qualitative relation of quantity."

What concerns comets is naively amusing. We have not had to wait in their regard (as W. S. J. seems to think) for the information of "Chambers' Handbook." The astronomers of the last century, as it appears, were able to speak better than even the "Handbook." Comets that return, they say, though after a great many years, travel in ellipses of enormous axes; whereas those that do not return may describe parabolas or hyperbolas. Such is the opinion of Science yet, though it may talk of many other explanations of non-return, as dissipation, interception, &c. This, I say, is how Science looks yet; but W. S. J., for his part, is under the belief that Science has actually within its ken comets that (so to speak) *revolve* in hyperbolas, as well as others that revolve in ellipses. (Positively such seems his idea. Now, Hegel is never once at fault here—in his own way, I mean; for whether in ellipse, in parabola, or hyperbola, Hegel's assignation of the moment of singularity to the comet is, on his own principles, justifiable. May not a non-returning comet, too, be attributed to that contingency which is, and must be, inherent in externality as externality? On the whole, it may be well for us all to let comets alone yet. Our greatest living authority can only philosophise them into stone-rattles which the sun (for his amusement?) whirls about his head.

One has only to consider these things and others the like—the exquisite little gibes, not forgotten, about a secret in two volumes and a secret in fifteen pages, &c.—to perceive that what we have here is only once again the blind rush of prejudice from its usual dark corner of relative ignorance—an ignorance which it will persist in, and not (through study) convert into the light of day. There is that approbative allusion to Mr. Smith, too; W. S. J. will yet be ashamed of that.

On the whole, however, I hope I have not spoken disrespectfully. for I cannot fancy who W. S. J. may be. He talks of law and logic, and is possibly a lawyer; he certainly has a profound contempt for "Hegel and his satellite Stirling;" but were he (what his initials may indicate) "the eminent Jaggers" himself, I cannot, whatever his power of *practice*, admire his capacity for *principles*.

Edinburgh, July 28

J. HUTCHISON STIRLING

Lakes with two Outfalls

IN NATURE, Aug. 14, a paper under this heading concludes thus:—"Colonel George Greenwood, who is, I presume, the same as the former active correspondent about this subject, visited this lake (Lesjeskaugen) last summer, as appears from the entry of his name in the day books. I am not aware that he has since published any opinion, but the lake seems, so far as I can judge, to support his view of the matter.—W. Stanley Jevons." I sent an account of my visit to Lesjeskaugen Lake to the *Geological Magazine* in July 1872, but it was not so fortunate as to meet with acceptance from the editor. The following extracts coincide singularly with the opinions of Prof. Stanley Jevons:—

The river Rauma, at the western end, which gives its name to Romsdal, is the *natural* outlet. The outlet to the river Logen, at the eastern end, is entirely *artificial*. The water-parting there, between Romsdal and the Dovre Feld, is an ancient ridge of drift. A cut has been made by man through this ridge. The stream through this cut now works a saw mill, but was formerly connected with the old iron works. The one outlet from the lake enters the mill pool, from which there are two outlets, one to serve the mill the other for the waste water. All these three outlets are kept each at its required level *artificially*, that is, with piles, logs, boulders, and rubble, so that the quantity of water which is let out of the lake is regulated by 'the miller and his men.' The case is precisely equivalent to the Black Loch, in Dumfriesshire, whose *natural* (!) outlet is an iron sluice in a stone dam opening to a mill lead cut through the water-parting to Lord Bute's mill. (See *Athenæum*, Aug. 6, 1864, and 25 in. Ordnance maps.) If such lakes as these are lakes with two outlets, then the new conduit for the water supply of Glasgow makes Loch Katrine a lake with two outlets. An old dry channel is in direct continuation of the present mill lead. It passes so close to the old iron work as actually to touch its base. If, as I imagine (as does also Prof. Stanley Jevons), the two are connected with each other in origin, the artificial outlet to the lake may be of very great antiquity.

A notice in NATURE, of a new work by Capt. Burton (1872), quotes these words of his: "The northern and north-western portions of the so-called 'Victoria Nyanza' must be divided into three independent broads or lakes . . . *in order to account for the three effluents*, within a little more than sixty miles." Here, then, the great traveller adopts my dictum, that "a lake can only have one outlet." I first published this dictum in the *Athenæum*, July 4, 1863, when the late lamented Capt. Speke, in his "Sketch Map," gave four outlets to Lake Victoria Nyanza, three on "native information;" and in the *Athenæum*, July 25, I said, "I think that the native information will prove to be erroneous." GEORGE GREENWOOD

Brookwood Park, Alresford, Aug. 15

AS Prof. Jevons has revived the question of the existence of lakes possessing more than one outlet, I would invite the attention of your readers to what appears to me an unequivocal instance of the kind, though on a small scale, in the neighbourhood of the place whence I write.

On the high and very broken ground between the old mountain road from Dolgelly to Towyn (which runs at the foot of Cader Idris) and the south shore of the estuary of the Mawddach is a watershed, which separates streams flowing directly into the estuary by Capel Arthog from others which, after joining the stream that descends from Llyn y Gader in the hollow immediately under the summit of Cader Idris, find their way into the estuary some three miles higher up. On this watershed lies a lake about half-a-mile long, named Llyn Creigenen, which occupies a rock basin with two lips at exactly the same level, one at its western, the other at its eastern extremity. By the western lip a small stream issues which descends rapidly and at one part of its course forms one of the branches of the Falls of Arthog, well known to visitors at Barmouth. By the eastern lip also, a stream, diminutive, it is true (at any rate in the summer months), but still quite distinct, issues and descends into a boggy tract, along which it wanders for some two miles, until it joins the stream before mentioned from Llyn y Gader. These facts are distinctly recorded on the Ordnance map, and I have frequently verified them myself and pointed them out to others. I think there can be no doubt but that in this instance both of the outlets are *natural*, and that a stream must issue from one if a stream issues from the other, at any rate at the ordinary level of the water in the lake. It is perhaps, impossible to say that both outlets are *permanent* in that *secular* sense which Prof. Jevons seems to attribute to the word, as circumstances are easily conceivable under which the flow through the smaller easterly outlet might cease; but at any rate for many years, supposing the average supply of water to the lake to remain the same, and no artificial barrier to be erected, the two streams will continue to issue from the lake at all seasons.

Prof. Jevons remarks that "on *à priori* grounds it seems very unlikely that there should exist any lake with two distinct outflows." I would reply that, while it is undoubtedly improbable that any particular lake named at random should possess this characteristic, it can hardly be regarded as *à priori* very unlikely that among all the lakes on the earth's surface there should be found here and there one with more than a single outlet. At any rate, I would recommend anyone who is sceptical in the matter to visit Llyn Creigenen, which is but an easy hour's walk from the Arthog Station on the railway between Barmouth Junction and Dolgelly. ROBERT B. HAYWARD

Capel Arthog, Aug. 16

Cranes in the Gardens of the Zoological Society of London

IN NATURE of June 26 (*antea*, p. 164), Mr. W. A. Forbes points out an error in the report of the meeting of the Zoological Society for June 15, in a statement that no example of *Grus vipio* (*sive leucauchen*) had been brought to Europe previously to those lately received by that Society. Instead of "Europe" the word "England" should have stood in the paragraph in question, which would then have been correct.

It is quite true (as stated by Mr. Forbes) that the collection of living cranes in the Gardens of the Zoological Society of Amsterdam is the finest in the world. At the same time the series of these birds in the Regent's Park is also at the present moment very nearly perfect, embracing, as it does, examples of all the usually recognised species, with the exception of *Grus leucogeranus*, and *G. monacha*.

Of the former of these the Society once possessed a living specimen, but the rare *G. monacha* of Japan has, I believe, never yet reached Europe alive.

The following is a list of the Zoological Society's present series of the Gruidæ:—2 Common Cranes (*Grus cinerea*), 1 Brown Crane (*G. canadensis*), 2 White-necked Cranes (*G. leucauchen*), 1 Sarus Crane (*G. torquata*), 1 Australian Crane (*G. australasiana*), 1 White American Crane (*G. americana*), 1 Mantchurian Crane (*G. montignesia*), 2 Wattled Cranes (*G. carunculata*), 1 Balearic Crowned Crane (*Balearica pavonina*), 4 Cape crowned Cranes (*B. regulorum*), 3 Demoiselle Cranes (*Anthropoides virgo*).

August 27 P.L.S.

Colour of Lightning

I SHOULD be much obliged to any of your readers who would give me any information as to the cause of the colour of lightning.

In one of two storms which passed over here yesterday evening the lightning was decidedly pink in tint; later in the night it had regained its normal yellow or bluish colour.

Odrey, Aug. 25 H. GEORGE FORDHAM

Harmonic Causation and Harmonic Echoes

IN reference to the question of "Harmonic Echoes," allow me to suggest to those who may have the opportunity of observation, how desirable it is that these echo-tones should be investigated in a manner to determine whether they are truly harmonic or not. There would be no difficulty in testing the sounds given in response to the notes of a closed organ-pipe and an open one, or the notes of representative musical instruments, clarionet and flute. It might be found that the echo at Bedgebury Park would give the octave always, irrespective of the particular instrument provoking it; or, on the other hand, that it refused to answer to a closed pipe, or gave only the twelfth, its proper reply. We should then know whether the echo-tone was that of the harmonic or a new fragmental tone consequent on the breaking up of the wave of the fundamental or ground-tone, by "breakers ahead."

Now that we are called upon to recognise several varieties or classes of musical tone, it is time that the leaders in Science came to a general agreement upon the use of the term "harmonic." Is it to be applied indifferently to "over-tones," otherwise "partial-tones," to "combination-tones," to "concussion-tones," arising from the violence of the shock of sound-waves in collision, to "fragmental-tones" produced out of the wave of the ground-tone broken up by obstacles encountered in its course or in reflection, and to "echo-tones" which may be affiliated to either variety? It seems to me that we risk much confusion unless "harmonic" is restricted to its earlier usage, and applied solely to the "harmonic series,"—the tones which are the direct offshoots of the fundamental. These tones have but one order of succession, and will bear no interpolation: the octave, twice the velocity of the fundamental; the twelfth, which is three times; the fifteenth, or double octave, four times; the seventeenth, five times, and so on, always an acceleration by uniform addition. In the examples taken from the compilation of Dr. Brewer, the echo-tones go beyond all law of harmonic progression, and must be accounted for as belonging to other classes of tones, if the data can be relied upon, seeing that some instances are questionable as to authority, and others are beyond proof. The Bedgebury Park instance may be taken as proved; it is simple, and attested by living authority. Who will vouch for the other instances as evidence? The question is not put to cavil, but because of the dubiousness of the possession by the several recorders of the necessary qualification for an accurate estimate of the phenomena recorded.

Musical people of any pretension to critical power in these matters are generally "self-centered," each individual considers himself competent to pronounce judgment on "pitch," yet the delusiveness of this belief would be testified to by none more readily than by men who are daily engaged in tuning and in estimating minute relations of the invisible geometry denominated "pitch." Notwithstanding long experience and daily practice, no sooner does any question arise out of ordinary routine than they hesitate to depend on judgment alone, but resort to comparison with some fixed pitch already ascertained, and by this means prove themselves to be frequently at fault when least expecting it. Harmonic sounds are difficult to judge of, they lie at wide intervals, are frequently sharp, and if pure and faint, the ear is as liable to be deceived by an apparent lowness as it is with pure ground-tones. Fineness of ear for perception of niceties of pitch is by no means a common endowment, and where it exists, does not certify a fine musical organisation. Pitch bears relation to musical tune and to quality of tone similar to that which geometry bears to figure drawing and to painting. In rare instances only are the faculties for each associated in fair proportion, and frequently the possession of one power seems to exclude or override the others. Some men are gifted in this respect, and will tell you the pitchnote of a button, or a pencil, or a pin, as accurately as they will the notes of a song; or will discriminate, without hesitation, every note in a series of complicated chords with a skill almost as sure as instinct.

Professor Tyndall introduced the term "over-tones" in connection with "harmonic;" more recently, in Helmholtz' Lectures, Mr. Ellis has substituted "partial tones;" and Mr. Sedley Taylor adopts the same. This is a pity, for there is something incongruous in the idea of "partial tones" which yet are complete whilst component, and Tyndall's term "over-tones" is far more expressive.

The question of harmonic force, in which probably lies the explanation of the Bedgebury echo, came before me a few days since in experiments made to obviate, if possible, the wavy unsteadiness common to stopped pipes with high-cut mouths. Many variations were made without useful results. On withdrawing to some few yards' distance from the pipe into a recessed doorway, it was observable that the fundamental tone completely vanished, and the first harmonic, the twelfth, came into prominence instead of it, although you had only to step a yard forward to become again aware of the continued co-existence of the fundamental. On comparing this segregated twelfth with a corresponding note in the scale of the standard pitch of the organ, it was found to be decidedly too sharp, and thus the real cause of the waviness of tone was discovered, thereby saving many experiments in a false direction.

Several works now give elaborate analyses of harmonic tones; Mr. Sedley Taylor's "Sound and Music" is the last most useful addition, and supplies much previously wanting. In no work, however, do we meet with any definite statement as to the causation of harmonic tones; yet it seems necessary for the full understanding of their nature and of the relation they bear to the instruments producing them that the mode of their origination should not be left unheeded. The conclusion derived from my own investigations is that the harmonics of musical instruments have their origin solely in the *surplus energy* of the generating force over and above that necessary to produce the fundamental tone; this superabundant vigour finds its outlet in accessory vibrations, and the harmonics are the escape valves for securing to the fundamental tone freedom from fluctuations to which otherwise it would succumb. When the vibrating force is inadequate to waken the ground-tone of an organ pipe it settles down into the harmonic nearest related to its power; the tone may be considered as surplus energy, since it is disproportionate to its work, and only becomes harmonic because it falls short of the fundamental after which it is striving. Except in this relation we should regard it as ground-tone. When a pipe is overblown, the harmonics maintain themselves through the excess of energy to the complete exclusion of the fundamental, and they are sharp to the regulated pitch of the pipe. Harmonic tones when thus produced independently have considerably more intensity than the normal tones of pipes of corresponding pitch. In all the orchestral wind instruments it is the higher notes that require greatest wind-force for their production; the clarionet alone differs as respects a certain range of its high notes, where the reverse is the case, the force being considerably less than for the lower range, but the structural conditions of the instrument sufficiently account for the peculiarity.

The experiment with the stopped pipe previously described clearly shows the penetrating power of accessory tones, and that whilst the fundamental occupies the ear by its volume, the harmonic has the strongest vitality even in its associated condition. In view of these facts we may reasonably infer that the "octave echo" in Bedgebury Park is the reflected harmonic heard alone; still it would be well to prove it in the manner suggested. That the voice was returned from a plantation "across a valley," gives intimation of a distance favourable to the loss of the fundamental tone in the depths of the valley; and that "the original sound required to be loud and rather high" is an additional assurance of the presence of harmonic vigour in the vocal tone.

A remarkable instance of echo freaks within my own experience is well timed to be spoken of here. At the bottom of my garden there is a meadow, then a double row of houses with a high railway embankment at the end, and a wall rising beyond that. About two months ago, whilst looking over the meadow at the clouds of sunset, the sound of a band in the distance came upon me, and, immediately following, the sound of another and more demonstrative band from an opposite direction, giving prediction of horrible discord. Strange to say, although the two bands were playing most noticeably different melodic phrases, there was no conflict; one band seemed to be the symphonic accompaniment of the other, and there was a peculiar charm in the effect, causing regret that the music should come to a natural end. Knowing that the *first* band was echo-music, there was at once a singularity to attract attention, how to account for the precedence of that which should be secondary? but the greater puzzle was to understand how it came to pass that the music was *different*, so that whilst listening the illusion of a distinct band was difficult to dispel, doubts arose about Echo having any voice in it at all, only that from time to time the pauses between the phrases showed the *following* of the form upon the shadow. Reflection upon the matter afterwards furnished the probable explanation. The distance of the place of echo was approximately between six and seven hundred feet from the source of the sounds, my standing place being at about one-fourth of the distance; between me and the band three houses intervened, over which the music came to me, whilst the terrace on which the band was stationed opened freely on to the meadow; thus Echo received the music earliest by reason of the unobstructed passage, and her rendering was that of natural selection, the most vigorous tones, and the penetrating harmonics, whatever had most living power, infused by the players and sustained by the characteristics of the instruments, all these reached her and rose again in perfect accord with the original harmony, whilst all the other notes, those of low vibrating power and of inferior stamina, were lost by the way. It should also be noted that observation afterwards of the angle of incidence and positions of the band and of the listener showed that the course of the sound waves on their way to Echo was in *front* of a detached line of cottage buildings, then passing into the enclosed space between the double row of houses up to the embankment, the recourse being by the *back* of the cottage buildings, across gardens and the meadow to the listener. Doubtless the singular vividness of the phenomena was due in great measure to the state of the atmosphere, which at the time was peculiar, the western sky heavy with gorgeous clouds, and the air silent and sultry. The relation which the organ-pipe experiment first detailed has to the theoretical solution here offered will be readily perceived; and but for the support afforded by it one could hardly have ventured on the statement and the explanation, which else would have appeared to be, the one unreal, and the other fanciful.

August 25 HERMANN SMITH

The Oreodon Remains in the Woodwardian Museum

My attention has just been accidentally called to some notes on "Oreodon Remains in the Woodwardian Museum, Cambridge" in your number of August 14.

I hasten to correct an error into which your correspondent has fallen as to the locality in which the remains to which he refers were obtained. I did not visit the Mauvaises Terres of Nebraska, but collected all my specimens in the valley of the John Day River, in Upper Oregon, about long. 120° 10′ W., lat. 44° 40′ N.

Most of the specimens are from near the head of a small stream called Bridge Creek, a locality well known to Prof. Marsh, whose new species of Oreodon described in the *American Journal of Science and Art* was possibly obtained there. A few, however, are from the Great Cañon higher up on John Day's River, nearly opposite Old Camp Watson, where I passed the winter of 1871-72.

I was informed by a gentleman who accompanied Prof. Marsh's Yale College Expedition, in October 1871, that they had on that occasion found a skull of a new and unusually large species of Oreodon in one of the places above mentioned. But your correspondent is probably acquainted with all the descriptions that have been published in America, and will know whether the *Oreodon superbus* of my informant has or has not yet been christened in print.

I have regretted much since my return that I only devoted parts of three days to a search for these interesting remains.

WALSINGHAM

Merton Hall, Thetford, Sept. 5

Bright Shooting Stars

I BEG to send you the following particulars of the observed paths of nine bright shooting stars recently seen here.

Ref. No.	Date.	Time	Apparent Mag.	Began R A.	Began Dec. N.	Ended R A.	Ended Dec. N.	Length of path.	Approx. Radiant point.
		h. m.							
1	July 28	11 32	= ♃	210°	49°	200°	38°	14°	Pegasus
2	,, 28	11.48	1st mag. ✱	202	44	193½	36	10	Pegasus
3	,, 30	10 45	1st mag ✱	42	43½	45	36	8½	χ Persei
4	Aug. 2	11.40	= ♃	43	54	62	56	12	Pegasus
5	,, 7	9 33	2 × ♀	190	59	195	90	30	Polaris
6	,, 9	10 12	= ♀	41	75	196	73	30	χ Persei
7	,, 9	10.29	1st mag ✱	37	45	50	42	10	Andromeda
8	,, 9	11.05	1st mag ✱	337	59	304	50	20	χ Persei
9	,, 9	11 29	1st mag ✱	28	41	12	46	12	Andromeda

No. 5 in the above list was the brightest, and left a very perceptible train just N. of Cor. Caroli for 7ˢ. No. 9 also left a train, visible for 3ˢ, N. of γ Andromedæ.

The evening of August 9 was clear, and two observers counted thirty-five meteors in the interval between 10h. 15m. and 11h. 45m., after which time clouds obscured the sky. During the night of August 10 it remained overcast. Of the thirty-five shooting stars seen on August 9, the great majority were Perseids, but the radiant region is diffusely extended from the star group at χ Persei to B Camelopardali. There were also indications of radiation from Pegasus and Andromeda. The August meteors of this year appear to have been larger than those seen in former years; at any rate bright meteors have been exceptionally abundant during the dates included in the above list.

WILLIAM F. DENNING

Bristol, August 11

November Meteor Shower of 1872

MR. E. D. JONES, of San Paulo, Brazil, has sent me the enclosed extract from his diary, referring to the meteor shower of November last, which he observed whilst crossing the Atlantic.

HENRY C. BEASLEY

Gateacre, Liverpool, Sept. 3

"Nov. 27, 1872, s.s. *Halley*, N. lat. 11° 30′, W. long. 26° 50′. —There was a splendid shower of meteors this evening. I saw them shooting in profusion as soon as it was dark (about half-past six). I sat in a chair on deck facing the west, where Jupiter was flaming in the tropic sky, and watched the flying messengers from other worlds. I counted no less than 400 in half an hour, that is at the rate of about 14 per minute. They came in shoals, as it were. There would be a long pause, and then five or six would fly across together, reminding me forcibly of the flying-fish we had seen in the daytime. Every now and then a much brighter one than usual would flash into existence, and leave a trail of beautiful reddish light behind. Generally speaking, they were as bright as a star of the second magnitude. But the brighter ones I speak of were quite equal to stars of the first magnitude. One splendid one at about eight o'clock (local time) was so bright that it lit up the sails of the ship; it was of a red colour, and burst in two before disappearing. One later on left a trail which I could distinguish for half a minute. I was able to trace the point in the heavens from which the meteors emanated, viz., a point near the northern extremity of Perseus, between that constellation and Andromeda. About this point I often saw them come into view, and die away with scarcely any apparent motion, on account of their coming in a straight line towards the observer; below this point they fell towards the horizon, above it they fell across the zenith, and so on. Those with the longest path were in the western sky (opposite Perseus), as the view was the least fore-shortened there. The position of the *Halley* was that given at the heading of this extract. The following table shows that we probably did not see the thick of the shower, having passed it by daylight:—

G.M.T.	Time in which 100 were seen.	Number per minute.
8.30 P.M.	8 minutes	12·5
8.38 ,,	7 ,,	14·3
8.45 ,,	7 ,,	14·3
10.5 ,,	17 ,,	5·9
10.22 ,,	17 ,,	5·9
10.49 ,,	22 ,,	4·6
12.15 A.M.	36 ,,	2·8

"The reasons that the first observation gives fewer than the second, may be that the twilight did not allow of the less brilliant meteors being seen; that the eye of the observer was not so well practised in detecting them; and the light clouds flying through the air may have obscured some of them. The other observations show a regular decrease in the numbers from 8·45 P.M.

"I counted 750 meteors in my observations, and saw quantities more besides. Of course I could only see about one-third of the sky at a time, but I was looking in the direction of the thickest fall most of the time, so that I daresay I saw half the number that actually fell; taking this for granted, there must have been 3,500 between 8.30 P.M. and 12.15 A.M., Greenwich mean time."

EXPLORATIONS IN THE GREAT WEST

WE are now in possession of facts which will supplement our last reference to this subject. The following names may be added to the list of scientific men accompanying the Wheeler Expedition engaged on surveys west of the 100th meridian:—Mr. Severance, ethnologist; Drs. H. C. Garrow and J. L. Rothock, naturalists; Mr. H. Stewart Brown, meteorologist; Messrs. Klett and Louis Mell, topographers. The entire force numbers 175 men.

The surveying party of Mr. Clarence King, geologist, designated as the Geological Survey of the 40th parallel, has just finished its work and is recently disbanded. Among the scientific men accompanying it were Messrs. J. G. Gardner (astronomer and geographer), Wilson (topographer), J. D. Hague (mining geologist), Emmons (assistant geologist), Arnold Hague (chemist and mineralogist), Robert Ridgway (zoologist), and S. Watson (botanist). The force is largely absorbed by other expeditions now in the field. The results of this expedition are expected to fill five quarto volumes and accompanying atlases; of which one on mining in Nevada and adjacent territories with folio atlas will be by Mr. Hague, and one on botany is already published. The remaining volumes are well under way and will, it is expected, be completed during the present year.

There is an expedition known as the International

Northern Boundary Commission, engaged in the survey of the 49th parallel from the Lake of the Woods to the crest of the Rocky Mountains. Archibald Campbell, of Washington, is the commissioner in charge; Major Twining is the chief of engineers on the part of the United States, and Dr. Elliott Coues of the U.S. army is the naturalist of the expedition. The British Government details its proportion of the party, which is thoroughly equipped for this service. The operations of the present year extend westward from Pembina.

The expedition under Major J. Powell, to the cañons of Colorado, is still in the field. Major Powell has spent several years in explorations in this region, and has constructed a map of great interest and accuracy. His ethnological researches among the Piute and other Indian tribes have resulted in a large and exceedingly valuable collection.

Mr. Wm. H. Dall, well known by his elaborate work on the Territory of Alaska, founded on his former three years' residence in that region, is now actively employed in continuing his survey and hydrography in the Aleutian Islands, under the direction of the U.S. Coast Survey, a work on which he has been engaged during the past two years. His labours have been principally in Alaska and the adjacent islands, from which he returned last September, having gone there in the summer of 1871. He spent last winter in San Francisco, in preparing for the expedition of the present year, which included fitting out a vessel expressly for this service. Among other objects contemplated is the selection of an island on the western extremity of the Aleutian chain, to serve for the landing of the Japan cable, for laying which the U.S. steamer *Narragansett* has been detailed to make deep-sea soundings. Mr. Dall is assisted by Prof. Baker, of Ann Arbor, Mich., astronomer.

Mr. Henry W. Elliott is at the head of a private expedition to St. Paul and St. George, the fur-bearing seal islands of Bering's Sea. He has the assistance of Captain Bryant, who is in charge of the U.S. Revenue and other Government interests on these islands. Mr. Elliott is making exhaustive collections in natural history, which he sends to the National Museum at Washington, his investigations respecting seals and walruses being especially valuable and complete. His labours during 1872 were demonstrated by twenty large boxes of collections. He is a very skilful draughtsman, and his drawings of natural subjects are remarkable alike for accuracy and vigour.

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE*

V.

IV.—*The Metric System*

As a system of weights and measures, constructed on strictly scientific principles, the metric system may justly claim pre-eminence over all others. It was established upon the fundamental basis of the *metre*, its primary unit of length bearing a determinate decimal ratio to one of the largest natural constants, that is to say, the ten-millionth part of the earth's meridian-quadrant. It includes a fixed relation between the units of weight and capacity, the *kilogramme* and the *litre*, and the unit of length, the *metre*, from which both are derived; and it comprehends a uniform decimal scale of multiples and parts of these units. It must, however, be admitted that the more recent progress of modern science has demonstrated that the actual standards of metric length, weight, and capacity do not exactly correspond with their scientific definition; and apart from the insuperable difficulties which have been found to exist in the precise determination of material standards from any natural constant, the unanimous opinion of several of the highest scientific authorities in this country has been deliberately expressed that there is no practical advantage in adopting a unit founded in nature over one of an arbitrary character. In truth, the great advantage of the metric system consists in the simplicity and uniformity of its decimal scale, and the great convenience of this scale for all purposes of account as agreeing with the decimal system of notation, and more especially when combined with a decimal coinage which formed part of the original scheme. These undoubted advantages have proved the chief recommendations to the adoption of the metric system, first by France, and afterwards by so many other countries, and generally in the scientific world. There is now every prospect of the metric system being generally adopted in all countries of the civilised world, thus greatly enhancing its value as a common international system of weights and measures, and constituting, as it were, a universal language for expressing all quantities weighed or measured.

The original steps which led to the establishment of the metric system in France were taken with a view of reforming the old French system of weights and measures, which had become intolerable from their defective state and want of uniformity. In 1790, on the motion of M. Talleyrand, in the National Assembly, the question of the formation of an improved system to be based upon a natural constant, was referred to the French Academy of Sciences. A request was also made at the same time to the British Government that the Royal Society should act jointly with the French Academy, but no response was given to the invitation, in consequence of the distrust then entertained in this country at the progress of the revolutionary party in France. The preliminary work was consequently entrusted to five of the most eminent members of the French Academy, Lagrange, Laplace, Borda, Monge, and Condorcet. The important Report of this Committee, which bears also the signature of a sixth member, Lalande, gave rise to the metric system. It was presented to the Academy on March 19, 1791, and is printed at length in their Memoirs. The choice of the fundamental unit of the new system lay in its derivation either from the length of the seconds-pendulum, of the earth's equator, or of the earth's meridian. The Committee rejected the length of the pendulum beating seconds as the basis of the new standard unit of length, because it involved a heterogeneous element, that of time, as well as an arbitrary element, the division of the day into 86,400 seconds. They proposed a unit of length taken from the dimensions of the earth itself, and not dependent upon any other quantity; and they did not hesitate to select as its basis the quadrant of the meridian in preference to a quadrant of the equator, from its being a universal measure applicable to all countries, as every country was placed under one of the meridians of the earth, whilst only few countries are under the equator. They considered also that no greater dependence could be placed upon the regularity of the equator, than upon the equality or regularity of the several meridians. They recommended the ten-millionth part of the quadrant of the meridian as the definition of the new fundamental unit of length. Renouncing the ordinary subdivision of the meridian-quadrant into degrees, minutes, and seconds, they proposed a uniform decimal scale as practically the best, from its agreeing with the scale of arithmetical notation. In order that no other arbitrary principle should be introduced into the new system of weights and measures, they recommended for the basis of the unit of weight a measured quantity of distilled water, being a homogeneous substance, always to be easily found in the same degree of purity and density; and that such quantity should be weighed in a vacuum at its temperature when passing from a solid to a liquid state.

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* Continued from p. 370

For the practical purpose of ascertaining the length of the meridian quadrant, they proposed to measure an arc of the meridian from Dunkirk to Barcelona, a distance of nearly 9⅓°, and comprehending about 6° to the north and 3½° to the south of the mean parallel of latitude. These extreme points had also the advantage of being both at the sea level. The actual operations required were stated to be as follows:—

1. To determine the difference of latitude between Dunkirk and Barcelona.

2. To re-measure the ancient bases which had served for the measurement of a degree at the latitude of Paris, and for making the map of France.

3. To verify by new observations the series of triangles employed for measuring the meridian, and to prolong them as far as Barcelona.

4. To make observations in lat. 45° for determining the number of vibrations in a day, and in a vacuum at the sea level, of a simple pendulum equal in length when at the temperature of melting ice, to the ten-millionth part of the meridian quadrant, with a view to the possibility of restoring the length of the new standard unit, at any future time, by pendulum observations.

5. To verify carefully and by new experiments the weight in a vacuum of a given volume of distilled water, at the temperature of melting ice.

6. To draw up tables of existing measures of length, surface, and capacity, and of the different weights in use, in order to ascertain their equivalents in the measures and weights of the new system, as soon as they should be determined.

In pursuance of the recommendations of this Report, the law of March 26, 1791, was passed by the National Assembly for constructing the new system upon the proposed basis; and the Academy of Sciences was charged with the direction of the necessary operations. They entrusted the measurement of the arc of the meridian from Dunkirk to Barcelona to two of their members, Méchain and Delambre, who carried on the work during seven years, from 1791 to 1798, notwithstanding many great difficulties and dangers.

The unit of measure adopted for the actual measurement was the existing French standard of length, the Toise of the Academy, better known as the *Toise de Perou*, a measure of 6 French feet (*Pieds du Roi*). This standard is now deposited at the Observatoire at Paris. It is a bar of polished iron, about 1⅛ inch in breadth, and ⅓ inch in thickness, and a little longer than a toise. The length of a toise is marked by a rectangular step near each end of the bar, leaving the remaining portion at the ends half the thickness of the measuring part of the bar.

The true length of the toise was taken about a line (or $\frac{1}{13}$ inch) above the re-entering angles of the bar, at the temperature of 13° Réaumur, or 16°·25 C. It has been declared to be equal to 76·7563 English inches, the old French foot (which was divided into 12 inches and the inch into 12 lines), being equal to 12·792 English inches. The toise was afterwards found to be equal to 1·94904 metre.

This standard had been originally constructed as the unit for measuring an arc of the meridian in Peru, and for verifying the meridian of Paris, in 1740; and it was substituted in 1766 for the more ancient French standard of length, the *Toise du Grand Chatelet*, from which it had been originally derived. This older toise was deemed wanting in the scientific precision requisite for a standard of length. It had been constructed in 1668, and is said to have been 5 lines shorter than the toise measure then ordinarily used, for which no authoritative standard could be found; and to have been actually derived from the width of the inner gate of the entrance to the Louvre, which, according to the original plan, was made 12 feet wide, and one half of this width was taken for the length of the standard toise.

The measures actually used for the survey operations are known as the *Règles de Borda*. They were four in number, each consisting of a bar of platinum two toises, or 12 French feet, in length, about ½ inch broad, and $\frac{1}{12}$ inch thick. Each platinum bar was fixed at one end only to a bar of brass about 11½ feet long, the other end of the platinum bar being free and extending about 6 inches beyond the corresponding end of the brass bar. The object of this second bar was that it should form, together with the first bar, a metallic thermometer, indicating the temperature of the two bars by their difference of dilatation, which could be measured by a fine vernier. The four measuring bars were accurately verified, and found, when placed together, end to end, not sensibly to differ from eight times the length of the Toise of Peru at the temperature of 12°·5 C.

The base for the measurement of the northern portion of the work was measured at Melun, and found to be 6075·90 toises. The base for the southern portion was measured at Perpignan, and found to be 6006·25 toises.

Meanwhile the Academy of Sciences was abolished in 1793, by a decree of the National Convention, and a Commission of eleven scientific men, consisting principally of those who had been previously engaged in the proceedings, was appointed, in 1795, to carry out all the arrangements for the definitive establishment of the Metric System. In 1798, towards the close of the operations, an equal number of scientific men, representatives of foreign countries, were added to the Commission, which was then composed as follows:—

French Members: MM. Borda, Brisson, Coulomb, Darcet, Delambre, Lagrange, Laplace, Lefevre-Gineau, Legendre, Méchain, de Prony.
From the Batavian Republic: Aeneae, Van Swinden.
Sardinia: Balbo, afterwards replaced by Vassali, from the Provisional Government of Piedmont.
Denmark: Bugge.
Spain: Pédrayés, Ciscar.
Tuscany: Fabbroni.
Roman Republic: Franchini.
Cisalpine Republic: Mascheroni.
Ligurian Republic: Multedo.
Helvetian Republic: Trallès.

The final results of all the operations for determining the new metric unit of length, were stated by the Commission in their Report, dated April 30, 1799. They found:—

1. That the length of the arc of the meridian comprehended between Dunkirk and Barcelona, was 9·6738° (or 9° 40′ 45″), and measured 551,584·72 toises.

2. Assuming, from the previous measurements in France and Peru, that the mean ellipticity of the earth was $\frac{1}{334}$, they computed the length of the meridian-quadrant to be 5,130,740 toises.

3. That the length of the new unit of length, the ten-millionth part of the meridian-quadrant, was equal to 0·5130740740 toise, or 3 feet and 11·296 lines; being 443·296 lines of the Toise of Peru (which contained 864 lines), at its standard temperature of 16°·25 C. In terms of the new standard unit, the Toise of Peru was equal to 1·949036591 metre.

4. That the length of the pendulum at the temperature of melting ice, beating seconds in a vacuum at the sea level at Paris, was equal to 0·99385 metre.

The actual construction of the new standard measure of length had been entrusted to the mechanician Lenoir. As a preliminary proceeding, he made four end-standard metres of brass, differing in length very slightly from each other, and each about equal to 443·242 lines of the Toise of Peru. This was the computed length of one ten-millionth part of the meridian-quadrant, as deduced from the previous measurements of an arc of the meridian in France made in 1740. The length of these four brass metres, when placed end to end, was nearly 1,773 lines, thus exceding double the length of the Toise of Peru, by about 45 lines. Lenoir constructed a supplementary measure of this excess of length, and its exact relation to the toise was ascertained by numerous comparisons, for which other intermediate measures were employed, and their exact length determined. The actual comparisons of the four brass metres were made not with the Toise of Peru itself, but with two standard toises constructed by Lenoir, the length of each of which in relation to the Toise of Peru had been carefully determined. In these comparisons the additional length of the measure of 45 lines was also employed. The comparing instrument was a *comparateur* made by Lenoir, which enabled very minute differences in measuring bars under comparison to be read off on a subdivided scale by means of a contact lever. One division of this scale was equal to 0·00001 toise, and one-tenth part of one of these divisions (= 0·001949 mm.) could be read off with the aid of a vernier. It appears from the Report of MM. Borda and Brisson, dated July 17, 1795, that the result of a number of comparisons, including those of the four metres with each other, showed metre No. 2 to be nearest to the required length, being 443·4519 lines of the Toise of Peru at the mean temperature during the observations of 12°·96 Réaumur, thus very closely approaching its standard temperature of 13° Réaumur, and exceeding the required length at this temperature by only 0·0119 line. It was accordingly selected as the provisional Standard Metre. But they considered that its standard temperature would more conveniently be fixed at 10° C., and as, according to Borda's determination, the coefficient of dilatation of brass between 0° and 32° C, was 0·00001783 for 1° C. they determined its length at 10° C. to be 443·401 lines of the Toise of Peru.

For obtaining the definitive standard, which was to be the length of 443·296 lines of the Toise of Peru at 16°·25 C., which was thus so nearly indicated by the provisional metre, two standard metres of platinum, and twelve metres of iron, were constructed by Lenoir, his comparing apparatus having been improved so as to show differences of 0·001 line. The Commission were not satisfied with making numerous comparisons of these metres and the provisional metre of brass among themselves, but they also compared them repeatedly with the four *Règles de Borda* and a new supplementary measure of above 45 lines, so as to determine not only their relative and absolute length, but also the rates of expansion of the three metals of which they were composed. The rates of expansion definitively adopted by the Commission, from observations made by Borda between 0° and 32° C., were as follows:—

In a metre.
Coefficient of linear expansion of platinum for 1° C. = 0·00000856, or 0·0031 mm.
" brass " = 0·00001783, or 0·0092 "
" iron " = 0·00001156, or 0·0063 "

The comparisons and corrections of the several metres were continued until no difference amounting to 0·000001 toise, or 0·001 millimetre, could be found at the temperature of melting ice, either in their desired absolute length of 443·296 lines of the Toise of Peru or in relation to each other. They were consequently all determined to be perfectly exact. One of the platinum metres, subsequently known as the *Mètre des Archives*, from its place of deposit, was reserved as the new prototype measure of length; the other was kept at the Observatoire at Paris, as its accessible representative. The twelve iron standard metres were distributed amongst the several countries represented at the Commission.

The primary *Mètre des Archives* is a rectangular platinum bar, bearing no mark or description. Its breadth is 25 mm. (0·984 in.), its height 3·5 mm. (0.138 in.). Its ends are planes perpendicular to its axis of length, and the straight line between them in this axis denotes the true length of the metre at 0° C., or the temperature of melting ice. It thus constitutes what is termed a *Mètre-à-bouts*, or end-standard metre.

The unit of metric weight was defined to be the weight in a vacuum of a cubic decimetre of distilled water at its maximum density, or the temperature of 4° C. Distilled water was selected as the best material in nature for thus determining the unit of weight, from its being obtainable everywhere and at all hours in the greatest purity, its being perfectly homogeneous, and its density being invariable at any given temperature. It was required first accurately to ascertain the weight of this volume of water, and then to construct a metallic standard of equivalent weight. The necessary operations for effecting both these objects were entrusted to M. Lefevre-Gineau in 1795. He had to decide between two modes of proceeding for accurately determining the volume of water to be weighed; one, by measuring the internal capacity of a vessel to contain this volume of water; the other, by measuring externally a solid or hollow body, in order to ascertain the weight of the volume of water displaced by it. He chose this last method, considering that the accurate external measurement of a metallic body was much less difficult than that of the internal capacity of a metallic vessel; and it was determined that the best form of this body was a cylinder of a height equal to the diameter of the base, this form being capable of being made and measured with the greatest precision.

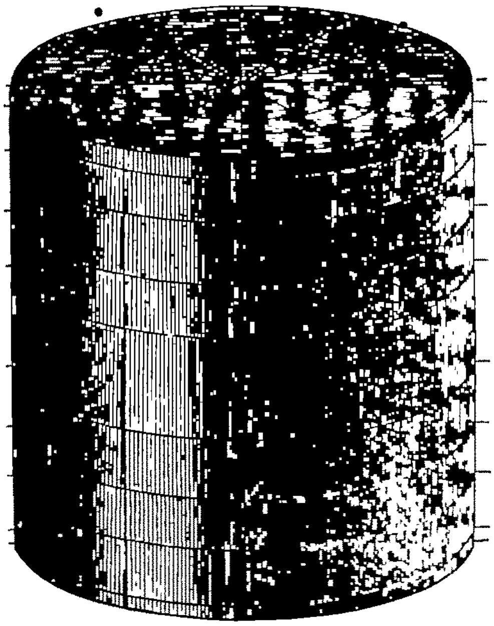

Fig. 10.—Cylinder for determining cubic centimetre.

It was not thought requisite that the cylinder should be of the specified volume of a cubic decimetre, but only of the most convenient size for arriving at the desired result by computation. The cylinder actually used was made of brass, and hollow, being only so much heavier than the same bulk of water as to enable it to sink by its own weight when plunged in water. It was intended to be 2·435 decimetres (about 9½ inches) in diameter and height.

To facilitate the accurate measurement of the cylinder, 12 radial lines or 6 diameters were drawn on its base plane, dividing it into twelve equal parts; and corresponding lines were drawn on its upper plane. The ends of these two series of lines at the circumference were joined by vertical lines on the cylinder, thus dividing it vertically into twelve equal parts. Circular lines were also traced on the two plane surfaces at about 11 mm. from the circumference, and at half and two-thirds of the radius from the centre; and eight horizontal lines were drawn around the cylinder at the following distances from the base:—13, 35, 67, 95, 148·5, 176·5, 208·5, 230·5 millimetres. The height of the cylinder was determined from the ascertained mean distance of the corresponding 37 points of intersection of the lines on the upper and lower surfaces, including the centres. The diameter of the cylinder was determined from the ascertained mean length of the 48 diameters, included between the corresponding points of intersection on its cylindrical portion.

The measurement was effected by means of an apparatus specially constructed for the purpose by Fortin, and it indicated minute differences of length of $\frac{1}{1000}$ line, or $\frac{1}{1400}$ mm.. The standard measures used for determining the absolute length measured were 16 brass measures specially constructed for the purpose, each very nearly equivalent to the height of the cylinder, and 16 other measures, each nearly equivalent to its diameter. The length of each of these two series of measures in relation to each other was ascertained by numerous observations with the new apparatus; and the total length of each set of 16 measures in relation to the new standard unit was obtained by comparing the sum of their length with Borda's *règle* of 2 toises, No. 1, to which they very nearly corresponded in length, by means of the *comparateur* used for the comparison of these large measuring rules.

The final result of the measuring operations was that the mean height of the cylinder was determined to be 2·437672 decimetres, and its mean diameter 2·428368 decimeters, at the temperature of 17°·6 C. According to Borda's determination of the coefficient of the linear expansion of brass, the volume of the cylinder was determined by computation to be nearly 11·28 cubic decimetres, when at the temperature of melting ice.

For ascertaining the weight of water displaced by this cylinder, a series of brass weights was specially constructed, consisting of a unit or provisional kilogram, made as nearly as possible of the estimated weight of a cubic decimetre of water, together with 11 exact copies and smaller weights in decimal subdivision down to the millionth part, all carefully verified and deemed to be accurate within less than half of one-millionth part.

The mean weight of the cylinder in ordinary air was taken, no reduction to a vacuum being deemed requisite, as the weights used were of similar metal to the cylinder, the interior of which communicated with the external air. For this purpose a metallic tube, 1·285 mm. in diameter, was screwed to the top of the cylinder, its end being out of the water when the cylinder was immersed. The top of the cylinder was 43 mm. from the surface of the water during the weighings, and the volume of the tube immersed was therefore 55·77 cubic mm. Taking the volume of the cylinder to be 11·28 cubic decimetres, the volume of the metallic part of the cylinder was computed to be 1·506 cub. decim., and of the hollow part filled with air 9·774 cub. decim. During the weighings the cylinder was surrounded with ice, but the temperature of the water was never below 0°·2 C. and the mean temperature was 0°·3. The final results of the weighings were declared to be as follows:—

	Prov. kilo.
Weight of the cylinder in air, in terms of the unit employed	= 11·4660055
Its mean weight in distilled water, after deducting the weight of air in the cylinder, and of the air displaced by the weights used	= 0·1967668
Hence weight of the volume of distilled water equal to the volume of the cylinder	= 11·2692387

H. W. CHISHOLM

(*To be continued.*)

NOTES

IT is announced that the Transatlantic Balloon will leave New York to-day. It will carry four passengers—Prof. Wise and Mr. Donaldson, the aeronauts; an officer of the United States Signal Service, and an agent of the *Daily Graphic*. They hope to reach some point on the English or Continental coast in about sixty hours from their departure from New York. They have with them six very powerful and experienced carrier-pigeons, purchased in Belgium, which, if liberated from the balloon within "pigeon flight" of the coast, are expected to fly directly to their old homes. Each of these has painted on his breast, in indelible ink, the outline of a balloon, and on his wings the words, "Send news attached to the nearest newspaper." Despatches received by these pigeons should be sent to the nearest newspaper for publication. We wish these daring men a safe landing; but while we do this we regard the enterprise as one needlessly hazardous, so far as the settlement of the scientific problem is concerned.

MR. CAMPBELL, the Chief Secretary of the Inspectorate-General of Customs in China, is now in Europe with a view of obtaining instruments for a complete chain of meteorological stations in that country. It is also proposed to transmit weather information all along the east coast of Asia. This is great news, and we shall return to this important matter, giving full details of the proposals.

MISS ELIZABETH THOMPSON, of New York, has made a donation to the American Association for the Advancement of Science of 1,000 dols., for the purpose of advancing scientific original research; and she intends repeating the donation annually during her life.

M. STEPHAN has succeeded in finding Faye's Comet. The correction of the Jahrbuch Ephemeris is almost *nil*.

MR. FROUDE, who is now with the *Devastation*, informs us that it is Mr. W. Barlow, not himself, who is president of Section G at the ensuing meeting of the British Association. Mr. Froude will, indeed, probably not even be able to attend the Bradford meeting at all.

WE learn from the *Monthly Microscopical Journal* that Prof. Gegenbauer, of Jena, the well-known comparative anatomist, has been nominated Professor of Anatomy and Director of the Anatomical Institute in the University of Heidelberg.

THE arrangement made by Prof. Henry, of the Smithsonian Institution, a few months ago, for the interchange between America and Europe, by Atlantic cable, of important astronomical discoveries and announcements, appears to have borne excellent fruit. One great object of this movement was to enable astronomers in all parts of the world to concentrate attention upon any celestial phenomenon before too great a change of place had occurred, or before the intervention of a long period of moonlight after the first discovery. On the 26th of May last Prof. Henry announced a new planet, discovered by Prof. Peters, to the Observatory of Paris, among other institutions, and on the following night it was looked for by the director of the Observatory of Marseilles, who at once detected it, and subjected it to a careful criticism. The announcement of three planets has thus far been made from the Smithsonian Institution to Europe; the only return communication being that of a telescopic comet, discovered at Vienna on July 5. On being notified of the fact, Prof. Hough, of the Dudley Observatory at Albany, made search for it, and succeeded in finding the object without any difficulty.

BIOLOGY is flourishing at the Antipodes. The last mail has brought us "Australian Vertebrata, fossil and recent," by G. Krefft, curator and secretary of the Australian Museum, Sidney; a list of Australian Longicorns, chiefly described and arranged by Francis P. Pascoe, with additional remarks by George Masters, assistant curator of the Australian Museum; Guide to the

Australian Fossil Remains exhibited by the trustees of the Australian Museum, by G. Krefft, curator and secretary; a Catalogue of the Marine Mollusca of New Zealand, by Capt. F. W. Hutton, assistant geologist; and a paper on the Geographical Relations of the New Zealand Fauna, by the same.

WE have received from the Science and Art Department the following list of Queen's Medallists in the Science Examination, May 1873; we regret that want of space compels us to give only the gold and silver medallists.—Practical Plane and Solid Geometry: Atkinson, Roger, Crewe Mech. Inst., gold; Millington, F. H., Patricoft Mech. In., silver.—Machine Construction and Drawing: Daltry, Thomas L., Newcastle, Elswick Mech. In., gold; Atkinson, Roger, Crewe Mech. In., silver.—Mathematics: McAlister, Donald, Liverpool In., gold; Edwards, Harry H., Liverpool In., silver.—Theoretical Mechanics: McAlister, Donald, Liverpool In., gold; Sisson, William, Newcastle Mech. In., silver.—Applied Mechanics: Millington, Fred. H., Paticroft Mech. In., gold (obtained gold medal in 1872); Dixon, Samuel, Manchester Mech. In., gold; Daltry, Thomas L., Newcastle, Elswick Mech. In., silver.—Acoustics, Light, and Heat: Martin, T. W., Newton Abbott, gold; McAlister, D., Liverpool Inst., silver.—Magnetism and Electricity: McAlister, Donald, Liverpool Inst., gold; Louis, Henry, Islington Sci. and Art Sch., silver.—Organic Chemistry: Whiteley, John, Halifax W. M. Coll., gold; Taylor, William D., Belfast, W. M. Inst., silver.—Geology: Dowlen, Ethelbert, Woking, St. John's, gold; Southeran, Arthur, Marske Inst., silver.—Vegetable Anatomy and Physiology: Dowlen, E., Guildford Science, silver.—Navigation: Windass, John T., Hull Nav. Sch., gold; Daws, Thomas, Plymouth, Courtenay Street Sch., silver.—Nautical Astronomy: Lawson, Henry, Hull Nav. Sch., silver (obtained silver medal in 1872); Ashford, Joseph, Hull Nav. Sch., silver.—Steam: Fairweather, James, Glasgow, Anderson Univ., gold; Daltry, Thomas L., Newcastle, Elswick Mech. Inst., silver.—Physical Geography: Forbes, James L., Torphins Sci. Sch., gold; Armstrong, J. W., Blackburn School of Science and Art, silver.

MR. J. WOOD-MASON, of Queen's College, Oxford, is to officiate as Professor of Comparative Anatomy and Curator of the Comparative Anatomy Section of the Medical College Museum, Calcutta, during the absence, on furlough, of Dr. J. Anderson.

MESSRS. LONGMANS announce the following among their forthcoming scientific publications:—A new volume of Transatlantic Travel, entitled "The Atlantic to the Pacific; What to See, and How to See it," by John Erastus Lester, M.A., author of "The Yo-Semite, its History, Scenery, and Development." A study of Asiatic savage life, entitled "A Phrenologist amongst the Todas, or the Study of a Primitive Tribe in South India—History, Character, Customs, Religion, Infanticide, Polyandry, Language," by William E. Marshall, Lieut.-Col. of H.M. Bengal Staff Corps. A second Supplement to Watts's "Dictionary of Chemistry." The first Supplement, bringing the record of chemical discovery down to the end of the year 1869, was published in 1871. The second Supplement, now in course of preparation, is intended to bring the record of discovery down to the end of 1872, including also the more important additions to the science published in the early part of 1873. This Supplement will form a volume of about 800 pages, and is expected to be ready in the year 1874. The author has been fortunate in securing the co-operation of several of his former contributors. A new work on "Sidereal Astronomy," by R. A. Proctor. "Introduction to Experimental Physics, Theoretical and Practical, including Directions for Constructing Physical Apparatus and for Making Experiments," by Adolf F. Weinhold, Professor in the Royal Technical School at Chemnitz, translated and edited (with the author's sanction) by Benjamin Loewy, F.R.A.S., with a Preface by G. C. Foster, F.R.S., Professor of Physics in University College, London. "A Treatise on Practical, Solid, or Descriptive Geometry, embracing Orthographic Projection and Perspective or Radial Projection," by W. T. Pierce, Architect, late Lecturer on Geometrical Drawing at King's College, London, and at Harrow School. "On the Sensations of Tone, as a Physiological Basis for the Theory of Music," by H. Helmholtz, Professor of Physiology, formerly in the University of Heidelberg, and now in the University of Berlin, translated from the third German Edition by Alexander J. Ellis, F.R.S., formerly Scholar of Trinity College, Cambridge. "Organic Chemistry," by H. E. Armstrong, Ph.D., Professor of Chemistry in the London Institution; "A Manual of Qualitative Analysis and Laboratory Practice," by T. E. Thorpe, F.R.S.E., Professor of Chemistry in the Andersonian University, Glasgow, and M. M. Pattison Muir; "Telegraphy," by W. H. Preece, C.E., Divisional Engineer Post Office Telegraphs, and J. Sivewright, M.A., Superintendent (Engineering Department) Post Office Telegraphs; "Elements of Machine Design, with Rules and Tables for designing and drawing the Details of Machinery," adapted to the use of Mechanical Draughtsmen and Teachers of Machine Drawing, by W. Cawthorne Unwin, B.Sc. Assoc. Inst., C.E., Professor of Hydraulic and Mechanical Engineering at Cooper's Hill College; "Principles of Mechanics," by T. M. Goodeve, M.A., Lecturer on Applied Mechanics at the Royal School of Mines, and formerly Professor of Natural Philosophy in King's College, London. These five works form part of the series of text-books now being published by the Messrs. Longmans.

AMONG Messrs. Macmillan's announcements of forthcoming works are—"On the Theory of Sound," by Lord Rayleigh, F.R.S.; "Contributions to Solar Physics," by J. Norman Lockyer, F.R.S., with numerous illustrations; "Cave Hunting," by W. Boyd Dawkins, F.R.S., being researches on the evidence of caves respecting the early inhabitants of Europe; "The Origin and Metamorphoses of Insects," by Sir John Lubbock, F.R.S. (vol. ii. NATURE Series); and a new edition of Canon Kingsley's "Glaucus."

DURING the ensuing season Messrs. H. S. King and Co. will publish the following new volumes of their "International Scientific Series":—"Mind and Body," by Alex. Bain, LL.D.; "Animal Mechanics," by J. Bell Pettigrew, M.D., F.R.S.; "Principles of Mental Physiology," by W. B. Carpenter, LL.D., F.R.S.; "On the Conservation of Energy," by Prof. Balfour Stewart; "The Animal Machine, or Aërial and Terrestrial Locomotion," by Prof. C. J. Marey; "The Study of Sociology," by Herbert Spencer. With the exception of the last-named work, the whole of the above will be illustrated.—Messrs. H. S. King and Co. also announce the following books of interest to scientific men:—"Studies of Blast-furnace Phenomena," by M. L. Gruner, translated by L. D. B. Gordon; "The Norman People and their Existing Descendants in the British Dominions and the United States of America," and "The History of the Natural Creation," a series of popular Scientific Lectures on the Theories of Progression of Species, by Prof. Ernst Haeckel.

MR. VAN VOORST has recently published new editions of "Blackwall's Researches in Zoology, Illustrative of the Structure, Habits, and Economy of Animals," and Salvin and Brodrick's "Falconry in the British Isles."

PROF. E. D. COPE has been bold enough, in the August number of the *Penn Monthly* (Philadelphia), to portray his conception of the general external appearance of the new gigantic mammal from Wyoming, named *Tinoceras anceps* by Marsh, and *Loxolophodon cornutus* by himself. The result is an elephantine form, with elephantine knees, feet, ears, and tail; bovine preputial sheath; and a head with two pairs of somewhat cervine horns, and an anterior pair of simple but diverging processes. A proboscis about half as long as the head is made to project forwards in a Tapir-like manner, below the base of which the upper canines descend in a way which shows that it would be impossible to use them for defence or obtaining food, without doing great injury to the sensitive trunk which overshadows them. Nothing seems more illogical than the assumption, that because an animal has elephantine proportions and feet, it should possess a proboscis, especially when all arguments from the skull tend in a different direction.

THE Quarterly Weather Report, from July to September, contains, in addition to the usual tabular results, a discussion of four years anemometrical results for Bermuda.

WE have received the Report on the Freshwater Fish and Fisheries of India and Burmah, by Surgeon-Major Francis Day, Inspector-General of Fisheries in India.

WE have received from Prof. Edward Morse an excellent paper, read by him before the Boston Society of Natural History, on the Systematic Position of the Brachiopoda, in which, from a careful study of the anatomy and development of those animals, he has been led to endorse and substantiate Steenstrup's opinion as to their affinities being with the Annelids instead of with the Mollusca, as generally believed. The following is his concise summary:—"Ancient Chaetopod worms culminated in two parallel lines—on the one hand in the Brachiopodæ, and on the other in the fixed and highly cephalized Chaetopods. The divergence of the Brachiopodæ, having been attained in more ancient times, a few degraded features are yet retained, whose relationships we find in the lower Vermes; while from their later divergence the fixed and cephalized Annelids are more closely allied to present free Chaetopods." The author lays stress on the certainly soft and uncalcified condition of the earliest forms of life causing great imperfection in the earliest geological record.

IN the death of Mr. William S. Sullivant, which is recorded in the scientific columns of *Harper's Weekly*, and which took place at Columbus, Ohio, on April 30 last, the United States has lost one of its most accomplished botanists, especially in the department of the mosses, in which he was the recognised head for many years. From a biographical notice published by Professor Gray in the *American Journal of Science*, we learn that Mr. Sullivant was born in 1803, near Columbus, in the vicinity of which place he resided the greater part of his life. His first publication appeared under the title of *Musci Alleghanienses*, a work on the mosses and liverworts of the Alleghany Mountains, illustrated by prepared specimens of the plants themselves. This was shortly after 1843, and a few years later a work on the same subject was published in successive numbers as a memoir of the American Academy. The section of Mosses and Hepaticæ in Prof. Gray's *Botany of the Northern United States* was prepared by Mr. Sullivant, and credited to his pen. A separate edition was subsequently published by the author. A work on the mosses of Cuba was prepared by him, illustrated by specimens collected by Mr. Charles Wright. He also published, in 1859, the account of the mosses collected by the Wilkes expedition. The most important of Mr. Sullivant's publications, however, consists of his *Icones Muscorum*, being "figures and descriptions of most of those mosses peculiar to Eastern North America which have not been heretofore figured"—this forming an imperial octavo volume with 129 copper-plates. It is stated by Prof. Gray that a second or supplementary volume of *Icones* was in preparation by Mr. Sullivant, and nearly completed at the time of his death.

THE additions to the Zoological Society's Gardens during the past week include two Mouflons (*Ovis musimon*) from Sardinia, presented by Mr. H. E. Holloway; two Barbel (*Barbus vulgaris*) and a Bream (*Abramis brama*) from British seas, presented by Mr. E. S. Wilson; two Sacred Ibises (*Geronticus æthiopicus*) from Gough's Island; a Black-handed Spider Monkey (*Ateles melanochir*) from Central America, purchased; five Horned Lizards (*Phrynosoma cornutum*) from California, deposited.

SPÖRER'S OBSERVATIONS ON THE SUN*

THE author gives chiefly the results of his spectrum observations, and simultaneous spot observations, recorded in the Transactions of the Berlin Academy of Sciences for November 1871, and May 1872. To the two earlier instances of striking changes observed in the protuberances, there is added an interesting observation of August 8, 1872. It was estimated that the prolongation of the upper part of the protuberance had a velocity of forty-two kilometres per second, parallel to the sun's surface. In the case of many protuberances, it will be readily allowed that they are not only subject to cyclones, but also owe their origin to them. Protuberances of similar form, observed on several successive days, in the same heliographic latitude, Sporer has accounted for, by the supposition of volcanic eruptions, owing to the smaller rate of linear rotation of the deeper strata; if, however, we regard these protuberances as the results of cyclones, the explanation of the changes of position would rest upon the impelling power of the storms, and their tendency to create new forms; and the velocity of the advancing cyclone would, in several instances, average 1·4 kilometre.

Sporer, in this work, adheres to his division of protuberances into two classes. Secchi, in his work on the Sun, has distinguished four classes of protuberances, but afterwards accepted Spörer's twofold division. Both observers are at one in this, that the protuberances, which Sporer has named "flame" and Secchi "ray" protuberances, give different spectral lines, and stand in intimate connection with the spots. But with regard to the proper hydrogen protuberances, Secchi says they are not in the condition to give rise to a spot, against which Sporer adduces examples of their influence in neighbouring spot formation, especially prominent in the intervals between considerable protuberances of hydrogen.

The observation of the protuberance, which Secchi also noticed, on July 7, 1872, and which gave a well-marked image with the line 6543, is particularly described, and drawings are appended.

With regard to observations of spots, interesting comparisons are given, showing the difference between the two hemispheres in respect to the frequency of spots, and the mean heliographic latitudes. In this connection, Carrington's observations, from November 1853 to the beginning of 1861, are gone into, so that the comparisons embrace a period extending from November 1853 to the end of 1871. With regard to frequency of spots, it appears that the southern hemisphere exceeds the northern both in maximum and minimum. The curves also show distinctly the rapid passage from minimum to maximum, and the slow decrease after the maximum.

The mean heliographic latitudes are obtained through assigning to each group of spots, a factor of value (*Werthfactor*). The union of five-rotation periods gave a point of the curve for the northern as well as for the southern hemispheres. Carrington had obtained from his observations the striking result, that the spots at the time of the minimum approach the equator, thereafter veered off to higher latitudes, and that then the more numerously spotted zones gradually approached the equator. Spörer, by his observations since 1861, has confirmed this result.

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* Translated from a review in *Der Naturforscher*, No 29, of Beobachtungen der Sonne, von Prof. Dr. Sporer, Abhandlung zum Programm des Gymnasiums. Separat-Abdruck. Anklam. Verlag und Druck von Richard Poettcke: 1873.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE meeting of the American Association for the Advancement of Science was held this year at Portland, in the State of Maine, during the fourth week of August; there was a large attendance of well-known scientific celebrities and members. The following account, for which we are indebted to the *New York Tribune*, will give an idea of some of the most important papers and discussions.

A discussion on the Darwinian hypothesis, which was started by Prof. Swallow, who is a vigorous opponent, was continued by Dr. Dawson, who began by stating with some fullness of detail the demands upon our credence made by the advocates of the evolution theory. Among other requirements of the theory, he said, it must provide an explanation of the origin of life. To accomplish this the experiments of Bastian were brought forward. Referring to these, he stated that no less an authority than Prof. Huxley, though an evolutionist, had denied their conclusive character and disputed the alleged results. We are expected to admit, in every department to which scientific inquiry relates, that in all things there has been a successive progress from the lower to the higher. Why should we make this admission? What proof is there of it? The recent discoveries of embryology, showing the likeness of early forms of the embryo to other animals of the same families, furnished to the advocates of evolution no real argument in its favour. They proved nothing. Admit if you will the close resemblance of similar bones and general physical structure in the ape and man, it is not the slightest evidence of identity. While it may be true that there is bone for bone in monkey and in man, still it remains that the bones of one are different from those of the other. The making of monkey and of man is explicable quite as readily, to say the least, on the theory of plan as on that of evolution. The history of the growth of an animal has been cited as the evidence of a development from a lower to a higher form. But what are the facts in the case? The egg grows into the animal, and that organism produces an egg again. This is revolution, not evolution.

We are told to accept as a postulate that mind too is a result of development; that the moral as well as the material being is simply a consequence of the evolving process. I do not grudge the naturalists who have adopted such theories the intellectual exercise which is involved, but I regret that much of their labour is wasted, and the results will be burnt when the fires of truth are applied to the chaff they are accumulating. This is not a question of physics that they are arguing, it is one of metaphysics, and it would be well for our children as well as growing scientists if they were taught more of mental and moral philosophy as a basis for such inquiry.

But I thank the students who are thus engaged for some good results of their exertions. They have thereby succeeded in reducing the superfluous numbers of species, and have obtained far better views in respect to classification. Good results will also flow from the profound embryological researches of the day. But I am sorry for the investigators, for their reputations are at stake, and they have chosen a mistaken path.

We are, however, approaching in our studies a correct theory. After its appearance in geological history, every species has a plastic tendency to spread to its utmost limits of form. Then ensues a period of decadence until it may become extinct. This has been set forth in some of my printed memoirs on the plants of the carboniferous series. I believe that a similar process is true of the human race. He referred to the skull of Mentone and its finally developed character—a grandly developed man cerebrally and bodily. The burial of his dead testified to his religious belief. The people of the Cromagno skull age were of a similarly elevated character. The only point of difference from men to-day was in the flattening of one of the leg-bones. This was perhaps a result of the habits of the tribe, running through forests in pursuit of game. It begins to be admitted that the man of Western Europe came in with the modern mammalia at the close of the glacial period. This was a period of decadence, and when the pliocene fauna were dying out and new forms were taking their places. The most ancient form of man is beyond the average standard of modern humanity. If the man of Cromagno or Mentone had been sent to Harvard, he would have been graduated with the full honours of an average American student.

Prof. Morse stated that Dr. Dawson and Prof. Swallow had both misquoted Prof. Huxley, who had said, in respect to the ancient skull referred to, that it might have held the brains of a thoughtless savage, or it might have contained those of a philosopher. Dr. Dawson had referred to only the differences in those remains from those of the man of to-day in respect to the flattened tibia. There were, however, several other characters of a similar nature which Dr. Dawson had not referred to, some of which had been discovered by Prof. Wyman, and had not yet been published. In the existing races of man the *foramen magnum* (the large opening at the base of the skull through which the brain communicates with the spinal cord) exhibited very little change of position in its relation to the rest of the skull, while with the higher primates (apes) this opening is very near the posterior portion of the skull. In eleven ancient skulls from the shell heaps of Tennessee, the *foramen magnum* in every case was nearly an inch further back than in those of present existing races. The powerful muscles on the sides of the head that move the jaws leave a distinct line at their upper points of attachment. These lines are called temporal ridges. In all present existing races a space occurs on the top of the skull, between these lines, of from three-and-a-half to four inches. In the apes these muscles meet in the median line which rises into a bony crest so characteristic of the gorilla. There was a remarkable skull discovered by Prof. Wyman in the lowest beds of the ancient shell heaps of Florida. This has the temporal ridges approaching each other within a half inch at the top of the skull. If the high development of the skull referred to by Mr. Dawson was such as he states, it only carries man further back. Similarly, in the light thrown upon the history of man by the wonderful discoveries in archæology, where we meet with traces of an ancient civilisation, with complicated language and manners, we can surely believe in savage hordes pre-existing from which this ancient civilisation has been evolved.

As to the early traces of man, we must fully appreciate the rare possibility of their occurrence. Wherever you dredge in the waters of the present day the traces of man are among the rarest discoveries. The Lake of Haarlem, upon whose waters naval battles have been fought, and on whose shores a dense population has existed, was drained, and on its bottom not the slightest traces of man's existence were found. Prof. Morse dredged repeatedly for years off the shores of Maine, and no trace of man was ever brought up, except a single spike. When we consider how abundant the material for such remains must be now compared with those furnished by the simple methods of life and the sparse population of earlier days, the indications of man's existence in geological eras must be of the rarest occurrence. In fact, in such rocks as the drift, only the rude stone implements could be preserved.

The evolution theory as compared with that of special creation presented similar features to the undulatory theory of light as compared with the emission theory. Newton's theory required a new modification with every discovery in optics, until, as a writer said at that time, the emission theory is a mob of hypotheses. The undulatory theory of Young not only explained all that was difficult to Newton, but gave physicists the power of prevision. So with evolution. It not only accounts for existing phenomena, from the modification of a flower or the spot on a butterfly's wing to the genesis of the solar system, but it has endowed naturalists with the gift of prophecy and enabled them to predict the intermediate forms afterwards discovered in the records of the rocks.

On Calvert's Supposed Relics of Man in the Miocene of the Dardanelles. By G. Washburn.—The author reports, in view of the facts to which the paper refers, as to the flints, the split bones, and the marks upon the fossil bone, that he believes that Mr. Calvert and Sir John Lubbock (who had never seen the specimens) are mistaken in the conclusions to which they have come, and that they have not been able to find any evidence whatever at the Dardanelles in reference to the antiquity of man.

The Rotation of the Planets as a Result of the Nebular Theory. By Prof. Benjamin Pearce.—Prof. Peirce's paper set forth an explanation of the actual rotation of the planets on the supposition of their being formed according to the nebular hypothesis, from rings thrown off from the rotating main body in the process of condensation. He instanced more particularly the planets Jupiter and Saturn. The inner portions of such a ring having a less velocity than the outer ones, axial rotation in the same direction as that of the primary would be determined in the breaking up and running together of the ring into a planetary body. He showed, by a mathematical analysis of the movements of the particles composing the ring, that the velocity of the resulting rotation must be such as is actually observed in the case of the planets referred to, whose mass represents nine-tenths of the whole planetary system.

In Jupiter and Saturn, the velocity of a particle in the planet is very nearly the velocity of the planet itself. Then Jupiter and Saturn must have derived their material from the whole mass of the planetary system. The best theories of the earth make it of uniformly decreasing density from the surface to the centre. Suppose that after Jupiter were formed it were condensed, that might otherwise explain its velocity. He showed that, in the case of the planets, the velocity, had it been one-half what it actually was, would have resulted in their having no rotation. This theory was applied to the absence of rotation in the case of our satellite. He showed the probability that the original nebular ring from which the planets were formed may have been of twice the size of their present orbits. The nebular theory, to meet the requirements of the mere mathematician, would have placed all the planets at regular distances, and given them exactly similar motion. But not such was the method of nature.

In the discussion which followed he stated that we have never seen anything of Jupiter or Saturn but the clouds which cover them. He thought that those planets were yet at a white heat, and we simply saw the clouds that are raining down upon them. The present state of the satellites may be a result of their tides, and not the index of their original velocity. Jupiter and Saturn took so large a proportion of all the planet-forming material that the laws impressed upon them may serve best to tell the whole history of the solar system. There may be, however, a rotation of the inner mass of those planets of which we know nothing.

Geology of Southern New Brunswick. By Prof. T. Sterry Hunt.—The recent labours under the Geological Survey of Canada, by Messrs. Bailey Matthew and the author, were sketched. They show south and west of the coal basin various uncrystalline formations, all resting upon ancient crystalline rocks. These latter are by the author regarded as for the most part the equivalents of the Green Mountain and the White Mountain series, or what he calls Huronian and Montalban. These are penetrated by granites, and associated in one part with Norian rocks, but the presence of Laurentian is somewhat doubtful. While the author recognises thus, at least, four distinct series of pre-Cambrian crystalline rocks in Eastern North America, he does not question the possible existence of yet other series in this region. The analogies offered by the more recent rocks of this region are very suggestive.

On the Possibility that the Sun, while mainly Gaseous, may have a Liquid Crust. By Prof. Charles A. Young.—There can be very little doubt that Secchi and others, who hold that the sun is mainly gaseous, are correct in this: the smallness of density cannot possibly be explained on any other supposition. At the same time the eruptional phenomena which are all the time occurring on the surface, almost compel the supposition that there is a crust of some kind which restrains the imprisoned gases, and through which they force their way in jets with great violence.

Prof. Young suggests that this crust may consist of a more or less continuous sheet of descending rain, not of water, of course, but of the materials whose vapours exist in the solar atmosphere, and whose condensation and combinations are supposed to furnish the solar heat. As this tremendous rain descends, the velocity of the falling drops would be retarded by the resistance of the denser gases underneath; the drops would coalesce until a continuous sheet would be formed; and these sheets would unite and form a sort of bottomless ocean resting upon the compressed vapours beneath, and pierced by innumerable ascending jets and bubbles. It would have an approximately constant depth in thickness, because it would re-evaporate at the bottom nearly as rapidly as it would grow by the descending rains above, though probably the thickness of this sheet would continually increase at some slow rate, and its whole diameter diminish.

Prof. Young added an explanation of the narrow disc fringes seen at the moment of totality in a total eclipse, showing them to be optical interference effects caused by the sudden changes of the temperature of the air at the edge of the shadow. The twinkling of stars is analogous in many respects.

The Existence of Live Mammoths. By Prof. Feuchtwanger.—The discovery of the mammoths in Siberia in the deep gorges of the mountains near the Lena Viner, which was lately published as having been made by a scientific Russian convict, who had five living animals, twelve feet in height and eighteen feet in length, with projecting tusks four feet long, excites some discussion in Europe. I think it worthy of inquiry whether the mammoth of the past tertiary period, discovered during this century in Siberia, near the same river, can have any relation to the convict's discovery. Thousands of these animals have been found buried in the ice, with their well-preserved skins, and thousands of tusks are brought to England to this day for the use of the turner. These are of nearly the same dimensions as those seen by the Russian. The convict has received an unconditional pardon, on the recommendation of scientific men who have investigated his statements and believe them to be true.

Prof. E. S. Morse, of Salem, Mass., read a paper on the subject of *Variations in Wave Lengths.* Prof. Morse first called attention to the interesting discoveries of Lockyer, Huggins, and others in accounting for the displacement of lines in the spectrum in observations of celestial objects. It is well known that when a star is approaching the observer the luminiferous waves emitted by it are crowded together, and on the contrary are separated when the star is receding.

Mr. Morse brought forward an instrument by which this phenomenon in the case of light may be easily and plainly illustrated before a large audience. The instrument consists of a tank filled with water and set on wheels. On the top of this is a compartment containing compressed air. From one end of the tank a pipe protrudes, which is moved up and down at a fixed rate by simple clockwork. When the cock is opened, allowing the water to escape from the pipe, the stream assumes a sinuous line, which may be shown, if brilliantly lighted, across a large audience hall. This undulatory stream, when the tank is at rest, illustrates a luminiferous wave from a stationary source. To exhibit the shortening or lengthening of the waves of light by the approach or recession of the luminiferous body, Mr. Morse simply moves the apparatus rapidly back and forth on the table. As the apparatus moves with the direction of the stream its undulations are crowded together, and the waves are consequently shortened. On the other hand, when the motion of the apparatus is in an opposite direction, the waves are proportionably lengthened. The advantage of this illustration is that it exhibits precisely what takes place in the luminiferous waves approaching or receding from the observer of celestial bodies, producing the displacement of spectrum lines.

Concerning Hyalonema. By Dr. Samuel Lockwood.—The recent deep-sea dredgings have done much toward clearing up the singularly anomalous history of the Japanese glass-rope sponge. Prof. Lockwood, however, thinks that, either from inappreciation or otherwise, the knowledge thus obtained has not been applied to the elucidation of certain mooted points connected with Hyalonema. With regard to the mistakes in representing Hyalonema "wrong end up," my opinion is that the error was led off by the Japanese themselves. The drawings by the native artists represented these curious objects as attached to the sea bottom by the sponge mass, thus making the fascicle to be erect and uppermost. Obtained by the net, or some such means, from the bottom at great depths, it is supposable that the fishermen at Enoserma were entirely ignorant of the matter. Their theory, however, as represented by the native artists, has wrongly represented the Hyalonema. These ropes attached to the sponge and sand are some distance from the main or upper portion encrusted with parasites. After removing portions of the encrusting case from the fascicle, he could not detect any structural evidence that Polython owed anything for food to the object which had given it local support. It, however, "chums" with the sponge for a purpose of its own. Prof. Lockwood thinks that it draws sustenance from the fishing process of its radiating tentacles.

Both Polyps and sponge provide for themselves. In his view the zoophyte is what we must call a compensal, and could not exist without that sort of support from Hyalonema which the oak affords the vine: and Hyalonema, too, is a compensal; for how long would it endure without the support of Polython? The stem, without this support, would not be able to hold itself erect. Other varieties are supported by stems consisting of sheaves of short spicules, bound together by bony cement. These have and need no supporting Polython. He combated the view that Hyalonema was sunk in the mud up to the neck, arguing that the polyps surrounding the stem could not so live; that it could not use its tentacles to obtain food, and that the position of the egg-cases of the deep-sea shark, the oldest egg being attached, and the most recent at the bottom, sustained this view. Some account was then given of the material structure of the encoating Polython. The essayist spoke of the deep-sea sharks off Setubal, making that place their feeding-ground, because of the facility afforded them to secure these egg-cases by the abundance of the Hyalonemas there.

The Co-efficient of Safety in Navigation · an attempt to ascertain within what Limits a Ship can be located at Sea by Astronomical Observations. By Prof. Wm. A. Rogers.—This was an attempt to ascertain mathematically the average number of miles that a ship may be out of her reckoning. It was a paper of length, indicating long and careful research. It stated that in the case of British vessels there is a continual increase in the proportion of wrecks, as shown in the following :—

British vessels.	Wrecks.
Inc. 1858 over 1848......38 per cent.	Inc. 1862 over 1852......59 per cent.
Inc. 1868 over 1858......44 per cent.	Inc 1867 over 1857......57 per cent.

For 1869 we have a decrease in the number of vessels of 4 per cent., and an increase in the number of wrecks of 21 per cent. The confidence in reckoning by instruments had increased the danger. He considered separately (1) wrecks by causes beyond control; (2) wrecks to obtain insurance; (3) wrecks by deviation of compass; (4) wrecks by errors of observation. He concluded that 70 per cent. of wrecks were from preventible causes. There are 3·3 times as many insured vessels wrecked as uninsured. The ratio of errors in chronometers was illustrated in an elaborate series of tables showing that the navigator must expect from this source an error of 3·6 miles, must be on the look-out for one of 11·5, and must not be surprised at one of 21 miles, all on the supposition that he has an average chronometer. One serious source of error is varying temperature during a voyage. The conclusion was that the navigator who assumes that he can get the place of his ship certainly within five miles, or probably within fifteen, exhibits an over-confidence which may lead to his ruin.

There were other papers of interest, by Prof. Elliott, on International Coinage; by Prof. Wheildon, on the Arctic Regions; by Gen. Barnard, on the Relation of Internal Fluidity to the Precession of the Equinoxes; by Prof. Hilgard, on Transatlantic Longitudes, and on Meridional Arcs; by Col. Whittlesy, on Rivers in the Mississippi Valley; by Prof. Hunt, on Breaks in the American Palæozoic Series; by A. E. Dolbear, on a new method of measuring the velocity of light.

MR. HARTNUP ON DETERMINING THE RATES OF CHRONOMETERS*

THE difficulty in predicting the rate of a chronometer for a voyage arises from the imperfect state of the instrument; and by a well-arranged and carefully conducted test, these imperfections may be so exhibited as to enable the mariner to avoid the danger which must frequently follow from the neglect of such precautions. The Greenwich mean time is now so easily obtained in most seaports, that there can be no difficulty in ascertaining the daily gain or loss of a chronometer, if the rate so found could be depended on. The communication of time to the port of Liverpool, by the firing of the gun which is placed on the Morpeth Dock Pier Head, has been so successful that the difference between the flash of the gun and 1 P.M. Greenwich mean time has not, on any occasion during the past year, been such as could lead to an error in a ship's longitude to the extent of the width of the Mersey opposite the point on which the gun is placed; and by observing the flash of the gun on two occasions at an interval of a few days, the rate of a chronometer may be obtained with sufficient accuracy for most practical purposes. The rate so obtained might, however, differ very much from the rate at sea, if the temperature in which the rate was obtained in port differed much from that to which the instrument was exposed on the voyage.

Imperfect thermal adjustment is a defect so well known, that during the past thirty years the attempts made to improve the quality of marine timekeepers have been mainly confined to the compensation balance. The ordinary balance does not perfectly compensate for the change in the elasticity of the balance-spring, caused by change of temperature, and various forms have been given to balances with the view of attaining greater perfection. Balances have, without doubt, been made to compensate for change of elasticity in the spring throughout long ranges of temperature, but there is evidently some objection to their general adoption for the merchant navy. It is possible that the thinness of the laminæ, and peculiarity in the construction of balances which are made with the view of removing the defect above named, may render them less permanent in their action, and more liable to injury in the hands of a less skilful mechanic than the original maker; but however this may be, the ordinary balance seems to be almost universally used in the merchant navy. This having been found to be the case, about four years ago arrangements were made at the New Observatory for the trial of chronometers in three definite temperatures with the view of showing the amount of change in their rates due to error of thermal adjustment, and more than one thousand marine timekeepers have now been tested in 55°, 70°, and 85° of Fahrenheit. From a careful examination of the records of these tests there appears to be a definite temperature peculiar to each chronometer in which the instrument goes faster than in any other temperature, and as the number of degrees above or below this temperature of maximum gaining rate increases the chronometer loses in a rapidly increasing ratio. If we assume this law of variation to be that the change of rate is directly as the square of the number of degrees from the maximum gaining rate, the rates calculated on that assumption are found sensibly to agree with those obtained from observation; therefore, if we have the rate from observation for each of three definite temperatures, as given in my last two Reports, we can find, by computation, the correction for error of thermal adjustment due to any other temperature. In order to do this it is necessary to find—

T . . the temperature in which the chronometer has its maximum gaining rate,
R . . the rate at the temperature T, and
C . . the factor, or constant number, which multiplied by the square of any given number of degrees from T shows the amount of loss for that number of degrees.

The following example shows the method of calculating C, T, and R from the observed rates in 55°, 70°, and 85°:—

Chronometer, No. 727.

$$\begin{array}{ll} \text{Rate in } 55^\circ = -2{\cdot}92 \ldots r & \\ & r - r' = -1{\cdot}04 \ldots d \\ \quad ,, \quad 70 = -1{\cdot}88 \ldots r' & \\ & r' - r'' = +1{\cdot}25 \ldots d' \\ \quad ,, \quad 85 = -3{\cdot}13 \ldots r'' & \\ & d - d' = -2{\cdot}29 \\ & d + d' = +0{\cdot}21 \end{array}$$

$$C = \frac{2(d - d')}{30^2} = \frac{-4{\cdot}58}{900} = -0{\cdot}00509$$

$$T - 70 = \frac{d + d'}{C \times 60} = \frac{+0{\cdot}21}{-0{\cdot}3054} = -0{\cdot}69$$

$$T = 70 - 0{\cdot}69 = 69{\cdot}31$$

$$R = r' - (T - 70)\frac{d + d'}{60} = -1{\cdot}88 + 0{\cdot}69 \times 0{\cdot}0035 = -1{\cdot}878$$

From the preceding Examples

	Mean Daily Rate in 55°	in 70°	in 85°	C.	T.	R.
No. 727	−2·92	−1·88	−3·13	−0·00509	69·31	−1·88

Let N = any number of degrees from T, then the Rate at $T \pm N = R + C \times N^2$.

Required the Rate of No. 727 at 40°
Here N = 29·31 and $N^2 = 859{\cdot}08$

Therefore the Rate at 40° = $-1{\cdot}88 + (-0{\cdot}00509 \times 859{\cdot}08) = -6{\cdot}25$.

The values of C and T remain the same for long periods; as a rule, they do not sensibly change so long as the adjustments are not altered, and the instrument remains in good condition; but R is more changeable, and should be redetermined on all favourable occasions. To find the change in R the rate must be first carefully found in some definite temperature. Suppose, for example, that at some subsequent time the rate of No. 727 was found to be −2·13, instead of −3·13, in 85°, then the rate at T would be −0·88 instead of −1·88; but it might not be convenient to obtain the rate in either of the temperatures in which the rates are given in the test, and then it may be found as follows:—Suppose the rate has been found to be −1·55 in 81·5, then the rate must be computed for 81·5, on the assumption that R has not changed, and the difference between the rate observed and the rate computed will be the correction to be applied to R.

The computation is as follows:—81·5 − 69·3 or N = 12·2 and $12{\cdot}2^2 = 148{\cdot}84$.

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* Extracted from the Report of the Astronomer to the Marine Committee, Mersey Docks and Harbour Board, for the year 1872.

Therefore, the rate at $81{\cdot}5 = -1{\cdot}88 + (-0{\cdot}00509) \times 148{\cdot}84 = -2{\cdot}64$.

Observed rate in $81{\cdot}5 = -1{\cdot}55$. Computed rate in $81{\cdot}5 = -2{\cdot}64$. The losing rate at T must therefore be diminished by 1·09, making the newly found $R = -0{\cdot}79$ instead of $-1{\cdot}88$.

For any chronometer which has been allowed to remain at the Observatory for a period of five weeks the certificate of test issued with the instrument contains the necessary data for calculating the correction due to imperfect thermal adjustment.

THE WHITWORTH SCHOLARSHIPS

THE following Memorandum on the Whitworth Scholarships, prepared by Sir Joseph Whitworth, has been approved by the Lords of the Committee of Council on Education, South Kensington :—

1. The experience of the past competitions for my scholarships has proved to me the necessity of establishing rules which shall insure that the holders of scholarships shall devote themselves to the studies and practice necessary for mechanical engineering during the tenure of the scholarships.

2. To effect this I propose to the Lords of the Committee of Council on Education that as soon as possible, *i.e.* in the competition of 1875, every candidate for a scholarship should produce a certificate that he has worked in a mechanical engineer's shop, or in the drawing office of a mechanical engineer's shop, for two years consecutively. In 1874 six months' consecutive work only in the engineer's shop will be required. The candidate must be under 22 years of age.

3. The candidate for the scholarship will be examined in the appointed sciences; in smith's work, turning, filing, and fitting, pattern making and moulding, as already established, and the same marks will be awarded as at present.

4. In 1875 and the following years each holder of a scholarship appointed under these new rules will be required to produce satisfactory evidence at the termination of every year that he has made proper advances in the sciences and practice of mechanical engineering by coming up for an examination similar to that which is prescribed for the competition both in theory and practice.

5. The scholarships may be held for three years, but may be withdrawn at the end of each year if the scholar has not made satisfactory progress.

6. The number of scholarships in the competition of 1874 will be reduced from ten to six. Each scholarship will be of a fixed annual value of 100*l.*, together with an additional annual sum determined by the results of the progress made in the preceding year.

7. At the end of each year's tenure of the scholarship, the scholars appointed under these new rules will, as before stated, be examined in theory and in practice in the same manner as in the competition for the scholarships. On the results of this examination the following payments, in addition to the 100*l.* before mentioned, will be made among each year's set or batch of scholars:—To the scholar who does best in the examination, 100*l.*; to the second, 60*l.*; to the third, 50*l.*; to the fourth, 40*l.*; to the fifth, 30*l.*; and to the sixth, 20*l.*; provided that each scholar has made such a progress as is satisfactory to the Department of Science and Art, which will determine if the sum named, or any other sum, shall be awarded.

8. At the expiration of the three years' tenure of the scholarships under these new regulations a further sum of 300*l.* will be awarded in sums of 200*l.* and 100*l.* to the two scholars of each year's set or batch who have done best during their tenure of scholarship.

In this way it will be possible for the best of the scholars at the end of his period of tenure of the scholarship to have obtained 800*l.*, and the others in proportion.

9. The prizes under paragraph 7 will be awarded according to the total number of marks obtained by the students in practice and theory in the examination at the end of the year. The prizes under paragraph 8 will be awarded by adding together the marks obtained by the students at the end of each of the three years.

SCIENTIFIC SERIALS

THE current number of the *Zoologist* commences with a notice by the editor, of Mr. Lloyd's "Official Handbook to the Crystal Palace Aquarium." In an interesting historical sketch of the growth of aquaria, he divides its development during the last forty years into three eras, the earliest being the instructive, the second the poetic and fashionable, and the present the commercial. The early development of the aquarium is then entered into, the work done by Bowerbank, Daubeny, and Warington being fully described. This is followed by a review of Mr. T. J. Moggridge's work on Harvest-Idea that these insects do accumulate seeds in store-houses for winter consumption is correct, contrary to the assertions of Kirby, Latreille, and other high authorities. What is very peculiar is that these seeds scarcely ever show any tendency to gering Ants and Trapdoor Spiders, in which the author, from a careful and painstaking series of excellent observations on the habits of ants, which are described in detail, shows that the old minate, though under apparently very favourable circumstances.—Mr. Cornish notes the occurrence of the following fish at Penzance:—The Black Fish (*Centrolophus pompilus*), the Solenette (*Monochirus linguatulus*), the Braize (*Pagrus vulgaris*), Bloch's Gurnard (*Trigla blochii*), and the Torpedo (*Raia torpedo*).—Mr. F. H. Balkwill, in reply to a critical note which appeared in this journal (NATURE, July 24, p. 252) on a paper by him in the *Zoologist* for July last, objects to his remarks being thrown into the general form; the fact that the forms and arrangements of teeth in vertebrates is practically infinite, being assumed by him. But that such is very far from being the case will be agreed to by all zoologists; the types and arrangements of teeth being extremely few in comparison to what they might be. The argument does not require, as Mr. Balkwill thinks, the proof of the statement that the teeth of the wombat, dog, &c., should be of low type and simple development, which they are not; and he may be assured that all "genuine Darwinists" are of opinion that when two not distant types of animal life are in a position to occupy new and separate regions, the fact that their food can only be obtained from two sources, namely, animal and vegetable tissues, invariably leads to their divergence in two directions only, that is, towards a carnivorous and a herbivorous conformation. Therefore the non-placental type, on occupying Australasia, as well as the placentalia in the rest of the world, have differentiated into flesh-eaters and vegetable-eaters, each having developed, by natural selection, organs suitable for procuring their accustomed diet. It is not therefore to be wondered at that these organs should present many points of similarity in the two main divisions of the Mammalia.

BARON VON MALTZAN gives in the second number of the *Zeitschrift für Ethnologie* for 1873, an account of his travels in Arabia, and points out the various causes which have opposed the advance of our knowledge of its interior. Amongst these religion has acted as the most powerful obstacle, the exclusiveness of the Islam faith having, in fact, so effectually closed the country to modern research, that there are still many spots of which nothing is known beyond what Ptolemy was able to tell us. Baron von Maltzan selected the most southern extremity of the peninsula, which is as yet a *tabula rasa* on our maps, for the scene of his explorations. He draws attention to the artistic skill exhibited by these people in statuary and carving, before they fell under the rule of their Mahomedan conquerors from Central Arabia, when all their earlier civilisation was rudely checked and their language superseded, while they were then also first driven to adopt a monadic mode of life. In spite, however, of amalgamation with central Arabian elements, the population of South Arabia still admits of division into two distinct peoples, the Sabaer and the Himyarites, the former of whom have light yellow skins, while the latter, whose name he derives from *Hamr*, red, are so dark-skinned as to be genrally classed amongst the black races. Baron Maltzan observed a curious physical character in the family of the Himyarite rulers of the Fodli, or Ozmani-State, many of whom, both males and females, had six fingers and six toes on both hands and feet. This peculiarity is looked upon by the people at large as a special mark of blue blood, and prized accordingly by the possessors. It would seem that the practice of forming consanguineous marriages, which prevails in the Fodli, as in other ruling houses, may of itself explain, as a mere case of hereditary recurrence, the appearance of this physiological character in numerous and remote members of the family. The author concludes his paper with an appeal to men of Science to turn their attention to a region which is at once so little known and so rich in materials of interest for physiologists, ethnologists, and geographers.—Herr von Martens, in a critique on Prof. Strobel's paper on the appearance of *Unio* shells in the pile-dwellings of Upper Italy and in the Paraderos Patagonians, draws attention to the diversity of opinion to which the occurrence of this bivalve has given rise, Dr. Boni deducing from it the theory that the Emilian Terremare are the sites of human habitations on artificially constructed water basins, whilst Dr. Coppi regards them as the remains of sacrificial or other slaughter places. Dr. von Martens has ascertained by personal observation that the Paraderos of Patagonia resemble in very many respects the Danish Kjökkenmoddings. It is worthy of note in reference to this subject that shells of the Adriatic form (*Aporrhais pes pelicani* and *Venus verrucosa*) occur in the Moravian pile-dwellings near Olmutz, while Mediterranean shells (*Cypræa pyrum* and *lurida*) have been found on the Dordogne. These facts, which afford incontrovertible evidence of the extension of commerce in prehistoric ages, are corroborated by the appearance of Red Sea if not Indian Ocean forms of shells, as *Eburna spirata* in a Mariera at Reggio, and of *Cypræa pantherina*, in the Allemannic tumuli of Wurtemberg. It has been suggested by Dr. E. Friedel that the *Unio pictorum* L., and the *Alasmodonta compressa*, which are so abundant in Italian Lacustrine deposits, may be connected with the presence of domestic swine, as these bivalves constitute in the present day a very important element in the food of these animals in the poorer districts of the Oder and the Brandenburg Mark.—In conclusion we would draw attention to a curious paper read by Herr von Meyer before the Anthropological Society of Berlin on the origin of "Right and Left," and the causes which have led mankind to give the preference to one over the other, in using the hands and feet. The superior estimation of right over left is shown alike in the most ancient forms of Egyptian sculpture, in Jewish ordinances, in Hellenic poetry, and in language generally, whether of Turanian, Scythic, or Aryan origin. In these tongues the right hand is synonymous with what is good, straight, and right, while the left is identical with what is awkward, evil and abnormal. The author attempted to explain the universally diffused preference for the right hand on the ground of instinctive religious veneration in primæval man, who raised the right hand in adoration as he traced the course of the sun from its rising to its setting, while Prof. Virchow was inclined to refer it to a primary physical principle of the human organisation. The subject gave rise to an animated discussion in the Society, and led to the consideration of several questions of interest to the student of ethnology.

Sitzungsberichte der naturwissenschaftlichen Gesellschaft Isis in Dresden. Oct.—Dec. 1872.—The principal paper in this number is one by M. Ackermann, giving a comprehensive account of recent deep-sea researches.—Dr. Hoffmann furnishes a critique of Zollner's work on comets; and among the shorter notices will be found information on Phylloxera, the physical features, climate, and products of Venezuela, silkworm-cultivation, the Zoological Garden at Dresden, and other topics.—The succeeding number (Jan.—Mar. 1873) consists, in great part, of zoological lists.—M. Rostock enumerating the Neuroptera of Saxony, and Dr. Köhler the Gasteropoda and Conchifera of Schneeberg.—In the botanical section, M. Wilhelmi gives a list of plants found on the Murray river in Australia.—M. von Kiesenwetter communicates a paper on the history of zoology to the time of Linnæus, being chiefly an abstract of Carus's work on the subject in a voluminous "History of the Science in Germany," now in course of publication.

THE *American Journal of Science and Arts*, Sept. 1873.—In a fifth paper on some results of the earth's contraction from cooling, Prof. Dana treats of the formation of continental plateaux and oceanic depressions, thus concluding the reconsideration of the views he brought out in 1847. Besides the admission of a solid nucleus and the present partial union of the crust to the nucleus, these views have been modified in some points connected with mountain-making and metamorphism, in accordance with ideas developed by Le Conte and Mallet, and the results of personal study. The author gives a valuable summary of his progress.—Prof. O. Rood has a paper on the residual or secondary spectra which Brewster studied, and which are obtained when white light is passed through two prisms of different substances, so arranged as to compensate each other for colour. The Professor has obtained a large dispersion in such spectra by using as one of the constituents the spectrum furnished by oil of cassia, bisulphide of carbon, or flint glass, the other being the *normal spectrum* from a diffraction grating. Some curious experiments with these are described.—A paper on the explorations last year, by the Snake River Division of the U.S. Geological Survey of the Territories, is furnished by Prof. Bradley; and another geological paper, by Mr. Washburn, treats of the Bosphorus region. There are also notes on the Corundum of North Carolina, Georgia, and Montana; on minerals found at the Tidley Foster Iron Mines, New York; on an apparatus for rapid filtrations; and on the discovery of a new double star β Delphini.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Sept. 1.—M. Bertrand in the chair.—The following papers were read:—On the Aurora Borealis, by M. Faye. The author's paper related to Donati's late memoir on the same subject, in which he suggests that the passage of electro-magnetic currents from the sun to the planets is the cause of this phenomenon. M. Faye, on the other hand, deprecated the introduction of such a theory, and suggested that the effect of gravity as an agent in producing these effects may at least be probable. He suggested that motions such as are observed in the tails of comets might occur in the upper regions of our atmosphere, *i.e.* that excessively attenuated air might be constantly rushing from the side of the earth turned towards the sun to that turned from it, and that this motion might cause incandescence of the air, visible at the poles as auroræ.—On the Carpellary Theory as regards the *Amygdalaceæ*, by M. A. Trécul.—Gnomonic projection, &c., of a portion of the Sahara, by M. A. Pomel.—Study of the metallic veins of Cornwall; structure of the rich veins, and their relation to the stratigraphical arrangement of the country, by M. Moissenet.—On the Siemens coil, by M. A. Pellerin.—Observations of Planet 133 and of Borrelly's comet, by M. Stephan.—On the changes of form of Comet IV., 1873, and on its spectrum, by MM. G. Rayet and André. The comet has developed a tail and become brighter; it has no nucleus. Its spectrum at first consisted of three bands, one between D and E, another very close to *b*, and a third beyond F. After the tail had developed the same bands appeared, but they were larger and brighter and accompanied by a faint continuous spectrum.—On the form of the Martial seas as compared with the terrestrial oceans, by M. Stan. Meunier. The author considers that the long narrow straits on Mars are an additional proof of its greater age as compared with the earth. Taking the soundings of the Atlantic, he observed that if its level were reduced 4,000 metres (by absorption), it would then present a similar aspect to the Martial seas.

BOOKS RECEIVED

ENGLISH.—The Sea and its Wonders: Hartwig (Longmans & Co.).—Centrifugal Force and Gravitation: John Harris (Trubner & Co.).—Quantitative Chemical Analysis: Thorpe (Longmans & Co.)—What a House should be: William Bardwell (Dean).—The Convolutions of the Human Brain: Ecker (Smith, Elder & Co.)—Scripture Manual (Murby).—Mechanics: Skertchley (Murby).—Report of Freshwater Fish and Fisheries of India and Burmah: Surg.-Maj. Francis Day, Government of Calcutta.

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NATURE

THURSDAY, SEPTEMBER 18, 1873

SCIENTIFIC WORTHIES

I.—FARADAY

Michael Faraday, born September 22, 1791, died August 25, 1867.

WITH this number of NATURE we present to our subscribers the first of what we hope will be a long series of Portraits of Eminent Men of Science.

This first portrait is one of Faraday, engraved on steel, by Jeens, from a photograph by Watkins. Those who had the happiness of knowing Faraday best will best appreciate the artist's skill—he has indeed surpassed himself, for the engraving is more life-like than the photograph. We could ill spare such a memorial of such a man, one in which all the beautiful simplicity of his life beams upon us. There is no posturing here!

There is no need that we should accompany the portrait with a memoir of Faraday. Bence Jones, Tyndall, and Gladstone have already lovingly told the story of the grand and simple life which has shed and will long continue to shed such lustre on English Science, and their books have carried the story home to millions; nor is there any need that we should state why we have chosen to commence our series with Faraday; everybody will acknowledge the justice of our choice.

But there is great need just now that some of the lessons to be learnt from Faraday's life should be insisted upon, and we regard it as a fortunate circumstance that we have thus the opportunity of insisting upon them while our Scientific Congress is in session, and before the echoes of the Address of the President of the British Association for the Advancement of Science have died away.

In the first place, then, we regard Faraday at once as the most useful and the most noble type of a scientific man. The nation is bigger and stronger in that Faraday has lived, and the nation would be bigger and stronger still were there more Faradays among us now. Prof. Williamson, in his admirable address, acknowledges that the present time is "momentous." In truth the question of the present condition of Science and the ways of improving it, is occupying men's minds more than it has ever done before; and it is now conceded on all sides that this is a national question, and not only so, but one of fundamental importance. Now what is the present condition of English Science? It is simply this, that while the numbers of our professors and their emoluments are increasing, while the number of students is increasing, while practical instruction is being introduced and text-books multiplied, while the number and calibre of popular lecturers and popular writers in Science is increasing, original research, the fountain-head of a nation's wealth, is decreasing.

Now a scientific man is useful as such to a nation according to the amount of new knowledge with which he endows that nation. This is the test which the nation, as a whole, applies, and Faraday's national reputation rests on it. Let the nation know then that the real difficulty at present is this; we want more Faradays; in other words more men working at new knowledge.

It is refreshing to see this want so clearly stated in the Presidential Address:

"The first thing wanted for the work of advancing science is a supply of well-qualified workers. The second thing is to place and keep them under the conditions most favourable to their efficient activity. The most suitable men must be found while still young, and trained to the work. Now I know only one really effectual way of finding the youths who are best endowed by nature for the purpose; and that is to systematise and develop the natural conditions which accidentally concur in particular cases, and enable youths to rise from the crowd.

"Investigators, once found, ought to be placed in the circumstances most favourable to their efficient activity.

"The first and most fundamental condition for this is, that their desire for the acquisition of knowledge be kept alive and fostered. They must not merely retain the hold which they have acquired on the general body of their science; they ought to strengthen and extend that hold, by acquiring a more complete and accurate knowledge of its doctrines and methods; in a word, they ought to be more thorough students than during their state of preliminary training.

"They must be able to live by their work, without diverting any of their energies to other pursuits; and they must feel security against want, in the event of illness or in their old age.

"They must be supplied with intelligent and trained assistants to aid in the conduct of their researches, and whatever buildings, apparatus, and materials may be required for conducting those researches effectively.

"The desired system must therefore provide arrangements favourable to the maintenance and development of the true student-spirit in investigators, while providing them with permanent means of subsistence, sufficient to enable them to feel secure and tranquil in working at science alone, yet not sufficient to neutralise their motives for exertion; and at the same time it must give them all external aids, in proportion to their wants and powers of making good use of them."

Whether the scheme proposed by Dr. Williamson to bring such a state of things about will have the full success he anticipates is a matter of second-rate importance; what is of importance is, that the need of some scheme is now fully recognised.

So far the remarks we have made have been suggested by Faraday's usefulness. It is to be hoped that the nobleness of his simple, undramatic life, will live as long in men's memories as the discoveries which have immortalised his name. Here was no hunger after popular applause, no jealousy of other men's work, no swerving from the well-loved, self-imposed task of "working, finishing, publishing."

"The simplicity of his heart, his candour, his ardent love of the truth, his fellow-interest in all the successes, and ingenuous admiration of all the discoveries of others, his natural modesty in regard to what he himself discovered, his noble soul—independent and bold—all these combined, gave an incomparable charm to the features of the illustrious physicist."

Such was his portrait as sketched by Dumas, a man cast in the same mould. All will recognise its truth. Can men of science find a nobler exemplar on which to fashion their own life? Nay, if it were more widely followed than it is, should we not hear less of men falling away from the "brilliant promise" of their youth, tempted by "fees," or the "applications of Science," or the advantages attendant upon a popular exposition of other men's work? Should we not hear a little less frequently than we do that research is a sham, and that all attempts to aid it savour of jobbery?

Lastly we may consider Faraday's place in the general history of Science; this is far from easy. Our minds are still too much occupied with the memory of the outward form and expression of his scientific work to be able to compare him aright with the other great men among whom we shall have to place him.

Every great man of the first rank is unique. Each has his own office and his own place in the historic procession of the sages. That office did not exist even in the imagination, till he came to fill it, and none can succeed to his place when he has passed away. Others may gain distinction by adapting the exposition of science to the varying language of each generation of students, but their true function is not so much didactic as pædagogic—not to teach the use of phrases which enable us to persuade ourselves that we understand a science, but to bring the student into living contact with the two main sources of mental growth, the fathers of the sciences, for whose personal influence over the opening mind there is no substitute, and the material things to which their labours first gave a meaning.

Faraday is, and must always remain, the father of that enlarged science of electro-magnetism which takes in at one view, all the phenomena which former inquirers had studied separately, besides those which Faraday himself discovered by following the guidance of those convictions, which he had already obtained, of the unity of the whole science.

Before him came the discovery of most of the fundamental phenomena, the electric and magnetic attractions and repulsions, the electric current and its effects. Then came Cavendish, Coulomb, and Poisson, who by following the path pointed out by Newton, and making the forces which act between bodies the principal object of their study, founded the mathematical theories of electric and magnetic forces. Then Örsted discovered the cardinal fact of electro-magnetic force, and Ampère investigated the mathematical laws of the mechanical action between electric currents.

Thus the field of electro-magnetic Science was already very large when Faraday first entered upon his public career. It was so large that to take in at one view all its departments required a stretch of thought for which a special preparation was necessary. Accordingly, we find Faraday endeavouring in the first place to obtain, from each of the known sources of electric action, all the phenomena which any one of them was able to exhibit. Having thus established the unity of nature of all electric manifestations, his next aim was to form a conception of electrification, or electric action, which would embrace them all. For this purpose it was necessary that he should begin by getting rid of those parasitical ideas, which are so apt to cling to every scientific term, and to invest it with a luxuriant crop of connotative meanings flourishing at the expense of the meaning which the word was intended to denote. He therefore endeavoured to strip all such terms as "electric fluid," "current," and "attraction" of every meaning except that which is warranted by the phenomena themselves, and to invent new terms, such as "electrolysis," "electrode," "dielectric," which suggest no other meaning than that assigned to them by their definitions.

He thus undertook no less a task than the investigation of the facts, the ideas, and the scientific terms of electro-magnetism, and the result was the remodelling of the whole according to an entirely new method.

That old and popular phrase, "electric fluid," which is now, we trust, banished for ever into the region of newspaper paragraphs, had done what it could to keep men's minds fixed upon those particular parts of bodies where the "fluid" was supposed to exist.

Faraday, on the other hand, by inventing the word "dielectric," has encouraged us to examine all that is going on in the air or other medium between the electrified bodies.

It is needless to multiply instances of this kind. The terms, field of force, lines of force, induction, &c., are sufficient to recall them. They all illustrate the general principles of the growth of science, in the particular form of which Faraday is the exponent.

We have, first, the careful observation of selected phenomena, then the examination of the received ideas, and the formation, when necessary, of new ideas; and, lastly, the invention of scientific terms adapted for the discussion of the phenomena in the light of the new ideas.

The high place which we assign to Faraday in electromagnetic science may appear to some inconsistent with the fact that electromagnetic science is an exact science, and that in some of its branches it had already assumed a mathematical form before the time of Faraday, whereas Faraday was not a professed mathematician, and in his writings we find none of those integrations of differential equations which are supposed to be of the very essence of an exact science. Open Poisson and Ampère, who went before him, or Weber and Neumann, who came after him, and you will find their pages full of symbols, not one of which Faraday would have understood. It is admitted that Faraday made some great discoveries, but if we put these aside, how can we rank his scientific method so high without disparaging the mathematics of these eminent men?

It is true that no one can essentially cultivate any exact science without understanding the mathematics of that science. But we are not to suppose that the calculations and equations which mathematicians find so useful constitute the whole of mathematics. The calculus is but a part of mathematics.

The geometry of position is an example of a mathematical science established without the aid of a single calculation. Now Faraday's lines of force occupy the same position in electromagnetic science that pencils of lines do in the geometry of position. They furnish a method of building up an exact mental image of the thing we are reasoning about. The way in which Faraday made use of his idea of lines of force in co-ordinating the phenomena of magneto-electric induction[*] shows him to have been in reality a mathematician of a very high order

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[*] To estimate the *intensity* of Faraday's scientific power, we cannot do better than read the first and second series of his "Researches," and compare them, first, with the statements in Bence Jones's "Life of Faraday," which tells us the tales of the first discovery of the facts, and of the final publication of the results, and second, with the whole course of electromagnetic science since, which has added no new idea to those set forth, but has only verified the truth and scientific value of every one of them.

—one from whom the mathematicians of the future may derive valuable and fertile methods.

For the advance of the exact sciences depends upon the discovery and development of appropriate and exact ideas, by means of which we may form a mental representation of the facts, sufficiently general, on the one hand, to stand for any particular case, and sufficiently exact, on the other, to warrant the deductions we may draw from them by the application of mathematical reasoning.

From the straight line of Euclid to the lines of force of Faraday this has been the character of the ideas by which science has been advanced, and by the free use of dynamical as well as geometrical ideas we may hope for a further advance. The use of mathematical calculations is to compare the results of the application of these ideas with our measurements of the quantities concerned in our experiments. Electrical science is now in the stage in which such measurements and calculations are of the greatest importance.

We are probably ignorant even of the name of the science which will be developed out of the materials we are now collecting, when the great philosopher next after Faraday makes his appearance.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Tyndall and Tait

I HAVE hitherto refrained from intruding upon your space with reference to this deplorable Forbes' controversy, but now that the occasion has come when a brief deliverance on my part seems called for, I trust to your courtesy, if not to your justice, to allow me room for it.

In the first place I would ask permission to inform such of your readers as may feel an interest in the subject, that if they wish to form a correct opinion of the tone and logic of my rejoinder to Principal Forbes and his biographers, they will consult the rejoinder itself, as published by Longmans, and not the extracts and inferences of Professor Tait.

They will thus learn, among other things, that what Professor Tait calls "plausible," is simply unanswerable.

With regard to the taking up of the various points in Principal Forbes's reply, item by item, that may be done some day should I deem it a worthy occupation. In my rejoinder I converged attention on the two points which Principal Forbes himself considered the really serious ones, and having broken the neck of the argument in both these cases I cared little about prolonging the controversy. Nevertheless if circumstances show it to be necessary it may be prolonged.

Professor Tait invariably writes on the hypothesis that what is not contradicted cannot be contradicted, and must therefore be accepted as true—a natural, if not inevitable, assumption on his part. For example, Forbes's argument regarding the crevasses of Rendu was left unanswered by me, hence the conclusion that it was unanswerable. That argument, however, is now in shreds, as it might have been, had I so willed, any time during the last dozen years. Again, Principal Forbes makes an assertion regarding his tutelage of Agassiz; the assertion is left uncontradicted; it must therefore be accepted as true, and I am unjust because I do not so accept it. Thirteen years ago, however, I was in possession of a diametrically opposite assertion from M. Agassiz. Quite as distinctly, though not so specifically, he writes thus within the present year. "When Forbes came to visit me upon the glacier of the Aar, he knew not only everything that I had done, but also my plans for the future. When he left he positively declined to express any opinion concerning glacier phenomena, under the plea that he only came to gratify his curiosity, and had no intention of following up the subject, as he had no desire to be involved in the controversy then raging regarding the former extension of glaciers.* When he showed his hand I did not enter into a protracted discussion, but simply made a statement of facts and let the matter rest. . . . . When I look," adds M. Agassiz, "on the whole transaction it seems incredible. There is in it no vestige either of the gentleman or the honest investigator."

With statements of this character confronting the assertions of Principal Forbes, the proper course for me was to ignore assertions on both sides, and to confine myself to demonstrable facts. This I accordingly did.

With regard to Mr. Tait's criticism of my "popular" writings it has, of course, nothing to do with his defence of Forbes, but is the product of mere ignoble spite. He asks me to reply to him not according to the letter, but according to the spirit of his attack. If I might use the expression I would say, "God forbid!" for how could I do so without lowering myself to some extent to his level. The antecedents of Mr. Tait with reference to me are pretty well known. When I sought to raise from the dust a meritorious man whose name is now a household word in science, who has been elected by acclamation a member of the French Academy, and who has received the crowning honour of the Royal Society—when I sought to place Dr. Mayer in the position which he now holds, and from which no detraction can remove him, it was Mr. Tait who, in *Good Words*, charged me with misleading the public; who followed up his attack in the "Philosophical Magazine," and who when publicly hoisted by his own petard, retired to void his venom against me in the anonymous pages of the "North British Review." It is this man whose blunders and whose injustice have been so often reduced to nakedness, without ever once showing that he possessed the manhood to acknowledge a committed wrong, who now puts himself forward as the corrector of my errors and the definer of my scientific position. That position is happily not dependent upon him, and his opinion regarding it, is to me, as it will be to most others, a trifle light as air. But graver considerations than mere personal ones here arise. Might I venture, Mr. Editor, to express a doubt as to the wisdom of permitting discussions of this kind to appear in your invaluable journal. Having opened your columns to attack you are, of course, in duty bound to open them to reply, but if I might venture a suggestion, you would wisely use your undoubted editorial rights, and consult the interests of science, by putting a stop to proceedings which dishonour it. An illustrious person writes to me thus:—"I have just read Professor Tait's letters in NATURE, and feel a recurrence of that pain which similar communications once inflicted on myself—pain felt, not on my own account; for I knew that the attacks would no more sully me in the opinion of those whom I loved and respected, than they did in my own opinion; but pain for the wounded honour of science and the outraged dignity of scientific controversy."

JOHN TYNDALL

Athenæum Club, Sept. 16

[We deeply sympathise with Professor Tyndall's remarks on the injury done to scientific controversy by the introduction into it of personalities, and we should have made his own letter square with his canon if his reference to our duty in this matter, and his insinuation of injustice did not take the matter out of our hands. Prof. Tyndall forgets (1) that Prof. Tait's letter is an answer to a pamphlet by Dr. Tyndall, and that space was asked for it as such; and not an *attack* in the sense in which Prof. Tyndall uses the word; (2), that if the Editor were to assume the power and responsibility that Prof. Tyndall suggests, NATURE might easily fall from the position of absolute justice and impartiality in all scientific matters which it now occupies and become the mere mouthpiece of a clique.

What the Editor can do and has endeavoured to do in this case, is to guard the reputations of men of Science against the attacks of men of straw, and to see that no personalities are used; and it is under strong protest that he allows to pass in Prof. Tyndall's letter, for the reasons already stated, personalities, the equivalents of which, the Editor, in the exercise of his "undoubted editorial rights," struck out of Prof. Tait's communication.—ED. NATURE.]

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* This tallies with Forbes's own account (Travels, page 38). "Far from being ready to admit, as my sanguine companions wished me to do in 1841, that the theory of glaciers was complete, and the cause of their motion certain, after patiently hearing all they had to say, and reserving my opinion, &c." This reservation of opinion is probably the reticence referred to by Agassiz.

NOTES FROM THE "CHALLENGER"

VII.

ON Monday the 30th of June we sounded in 1,000 fathoms, about 114 miles westward from Fayal. The dredge was put over early in the forenoon, and came up half filled with a grey sandy ooze with a large proportion of the dead shells of Pteropods, many Foraminifera, and many pebbles of pumice. Many animal forms of great interest were found entangled in the swabs, or sifted out of the mud. Another Schizopod crustacean of large size and great beauty of form and brilliancy of colouring came up in this haul. Dr. von Willemoes-Suhm regards it as congeneric with the species taken at Station 69, at a depth of 2,200 fathoms, and as these crustaceans are among our most interesting acquisitions during the voyage between Bermudas and the Açores, I will abstract a brief description of them from his notes.

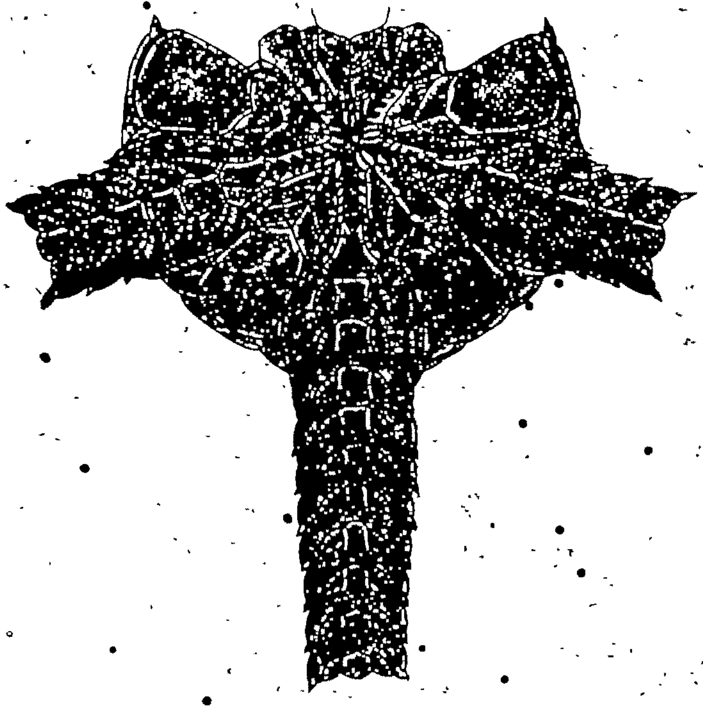

FIG. 1.—Ophioglypha bullata, Wy. Thomson—six times the natural size.

The two crustaceans for whose reception Dr. von Willemoes-Suhm proposes to establish the genus *Gnathophansia* present characters which have hitherto been found partly in Schizopods and partly in Phyllopods, but not combined in the same animal. They are, however, essentially Schizopods, and have much in common with *Lophogaster*, a genus described in great detail by the late Prof. Sars. It is proposed to refer *Gnathophansia* to the family Lophogastridæ, which must be somewhat modified and expanded for its reception.

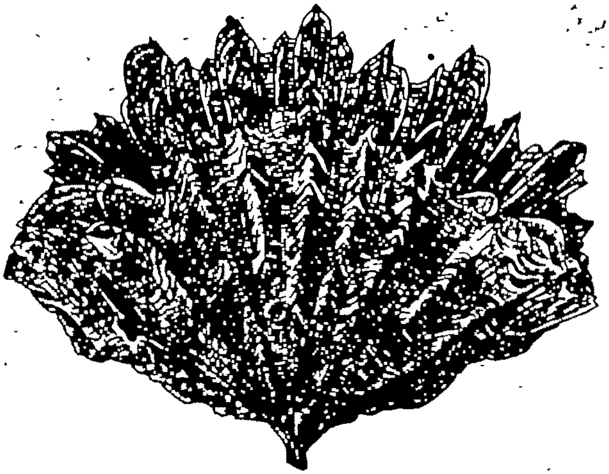

FIG. 2

FIG. 2.—Flabellum alabastrum, H. N. M.

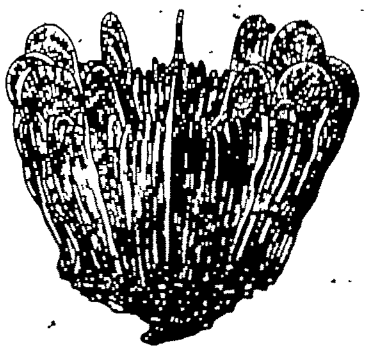

FIG. 3

FIG. 3.—Ceratotrochus nobilis, H. N. M.

In *Gnathophansia* the dorsal shield covers the thoracic segments of the body, but it is unconnected with the last five of these. The shield is prolonged anteriorly into a spiny rostrum. The stalked eyes are fairly developed in the ordinary position. There is an auxiliary eye on each of the maxillæ of the second pair.

The two species of the genus are thus distinguished: *G. gigas*, n. sp. (Figs. 4 and 5). Scale of the outer antenna with five teeth; dorsal shield with the outer angles of its posterior border produced into spines; no posterior spine in the middle line; length 142 mm. Of this species one specimen was taken from a depth of 2,200 fathoms, with a bottom of Globigerina ooze, at Station 69, 400 miles to the west of the Açores.

G. zoea, n. sp. (Fig. 6): Scale of the outer antenna with one tooth. A long central spine on the posterior border of the dorsal shield, but no lateral spines; length, 60 mm. A single specimen at the present station likewise from a bottom of Globigerina ooze.

On comparing the figures of these two species and of their anatomical details with that of *Lophogaster* given by Sars, one is struck by their great general similarity; but there are characters presented by the new genus, particularly in connection with the dorsal shield, which not only entirely separate it from Lophogaster, but enlarge our views on the whole Schizopod group. In both species the shield is sculptured by ridges traversing it in different directions, and in both there is a long spiny rostrum; but this shield is merely a soft duplicature of the skin, connected with the body only anteriorly, and leaving five thoracic segments entirely free. In the structure of the shield and its mode of attachment *Gnathophausia* has the greatest resemblance to *Apus* among all crustaceans, but it differs from it widely in all other respects. *Nebalia* is the only Schizopod in which the carapace is not connected with the posterior thoracic segments, but in that genus the form of the carapace is totally different, and the genera are otherwise in no way nearly related.

Neither the antennæ, nor the scales, nor the parts of the mouth present any marked differences from those of

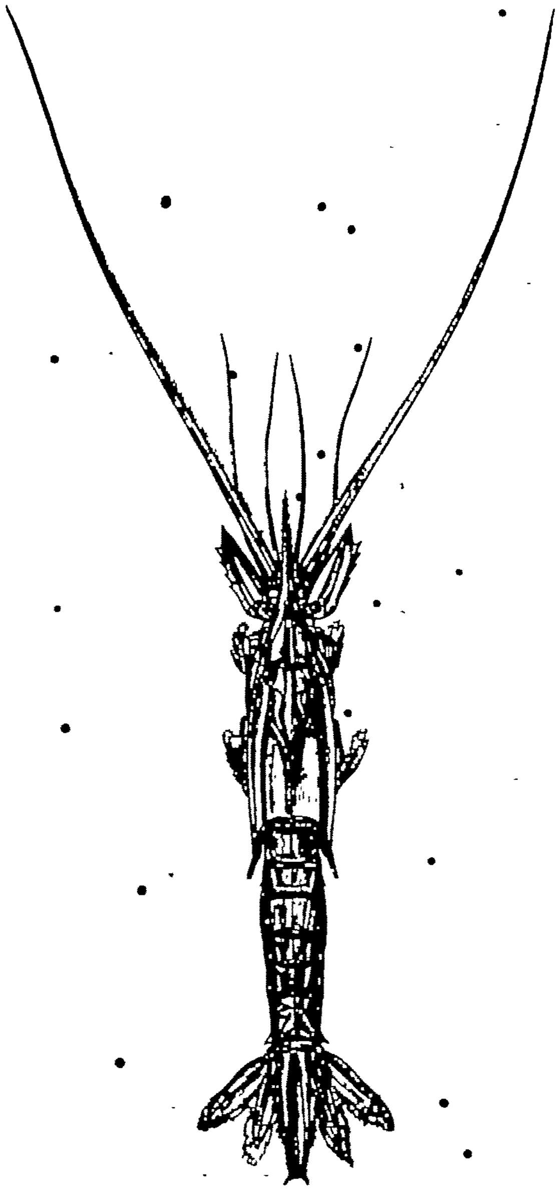

FIG. 4

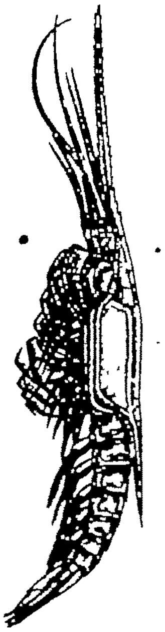

FIG. 6

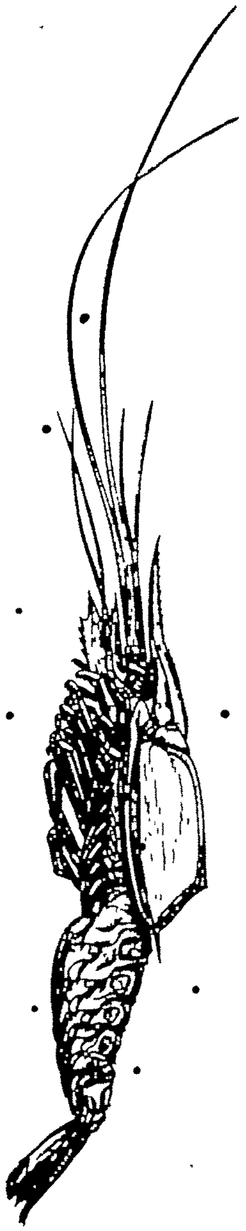

FIG. 5

FIGS. 4 & 5.—Gnathophausia gigas, v. W.-S.

FIG. 6.—Gnathophausia zoea, v. W.-S

Lophogaster, with the exception of the second maxillæ. These, with nearly the same form as in the Norwegian genus, bear a pair of accessory eyes. Such eyes are well known at the base of the thoracic and even of the abdominal limbs in the Euphausidæ, a family with which the Lophogastridæ have otherwise nothing in common, but hitherto they have not been met with in any other animal or in any of the manducatory organs.

Of the eight pairs of legs seven are ambulatory, only the first pair is, as in *Lophogaster*, transformed into maxillipeds. The gills are arborescent and attached to the bases of the legs. The abdomen and its appendages scarcely differ from those of *Lophogaster*. We find here also that the last segment is apparently divided into two. This would indicate an approach to such forms as *Nebalia*, which has nine abdominal segments, or at all events a tendency to a multiplication of segments which if really existing would scarcely allow the association of the genus with the true Schizopoda.

The weather was remarkably fine. During the day the island of Flores was visible like a cloud on the horizon, about 50 miles to the northward. In the afternoon we obtained a series of temperature soundings at intervals of 100 fathoms down to 1,000, and in the evening proceeded under steam towards Fayal.

On the following day, the 1st of July, we sounded in 1,350 fathoms, about 20 miles west of Fayal, apparently in a depression which separates the western group of the Açores, Flores and Corvo from the central group Fayal, Pico, San Jorge, Terceira, and Gracioza, and during the afternoon we gradually approached the fine island of Fayal, and enjoyed the development of its bold outlines and rich and varied colouring. In the evening we passed into the narrow channel between Fayal and Pico, and anchored in the roadsteads of Hôrtes. We found to our great disappointment that small-pox was prevalent in Fayal, and as Captain Nares considered it imprudent to give general leave, one or two of us only landed to pick up what general impression we might of the appearance of the place, and on the following morning we proceeded towards San Miguel, first taking a few hauls of the dredge in shallow water between Fayal and Pico, where we found a rather scanty fauna, resembling in character that of southern Europe, on a bottom of dark volcanic sand.

On Friday, July 4, we sounded in 750 fathoms on a rocky bottom. The ship water-bottle was sent down and brought up a sample of the bottom water. In the afternoon we shortened and furled sails, and proceeded under steam towards San Miguel, and in the evening stopped abreast of Ponta Delgada, the capital of the island, where we lay-to for the night, secured to a buoy. Next morning, as we found, greatly to our satisfaction, that the town was considered free from any epidemic of small-pox, we steamed in to the anchorage, and cast anchor in 13 fathoms.

We remained at San Miguel until Wednesday the 9th. We were well aware that the time at our disposal was quite insufficient to enable us to do anything of importance to add to the knowledge of the natural history of the island already so well worked out, and as we had had a long sea-cruise, we were in no way disinclined for a few days of complete relaxation. We accordingly combined into a large party, totally unscientific in its object, and by the aid of mules and donkeys made a most enjoyable raid among the caldeiras and volcanic ranges of the east end of the island. The random impressions collected during these *horæ subsecivæ* may perhaps be chronicled elsewhere.

Our first haul after leaving Ponta Delgada, was in 1,000 fathoms, mid-way between the islands of San Miguel and Santa Maria, and about fifteen miles north-west of the Formigas. The bottom was Globigerina ooze. The principal feature in this dredging was the unusual abundance of stony corals of the deep-sea group.

Two living specimens of a large species of *Flabellum* were sifted out, the same as the one which we had dredged previously at station 73, to the west of Fayal. The corallum is wedge-shaped, the calicle rising from an attenuated pedicle. The extreme height, from the end of the pedicle to the margin of the cup, is 50 mm.; the greatest diameter of this calicle is 65 mm., and the smallest 30 mm. The three species are very nearly of the same dimensions.

The lateral costæ make an angle with one another of 120° to 140°, and are sharp and moderately prominent, with an irregular edge. The external surface of the calicle is covered with a glistering epitheca, and near the margin is of a light pink colour. The costæ of the faces corresponding to the primary and secondary septa are almost as well marked as the lateral costæ, and appear as irregularly dental ridges, separated by slight depressions. The ends of the calicle are broadly rounded, and it is compressed laterally in the centre. The upper margin is curved, describing about one-third of a circle.

There are six systems of septa disposed in five cycles. The septa are extremely thin and fragile. They are tinged with pink, and covered with rounded granules, disposed in rows. The primary septa are approximately equal to the secondary, giving somewhat the appearance of twelve systems. These septa are broad and prominent, with a rounded superior margin, and curved lines of growth. The septa of the third, fourth, and fifth cycles successively, diminish in breadth, and are thus very markedly distinguished from one another, and from the primary and secondary septa. The septa of the fourth cycle join those of the third a short distance before reaching the columella. The septa of the fifth cycles are incomplete. The margin of the calicle is very deeply indented, the costal corresponding to the primary and secondary septa being prolonged in conjunction with the outer margins of these septa, into prominent pointed processes; similar but shorter prolongations accompany the tertiary, and some of the quaternary septa. Between each of the sharp projections thus formed, the edge of the wall of the calicle presents a curved indentation.

Two of the specimens procured, expanded their soft parts when placed in sea-water. The inner margin of the disc round the elongated oral aperture, presents a regular series of dentations, corresponding with the septa, and is of a dark madder colour; the remainder of the disc is pale pink. The tentacles take origin directly from the septa. They are elongated and conical. Those of the primary and secondary septa are equal in dimensions, and along with the tertiary tentacles, which are somewhat shorter, but in the same line, are placed nearest the mouth, and at an equal distance from it. The tentacles of the fourth and fifth cycles are successively smaller and at successively greater distances from the mouth. Placed on either side of each tentacle of the fifth cycle, and again somewhat nearer the edge of the calicle, there are a pair of very small tentacles which have no septa developed in correspondence with them. There are thus four successive rows of tentacles, and the normal number is ninety-six. The tentacles are of a light red colour, and between their bases are stripes of yellowish red and light grey.

This group belongs to the group *Flabella sub-pedicellata* of Milne-Edwards, and probably to that division in which the costæ are prominent and ridge like on the faces of the corallum, as well as on its lateral margins, but it differs from those described under this head by Milne-Edwards, in that it has five cycles, the fifth being incomplete, and in other particulars which appear from the description given.

A single living specimen of a coral referred, by Mr. Moseley to the genus *Ceratotrochus* was obtained from this haul. The corallum is white. The base sub-pedicellate with a small scar of original adherence. The principal costals are prominent, and round the region of the base beset with small spines directed somewhat upwards. The upper portion of the costa is without spines. The primary and secondary septa are broad and exsert. Pali are absent, the columella is fascicular. The absence of pali, the form of the columella, and the nature of the base, associate this form with the *Centrotrochi*, as defined by Milne-Edwards.

The animal is of a dark madder colour on the region of the margin of the calicle between the exsert primary and secondary septa, and on the membrane investing the wall of the corallum from the margin down to the commencement of the spines. This dark colour is succeeded on the disc by a band of pale bluish, within which there is again a zone of very dark madder colour round the mouth. The dark colouring-matter is interesting, as it gives an absorption spectrum of three distinct bands.

On Friday, July 11, we sounded in 2,025 fathoms, 376 miles to the west of Madeira, the bottom very well marked "globigerina ooze," and the bottom temperature 1° 5′ C.

The weather for the last few days had been remarkably fine, with a pleasant light breeze. When we turned up on deck on the morning of the 16th, we were already at anchor in the beautiful bay of Funchal, and, looking at the lovely garden-like island, full of anticipations of a week's ramble among the peaks and "currals" and the summer "quintas" of our friends—anticipations which were doomed to be disappointed.

WYVILLE THOMSON

THE INTERNATIONAL METRIC COMMISSION AT PARIS

IN continuation of the notices of the proceedings of this Scientific Commission (see NATURE, vol. vii. p. 237), it may now be stated that the French Section have been engaged during the present year in the work of the Commission entrusted to them, and have continued their sittings up to the present time. It appears from the printed "Procès Verbaux" that their attention has been principally directed to the further investigations and experiments required for the melting and casting of the large mass of alloy of platinum and iridium, determined upon as the material of all the new standards, with the view of obtaining a homogeneous ingot of these two metals in the proper proportions. This preliminary work is now so far completed that the twelve members of the Commission elected as the Permanent Committee, have been summoned to meet at Paris on October 1, to consult upon the subject with the French Section, and more particularly to discuss and decide the following points :—

1. The date of the definitive of the melting platinum-iridium intended for the construction of the new International metric standards.

2. The question whether the *Mètres-à-bouts* requested by some countries shall be constructed from the metal of the same melting as the *Mètres-à-traits*.

3. Whether the kilograms shall be made from the metal of the same melting as the *Mètres-à-traits*.

As to the number of metric standards required to be constructed by the Commission, the greater number of the Governments represented at the Commission have already intimated their wishes to have in all 31 metres and 24 kilograms. Germany and Italy have not yet notified their decision. Austria and Switzerland have declined to reply until the question of the creation of an International Bureau is satisfactorily settled, and it is understood that the same course is being followed by Germany. Russia is favourable to the creation of the Bureau, but has not yet decided on the number of standards she will require.

In addition to the number of fifty delegates already appointed by twenty-nine Governments to take part in the International Metric Commission, and whose names have been already announced, the Haytian Government has nominated M. Ch. Laforestie, Chargé d'Affaires of the Haytian Republic, and the Government of Brazil has nominated Prof. Such de Capanema as their respective delegates of the Commission. The French Government has also invited the Governments of Central America, Persia, China, and Japan to send delegates to take part in the proceedings of the Commission.

As it will be expedient to construct a number of spare copies of the new metric standards, it will probably be necessary to prepare for the construction of not less than fifty metres and nearly as many kilograms.

But difficulties must inevitably and at once arise at Paris from the course taken by the Governments of Germany, Austria, and Switzerland, as it tends materially to impede the attainment of the declared primary objects of the Commission to construct and furnish every Government interested with uniform metric standards, which are to be accurately verified, and of equal authority. After the expiration of four years from the date of the appointment of the Commission by the French Government, on September 2, 1869, and the passing of almost unanimous resolutions at a full meeting of the Commission in 1872, upon the mode of constructing the new standards, the time has now arrived when everything has been got ready for commencing the actual construction of the new standards. It can hardly be expected that this, the real work of the Commission, is to be stopped until the ulterior question of the creation of an International Metric Bureau is settled to the satisfaction of the three above-mentioned Governments. Nor does a further significant step which has been recently taken by the Austrian Government lead to much hope of a satisfactory solution of this question.

The Austrian Government has officially declared that it accepts in principle the establishment of an International Metric Bureau upon the basis of the resolutions passed by the Commission, so far as relates to the objects and functions of this Bureau; and that it is quite disposed to take part in a Convention upon the subject, provided that all the other Governments represented at the Commission give their adherence. But it expressly reserves the right of making new propositions when the questions of the organisation, the seat, and the direction of the Bureau are discussed, as well as the right of definitively approving the Convention.

It proposes, at the same time, that in order to maintain the international character of the negociation, the seat of the Conference shall be at Berne, where the International Telegraphic Conference is now held, or at Brussels, these two cities being equally upon neutral territory.

And that for facilitating the proceedings of the Conference, the Permanent Committee appointed by the Metric Commission, shall previously elaborate a project of Convention to be communicated to the several governments interested; and that the Conference be not convoked for completing the definitive Convention until the preliminary negociations shall be sufficiently advanced to allow of a favourable result.

The invitation given by the French Government to the Austrian and other governments, was to take part in the creation of the International Metric Bureau based upon the five points proposed by the Commission, and it now appears that Austria objects to three out of these five points. And even as regards the other two points, Austria's adhesion is conditional upon the concurrence of all the other governments represented at the Commission. Up to the present time, however, the governments of five countries only have officially notified their concurrence, whilst those of twelve countries have formally declined to take any part in the establishment of the proposed International Metric Bureau. Under these circumstances, its creation at all seems very problematical, however desirable it may be in the interests of metrological science.

It is evident that the decision upon these new propositions must be left entirely to the governments interested. At any rate, the discussion of the Austrian propositions appear to be quite beyond the powers of either the French Section or the Permanent Committee, who are in no way authorised to re-open questions which, so far as the action of the Commission is concerned, have already been unanimously decided at the full meeting of the Commission. Meanwhile, the specific work of the Commission must be proceeded with, and the approaching meeting at Paris will enable the final decisions to be made, which alone are now required for beginning the construction of the new Standards.

H. W. CHISHOLM

NOTES

AN election will be held on Thursday, October 30, to two fellowships in connection with Merton College, Oxford. The examination for one of these fellowships will be in mathematics, for the other in physical science. The election to the physical science fellowship will be decided with respect to proficiency in physics, but candidates will have an opportunity of showing a knowledge of chemistry as supplementary to physics. The examination in both these subjects will be partly practical, partly by papers, and will be held in common with Magdalen College. A lectureship in physics, tenable for three years, in Trinity College, of 200*l*. per annum, will be offered to the Fellow to be elected. The examination for the two fellowships will commence on Tuesday, October 7, at 9 A.M., in the Merton College Hall. Candidates are required to call on the Warden on Tuesday, October 7, between 4 and 5 P.M.

THE Opening Address of this session of the St. Thomas Charterhouse Teachers' Science Classes will be delivered by Mr. F. C. Buckmaster on Saturday morning, the 20th inst., at 10.30. The chair will be taken by Sir J. Bennett, and a deputation from the Science Department of South Kensington will attend. Last year this undertaking met with signal success: above 200 teachers of primary schools availed themselves of the privileges offered by the institution. Many of the late students are now qualified to give instruction in elementary science. The movement is likely to do an immense amount of good in the way of making the teaching of elementary science common amongst the masses. During the recess about 250*l*. has been expended in fitting up a chemical laboratory and purchasing scientific apparatus; this, together with the engagement of an additional number of lecturers, it is thought will again secure a large number students.

WE understand that the bryological books and exceedingly rich and important collections and preparations of mosses left by the late Prof. Sullivant, whose death we recorded last week, are consigned to the Grey Herbarium of Harvard University, with a view to their preservation and long-continued usefulness. The remainder of his botanical library, his choice microscopes, and other collections are bequeathed to the State Scientific and Agricultural College just established at Columbus.

THE *American Naturalist* for August records the death of four contributors to that journal, all more or less known as working naturalists:—Prof. John Lewis Russell, of Salem, one of the founders, and for many years president of the Essex County (Massachusetts) Natural History Society, which afterwards became part of the Essex Institute, an active worker in botany; Mr. George Gibbs, of New Haven, the distinguished American ethnologist and philologist, whose special work had been in the language and history of the North American Indians; Col. John W. Foster, president of the Chicago Academy of Science, a constant contributor of papers and memoirs on geological and archæological subjects, and joint author with Prof. Whitney of the Government Report on the Mineral Lands of Lake Superior, published in 1850; and Prof. Henry James Clark, of Amherst, one of the most thorough histologists and best microscopists in the country, and a large contributor to Prof. Agassiz's volumes on the Natural History of the United States. Of these losses to science, Prof. Clark was under 50, and only Prof. Lewis over 60.

THE first meeting of the Agassiz Natural History Club, recently organised by the students of the Anderson School of Natural History on Penikese Island, was held on July 24, and showed signs of great energy and activity. Although the school had only been open a fortnight, lectures on surface geology, the embryology of vertebrates and articulates, on physiology, physical geography, on the microscope and its construction, with practical lessons on its use; free hand drawing on the blackboard, zoological and landscape drawing, and daily dredging excursions in the yacht *Sprite*, together with instructions in collecting and preserving animals, have been given. The amount of laboratory work done is stated to be most satisfactory. Large aquaria are being set up in the temporary laboratory.

THE Council of the Pharmaceutical Society are desirous of forming a complete herbarium of medical plants from every quarter of the globe, whether officinal or not. Mr. Holmes, the Curator of the Society's Museum, 17, Bloomsbury Square, will be glad to enter into communication with any foreign botanists and pharmaceutists willing to co-operate in the work.

IN a telegram from St. Petersburg, September 11, it is stated that General Kaufmann reports that the Amoo Daria river is not navigable by steamboats. The scientific expedition sent out by General Kaufmann to explore the old bed of the Amoo Daria river as far as the lake of Lara Kamish, returned on July 23 to the camp at Kunurgentsch. The expedition explored the river to a distance of 450 versts, and succeeded in collecting much valuable information and scientific materials.

IN a telegram from St. John's, Newfoundland, of September 11, it is stated that the *Juniata* had arrived there and reported that the camp of the crew of the *Polaris* was discovered by the *Tigress* on August 14 at Littleton Island, where the ship was deserted. Manuscript records of the expedition up to a period of six weeks before the discovery were secured. The *Tigress* is still in search of the Buddington party, who are believed to be safe.

A PAPER in Petermann's *Mittheilungen* upon the driftwood found in Nova Zembla has at present a special interest in connection with the discovery of fragments of a similar character by the crew of the *Polaris* in Polaris and Newman Bays. The Nova Zembla specimens consisted mainly of willow of various thicknesses. There were also, however, pieces of beech nearly a foot in diameter, several species of pine, among these *P. sylvestris*, an *Abies*, &c. It is thought that a large portion of this material must have been derived from the Petschora, Ob, and Yenesei rivers, and that none of it could have been derived from the current of the Gulf Stream.

THE past winter was very mild in the southern portion of Iceland, but quite severe in the northern. In the middle of January an eruption of the volcanoes in the great Yokul Mountains, in the south-east corner of the island, took place, which continued with unusual violence for about a week, and then suddenly ceased. Since then no fire has been noticed. Large quantities of ashes have fallen on different localities, but it is believed that the deep bed of snow protected the pasture lands from destruction. Volcanic eruptions took place at the same time in Chili.

THE recent number of *Petermann's Mittheilungen* contains articles and maps on the American North Polar Expedition and Transcaspian Russia. The New Lybian Expedition and the Russian March on Khiva are the subjects of two of the articles.

BY the death of the last surviving porpoise the Brighton Aquarium has to lament the loss of one of its most attractive features.

WE have received the Prospectus of a new club to be called "The Scientific Societies Club." The approaching concentration of scientific societies, the Prospectus says, suggests that the present is a fitting time for the formation of a "Scientific Societies Club," which would afford in the neighbourhood of Burlington House conversation and reading rooms, as well as the usual facilities of a club for members of all scientific societies. In order to render the club generally available and as useful as possible to the scientific world, it is proposed that the entrance fee and the annual subscription shall each be small.

ACCORDING to Dr. Fritsch, the discovery has lately been made of lacustrine dwellings in the vicinity of Leipsic, as the result of certain engineering operations undertaken to regulate the course of the River Elster. After passing through a series of layers at a certain depth, the workmen found a series of oak piles pointed below and decomposed above, and supporting a certain number of oak trunks placed horizontally; and on the same level with these were found certain lower jaws and teeth of oxen, fragments of antlers, broken bones of various mammifers, shells of an Anodon, fragments of pottery, two polished stone hatchets, &c.

PROF. C. A. WHITE, of Iowa State University, and State geologist of Iowa, has been appointed to the new chair of Geology and Natural History at Bowdoin College.

A COMMUNICATION has been made to the Academia dei Lincei of Rome, by M. Tarry, giving the results of his personal experience and investigations into the connection between the cyclonic storms and the showers of sand that frequently visit Southern Europe. M. Tarry, after travelling as secretary to the French Meteorological Society into Northern Africa and the Desert of Sahara, and having consulted the files of the *Daily Weather Bulletin* of the Paris Observatory, believes himself to have established the fact that whenever a cyclone passes southward from Europe over the Mediterranean Sea into Africa (as some few of them do every season), it then returns northward or northwestward, and transports the sand which in the desert formed a sand-storm to the southern coasts of Europe as a sand-shower of greater or less duration. The satisfactory investigation of this subject is much impeded by the absence of barometric observations on the southern shores of the Mediterranean; and to remedy this defect, M. Tarry has recently established new meteorological statistics at Mogadore, Morocco, Terceira, Madeira, and even in the interior of the Sahara.

"GENERAL Remarks on the Climate of Bombay, with a brief description of the Peculiarities of the Weather of the year 1871," is the title of a pamphlet which we have just received, written by Mr. Charles Chambers, F.R.S., Superintendent of the Kolaba Observatory.

THE *Times of India* states that education is making rapid progress in Ceylon, and vernacular schools will soon be within the reach of every section of the native community. The same paper states that Ceylon will contribute a selection of colonial products to the next Exhibition at South Kensington.

THE Rev. Thos. Garnier, Dean of Winchester, who died recently at the age of 98, was the "father" of the Linnean Society, having been elected during the last century, in 1798, only ten years after the foundation of the Society. Some of his contributions to botanical literature bore the date of last century.

THE additions to the Zoological Society's Gardens during the past week include a Garnet's Galago (*Galago garnetti*) from East Africa, presented by Capt. Geo. Butchart; a Manx Shearwater (*Procellaria puffinus*), British, presented by Dr. Bree; a Reeve's Muntjac (*Cervulus reevesi*), from China, presented by Mr. R. Swinhoe; a Spotted Cavy (*Cœlogenys paca*), from South America, presented by Mr. J. de Castro; three Common Chamæleons (*Chamæleon vulgaris*), from Africa, presented by Mr. W. C. Hotham; an Alligator (*Alligator sp.*), presented by Mr. W. Gillespie.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Sept. 8.—M. Bertrand in the chair.—The following papers were read:—Fifth note on Guano, by M. Chevreul.—Note on the observations of M. Lecoq de Boisbaudran, relative to the appearance of Phylloxera in the vineyards of the Charente, by M. Milne-Edwards.—Note on the number of points of intersection which represent a multiple point common to two plane curves, &c., by M. de la Gournerie.—Researches on Crystalline Dissociation, continuation, by MM. P. A. Favre and C. A. Valson. This portion of the paper dealt with the valuation and division of the work done in saline solutions.—Note on a New System of representing the continuous Meteorological Observations, made at the National Observatory, Algiers, by M. Bulard.—Note on Magnetism, third part, by M. J. M. Gaugain.—On the Spontaneous Motion of Ascension of Liquids in Capillary Tubes, by C. Decharme. This portion of the paper treated of the subject from a theoretical point of view.—On Pyrogallol in the presence of iron salts, by M. E. Jacquemin.—Researches on the Spectra of Chlorophyll, by M. J. Chautard. The author has found that this substance so easily changed as viewed from the physiological point of view, is very stable when subjected to chemical reagents.—On the state of the Volcano of Nisiros, in March, 1873, by M. H. Gorceix.—M. de Laval sent a note stating that he was the original proposer of the use of the carbonic disulphide against the Phylloxera.—The ephemerides of Brorsen's Comet were received from Mr. Plummer, and a note on the same comet, and on that of Faye, from M. Stephan.—New observations on the presence of Magnesium on the Solar Limb, and an answer to certain points in M. Faye's theory, by Father Tacchini. The author stated in his letter that the fact of the line 1474 K always appearing with *b*, and even without it, induces him to think that the former is not due to iron which is much heavier than magnesium.—On the use of Chronometers at sea, by M. Magnac.—Reflections on Spontaneous generation, in relation to a note by M. Gayon, on the spontaneous changes of eggs, and a note of Mr. Grace Calvert on the power of preventing the development of Protoplasmic life, by M. A. Béchamp.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE forty-third meeting of the Association was opened yesterday evening in Bradford, when Dr. Carpenter resigned the Presidency, and was succeeded by Prof. A. W. Williamson, who delivered the opening address in St. George's Hall.

Notwithstanding that Bradford is considerably larger than Brighton, its resources in the way of sleeping accommodation have been considerably tried by the unusually large influx of visitors caused by the meeting of the Association. All the hotels, we believe, are full, as well as most of the private houses on the lists of the secretaries. Arrangements have, however, been made with the railway companies for conveying members to and from neighbouring towns where hotel accommodation may be obtained. The local secretaries, Dr. Campbell, Mr. Goddard, and Mr. Piele Thompson, have spared no pains to make the arrangements for the reception of the members of the Association perfect; and if the meeting is not in all respects a complete success, it will be no fault of theirs, nor of the local authorities, who seem anxious to do all in their power for the comfort and enjoyment of the visitors.

A very fine town-hall was opened in Bradford a few days ago, but so far as we can learn, none of the meetings of the Association will be held in it. Ample accommodation has been provided in other buildings for the various meetings. The Sections met to-day at 11 A.M., and continue to do so till Tuesday next. Section A meets in the School Room, Horton Lane Chapel; Section B in the School Room, Unitarian Chapel; Section C in the Lecture Hall, Horton Lane Chapel; Section D in the Church Institute; Section E in the Mechanics' Institute; Section F in the West Riding Court House; and Section G in the Church Institute. To-night a *soirée* will be held in St. George's Hall: in the same place, to-morrow night, at 8 30, Professor W. C. Williamson, F.R.S., of Manchester, delivers a discourse on "Coal and Coal Plants;" on Saturday evening, at 7.30, Dr. Siemens gives a lecture to the operative classes on "Fuel;" and on Monday evening, at 8 30, Professor Clerk-Maxwell, a discourse on "Molecules." On Tuesday next, a *soirée* takes place at 8.30 P.M. in the Mechanics' Institute, where, on Wednesday, the concluding General Meeting will take place at 2.30 P.M.; on the same evening, a Grand Complimentary Concert will be given in St. George's Hall, at 8 o'clock.

A number of Reports, both those involving and those not involving grants of money, will be given in, and will no doubt be listened to with great interest by the scientific men present. We hope that this year the Association will rise to the occasion in the matter of liberality, and give a practical example of what ought to be done in the endowment of scientific research. By the courtesy of the officers we are enabled to give the Inaugural and some of the Sectional Addresses. To the same source we are indebted for the following list of some of the papers to be read in the various sections:—

SECTION A.—Lord Rayleigh: A short paper on a Natural Limit to the Sharpness of the Spectral Lines.—W. Davis: Some Abnormal Effects of Binocular Vision.—H. Muirhead: On Regelation.—G. M. Whipple: A new Electrical Anemograph; a new form of Rutherford's Minimum Thermometer; on the Passage of Squalls across the British Isles.—W. R. Birt: On the Importance and Necessity of continued Systematic Observation of the Moon's Surface.—G. O. Hanlon: Some Suggestions towards the formation of extended Tables of Logarithms.—M. Hermite: On the Irrationality of the Base of Hyperbolic Logarithms.—R. S. Ball: Dynamometers for the Measurement of Force in absolute units; A quiescent rigid body possessing three degrees of freedom receives an impulse: determine the instantaneous screw about which the body commences to twist.

SECTION B.—Messrs. A. Vernon Harcourt and F. W. Fish: On a continuous process for purifying Coal Gas from Sulphuretted Hyd. and Ammonia, and for extracting Sulphur and Ammoniacal Salts.—W. H. Pike: On several Homologues of Oxalic Acid.—Dr. Gladstone: Black Deposits of Metals.—C. Horner: On the Spectra of certain Boric and Phosphoric Acid blow-pipe beads.—J. Spiller: On Artificial Magnetite.—W. Symons: Remarks on a paper by the Marquis of Salisbury on Spectral Lines of Cold Temperature.—A. Tribe: Spec. gr. bottle for liquids spontaneously inflammable in contact with air.

SECTION C.—Rev. J. F. Blake: Additional Remains of Pleistocene Mammals in Yorkshire.—W. Blandford: Some Evidences of Glacial Action in Tropical India.—A. Leith Adams: Concluding Report of the Malta Fossil Elephants.—R. Russell: Geological Sketch of Bradford and the neighbourhood.—J. Hopkinson: On Graptolites found (1) in Ramsay Island, St. David's; (2) in the Ludlow Rocks of Shropshire.—H. Hicks: On the Arenig and Llandeilo Rocks of St. David's.—J. L. Lobley: On the British Palæozoic Arcade.

SECTION D.—Hyde Clarke: Comparative Chronology of Man in America in relation to Comparative Philology.—Prehistoric Names of Weapons.—W. T. Blandford: The Fauna of Persia.—J. Willis: The Flora of the Environs of Bradford.—J. Milnes Fothergill: Heart and Brain.—K. Kaines: A true Cerebral Theory necessary to Anthropology.

SECTION E.—C. F. Beke: On the True Position of Mount Sinai.—W. Blandford: Physical Geography of the Deserts of Persia and Central Asia.—G. Darwin: On Some Maps of the World and on a Portable Globe.—Rev. W. B. Kerr: Overland Route from India.—E. L. Oxenham: A Journey from Pekin to Hankow.—Capt. Davis: The Voyage of the *Challenger*.—Sir F. Goldsmid: On Persia.

SECTION F.—Hyde Clarke: The Influence of Large Centres of Population on Intellectual Manifestation.—Dr. Appleton: On some of the Economical Aspects of Endowments of Education and Original research.—T. G. P. Hallett: The Income Tax Question.—W. P. Henderson: Commercial Panics.—W. Hastings: Postal Reform.—R. H. Palgrave: The Relation of the Banking Reserve of the Bank of England to the Current Rate of Interest.—G. C. T. Barnsley: The Poor-Law Board and its Effect on Thrift.

Among British men of science expected to be present at this year's meeting are the following:—Prof. W. G. Adams, F.R.S.; Major-General Sir J. Alexander, Sir Rutherford Alcock, K.C.B.; Prof. Attfield, Prof. R. S. Ball, Admiral Sir E. Belcher, K.C.B.; W. H. Barlow, F.R.S.; Prof. Balfour, W. Boyd Dawkins, F.R.S.; Sir P. G. Egerton, F.R.S.; Sir W. Fairbairn, F.R.S.; Dr. W. Farr, Prof. Michael Foster, M.D.: Mr. J. G. Fitch, Mr. P. Le Neve Foster, Mr. C. L. N. Foster, Col. Lane Fox, Sir G. D. Gibb, Bart.; Rev. Prof. Griffiths, Capt. D. Galton, C.B.; G. Griffiths, F.C.S.; Prof. Greenwood, Mr. J. W. L. Glaisher, Sir F. Goldsmid, J. P. Gassiot, F.R.S.; Dr. J. H. Gladstone, F.R.S.; Dr. P. H. Holland, W. Huggins, D.C.L., F.R.S.; Prof. Hughes, Lord Houghton, F.R.S.; Prof. G. Harley, F.R.S.; Prof. Herschel, Rev. R. Harley, F.R.S.; Mr. A. V. Harcourt, F.R.S.; Mr. G. J. Holyoake, Mr. A. K. Johnston, Prof. Leone Levi, Prof. J. Clerk Maxwell, F.R.S.; Prof. A. Newton, F.R.S.; Vice-Admiral Ommaney, C.B., F.R.S.: Prof. Phillips, W. Pengelly, F.R.S.; the Earl of Rosse, Prof. G. Rolleston, M.D., F.R.S.; Prof. Roscoe, F.R.S.; Dr. W. Rutherford, Dr. W. J. Russell, F.R.S.; Prof. Savage, Prof. Balfour Stewart, F.R.S.; Major-General Scott, Prof. Smith, Prof. Tyndall, F.R.S.; Prof. W. C. Williamson, F.R.S.; T. Wright, F.S.A.; Prof. Williamson, F.R.S.; the Archbishop of York, &c. The following foreigners are also expected to be present:—M. Guido Cora, Dr. Janssen, Prof. Klein, Baron von Richthofen, Arminius Vambery, &c.

Inaugural Address of Prof. Alexander W. Williamson, F.R.S., President.

INSTEAD of rising to address you on this occasion I had hoped to sit quietly amongst you, and to enjoy the intellectual treat of listening to the words of a man of whom England may well be proud—a man whose life has been spent in reading the great book of nature, for the purpose of enriching his fellow men with a knowledge of its truths—a man whose name is known and honoured in every corner of this planet to which a knowledge of science has penetrated—and, let me add, a man whose name will live in the grateful memory of mankind as long as the records of such noble work are preserved.

At the last meeting of the Association I had the pleasure of proposing that Dr. Joule be elected President for the Bradford Meeting, and our Council succeeded in overcoming his reluctance and in persuading him to accept that office.

Nobly would Joule have discharged the duties of President had his bodily health been equal to the task; but it became apparent after a while that he could not rely upon sufficient strength to justify him in performing the duties of the Chair, and, in obedience to the orders of his physician, he placed his resignation in the hands of the Council about two months ago. When, under these circumstances, the Council did me the great honour of asking me to accept their nomination to the Presidentship, I felt that their request ought to have with me the weight of a command.

For a good many years past Chemistry has been growing at a more and more rapid rate, growing in the number and variety of facts which are added to its domain, and not less remarkably in the clearness and consistency of the ideas by which these facts are explained and systematised. The current literature of chemical research extends each year to the dimensions of a small library; and mere brief extracts of the original papers published annually by the Chemical Society, partly aided by a grant from this Association, take up the chief part of a very stout volume. I could not, if I would, give you to-night even an outline of the chief newly discovered compounds and of the various changes which they undergo, describing each of them by its own name (often a very long one) and recording the specific properties which give to each substance its highest scientific interest. But I am sure that you would not wish me to do so if I could; for we do not meet here to study chemistry; I conceive that we meet here for the purpose of considering what this wondrous activity in our Science means, what is the use of it, and, true to our object as embodied in the name of this Association, to consider what we can do to promote the Advancement of Science. I propose to lay before you some facts bearing on each of these questions, and to submit to you some considerations respecting them.

In order to ascertain the meaning of the work which has been going on in chemistry, it will, I think, be desirable for us to consider the leading ideas which have been in the minds of chemists, and which guide their operations,

Now, since the father of modern chemistry, the great Dalton, gave to chemists a firm hold of the idea of Atoms, their labours have been continually guided by that fundamental idea, and have confirmed it by a knowledge of more and more facts, while at the same time steadily adding to our knowledge of the properties of atoms. Every chemist who is investigating a new compound takes for granted that it must consist of a great number of atom-clusters (called by him molecules), all of them alike, and each molecule consisting of a certain number of atoms of at least two kinds. One of his first endeavours is to ascertain how many atoms of each kind there are in each molecule of the compound. I must not attempt to describe to you the various kinds of experiment which he performs for the purpose of getting this information, how each experiment is carried out with the aid of delicate instruments and ingenious contrivances found by long experience to enable him to obtain the most trustworthy and accurate results; but I want to draw your attention to the reasoning by which he judges of the value of such experiments when they agree among themselves, and to the meaning which he attaches to their result.

If the result of his experiments does not nearly agree with any atomic formula (that is, if no conceivable cluster of atoms of the kinds known to be in the compound would on analysis give such results as those obtained), the chemist feels sure that his experiments must have been faulty: either the sample of substance which he worked upon contained foreign matter, or his analyses were not made with due care. He sets to work again, and goes on till he arrives at a result which is consistent with his knowledge of the combining-properties of atoms. It is hardly necessary to say that even the best experiment is liable to error, and that even a result obtained with the utmost care cannot be expected to afford more than an approximation to the truth. Every good analysis of a pure compound leads to results which approximate to those required by the Atomic Theory; and chemists trust so thoroughly to the truth of that guide, that they correct the results of such analysis by the aid of it.

The chemical idea of atoms serves for two purposes:—

1. It gives a clear and consistent explanation of an immense number of facts discovered by experiment, and enables us to compare them with one another and to classify them.

2. It leads to the anticipation of new facts, by suggesting new compounds which may be made; at the same time it teaches us that no compounds can exist with their constituents in any other than atomic proportions, and that experiments which may imply the existence of any such compound are faulty.

We have the testimony of the great Berzelius to the flood of light which the idea of atoms at once threw on the facts respecting combining proportions which had been accumulated before it was made known; and from that time forward its value has rapidly increased as each succeeding year augmented the number of facts which it explained.

Allow me at this point of my narrative to pause for a moment in order to pay a tribute of respect and gratitude to the memory of one who has recently passed from among us, and who in the time of his full activity was a leader of the discoveries of new facts in the most difficult part of our science. Liebig has been generally known in this country through his writings on agricultural chemistry, through his justly popular letters on chemistry, and other writings, by means of which his brilliant intellect and ardent imagination stimulated men to think and to work. Among chemists he was famed for his numerous discoveries of new organic compounds, and their investigation by the aid of improved methods; but I believe that the greatest service which his genius rendered to science was the establishment of the chemical school of Giessen, the prototype of the numerous chemical schools for which Germany is now so justly celebrated. I think it is not too much to say that the Giessen laboratory, as it existed some thirty years ago, was the most efficient organisation for the promotion of chemistry which had ever existed.

Picture to yourselves a little community of which each member was fired with enthusiasm for learning by the genius of the great master, and of which the best energies were concentrated on the one object of experimental investigation.

The students were for the most part men who had gone through a full curriculum of ordinary studies at some other University, and who were attracted from various parts of the world by the fame of this school of research.

Most of the leading workers of the next generation were pupils of Liebig; and many of them have established similar schools of research.

We must not, however, overlook the fact that Liebig's genius and enthusiasm would have been powerless in doing this admirable work, had not the rulers of his Grand-Duchy been enlightened enough to know that it was their duty to supply him with the material aids requisite for its successful accomplishment.

Numberless new compounds have been discovered under the guidance of the idea of atoms; and in proportion as our knowledge of substances and of their properties became more extensive, and our view of their characteristics more accurate and general, were we able to perceive the outlines of their natural arrangement, and to recognise the distinctive characteristics of various classes of substances. I wish I could have the pleasure of describing to you the origin and nature of some of these admirable discoveries, such as homologous series, types, radicals, &c.; but it is more to our purpose to consider the effect which they had upon the idea of atoms, an idea which, still in its infancy, was plunged into the intellectual turmoil arising from a variety of novel and original theories suggested respectively by independent workers as best suited for the explanation of the particular phenomena to which their attention was mainly directed.

Each of these workers was inclined to attach quite sufficient importance to his own new idea, and to sacrifice for its sake any other one capable of interfering with its due development.

The father of the atomic theory was no more; and the little infant had no chance of life, unless from its own sterling merits it were found useful in the work still going on.

What then was the result? Did it perish like an ephemeral creation of human fancy? or did it survive and gain strength by the inquiries of those who questioned Nature and knew how to read her answers?

Although anticipating my answer to these questions, you will probably be surprised to hear the actual result which I have to record, a result so wonderful that the more I think of it the more I marvel at it. Not only did these various theories contain nothing at variance with the atomic theory; they were found to be natural and necessary developments of it, and to serve for its application to a variety of phenomena which were unknown to its founder.

Among the improvements of our knowledge of atoms which have taken place, I ought to mention the better evaluations of the relative weight of atoms of different kinds, which have been made since Dalton's time. More accurate experiments than those which were then on record have shown us that certain atoms are a little heavier or lighter than was then believed, and the work of perfecting our observations is constantly going on with the aid of better instruments and methods of operation. But, apart from these special corrections, a more sweeping change has taken place, not in consequence of more accurate experiments interpreted in the usual way, but in consequence of a more comprehensive view of the best experimental results which had been obtained, and a more consistent interpretation of them. Thus the atomic weight of carbon had been fixed at 6 by Dumas's admirable experiments; and it was quite conceivable that a still more perfect determination might slightly increase or diminish this number. But those who introduced the more sweeping change asserted in substance that two of these supposed atoms, whatever may be the precise weight of each, always are together and never separate from one another; and they accordingly applied the term atom to that indivisible mass of carbon weighing twice as much as a carbon atom had been supposed to weigh. So also with regard to other elements, it has been shown that many atoms are really twice as heavy as had been supposed, according to the original interpretation of the best experiments. This change was brought about by what I may be permitted to call the operation of stock-taking. Dalton first took stock of our quantitative facts in a business-like manner; but the amount and variety of our chemical stock increased so enormously after his time, that the second stock-taking absorbed the labours of several men for a good many years. They were men of different countries and very various turns of mind; but, as I mentioned just now, they found no other fundamental idea to work with than Dalton's; and the result of their labours has been to confirm the truth of that idea and to extend greatly its application.

One of the results of our endeavours to classify substances according to their natural resemblances has been the discovery of distinct family relationships among atoms, each family being distinguished by definite characteristics. Now among the properties which thus characterise particular families of atoms, there is one of which the knowledge gradually worked out by the labours of an immense number of investigators must be admitted to constitute one of the most important additions ever made to our knowledge of these little masses.

I will endeavour to explain it to you by a simple example. An atom of chlorine is able to combine with one atom of hydrogen or one atom of potassium; but it cannot combine with two atoms. An atom of oxygen, on the other hand, can combine with two atoms of hydrogen or with two atoms of potassium, or with one atom of hydrogen and one of potassium; but we cannot get it in combination with one atom of hydrogen or of potassium solely.

Again, an atom of nitrogen is known in combination with three atoms of hydrogen; while an atom of carbon combines with four of hydrogen. Other atoms are classified, from their resemblance to these respectively, as Monads, Dyads, Triads, Tetrads, &c.

The combining value which we thus recognise in the atoms of these several classes has led us naturally to a consideration of the order in which atoms are arranged in a molecule. Thus, in the compound of oxygen with hydrogen and potassium, each of these latter atoms is directly combined with the oxygen, and the atom of oxygen serves as a connecting link between them. Hydrogen and potassium have never been found capable of uniting directly with one another; but when both combined with one atom of oxygen they are in what may be called indirect combination with one another through the medium of that oxygen.

One of the great difficulties of chemistry some few years ago was to explain the constitution of isomeric compounds, those compounds whose molecules contain atoms of like kinds and in equal numbers, but which differ from one another in their properties. Thus a molecule of common ether contains four atoms of carbon, ten atoms of hydrogen, and one of oxygen. Butylic alcohol, a very different substance, has precisely the same composition. We now know that in the former the atom of oxygen is in the middle of a chain of carbon atoms, whereas in the latter it is at one end of that chain. You might fancy it impossible to decide upon anything like consistent evidence such questions as this; but I can assure you that the atomic theory, as now used by chemists, leads frequently to conclusions of this kind, which are confirmed by independent observers, and command general assent. That these conclusions are, as far as they go, true descriptions of natural phenomena is shown by the fact that each of them serves in its turn as a stepping-stone to further discoveries.

One other extension of our knowledge of atoms I must briefly mention, one which has as yet received but little attention, yet which will, I venture to think, be found serviceable in the study of the forces which bring about chemical change.

The original view of the constitution of molecules was statical; and chemists only took cognizance of those changes of place among their atoms which result in the disappearance of the molecules employed, and the appearance of new moleclules formed by their reaction on one another. Thus, when a solution of common salt (sodic chloride) is mixed with a solution of silver nitrate, it is well-known that the metallic atoms in these respective compounds change places with one another, forming silver chloride and sodic nitrate; for the silver chloride soon settles to the bottom of the solution in the form of an insoluble powder, while the other product remains dissolved in the liquid. But as long as the solution of salt remained undecomposed, each little molecule in it was supposed to be chemically at rest. A particular atom of sodium which was combined with an atom of chlorine was supposed to remain steadily fixed to it. When this inactive solution was mixed with the similarly inactive solution of silver nitrate, the interchange of atoms known to take place between their respective molecules was nominally explained by the force of predisposing affinity. It was, in fact, supposed that the properties of the new compounds existed and produced effects, before the compounds themselves had been formed.

I had occasion to point out a good many years ago that molecules which appear to be chemically at rest are acting on one another, when in suitable conditions, in the same kind of way as those which are manifestly in a state of chemical change—that for instance, the molecules of liquid sodic chloride exchange sodium atoms with one another, forming new molecules of the same compound undistinguishable from the first, so that, in an aggregate of like molecules, the apparent atomic rest is the result of the interchange of like atoms between contiguous molecules. Such exchanges of atoms take place not only between molecules of identical composition, but also between contiguous molecules containing different elements. For instance, in a mixture of sodic chloride and potassic iodide an interchange of metallic atoms takes place, forming potassic chloride and sodic iodide. The result of the exchange in such a case is to form a couple of new molecules different from the original couple. But these products are subject to the same general law of atomic changes, and their action on one another reproduces a couple of molecules of the materials.

Thus a liquid mixture formed from two compounds contains molecules of four kinds, which we may describe as the two materials and the two products. The materials are reacting on one another, forming the products; and these products are, in their turn, reacting on one another, reproducing the materials.

If one of the products of atomic exchange between two molecules is a solid while the other remains liquid (as when sodic chloride is mixed with silver nitrate), or if one is gaseous while the other remains liquid, so that the molecules of the one kind cannot react on those of the other kind and reproduce the materials, then the continued reaction of the materials on one another leads to their complete mutual decomposition. Such complete mutual decomposition of two salts takes place whenever they react on one another under such conditions that the products cannot react on one another and reproduce the materials; whereas partial decomposition takes place whenever the materials form a homogeneous mixture with the products.

Now, if in any such homogeneous mixture more exchanges of atoms take place between the materials than between the products, the number of molecules of the products is increased, because more of them are being made than unmade; and reciprocally, if more exchanges of atoms take place between the products than between the materials, the number of molecules of the materials is increased. The mixture remains of constant composition when there are in the unit of time as many decomposing changes as reproducing changes.

Suppose that we were to determine by experiment the proportion between the number of molecules of the materials, and the number of molecules of the products, in a mixture the composition of which remains constant, and that we found, for instance, twice as many of materials as of products; what would this mean? Why, if every two couples of materials only effect in the unit of time as many exchanges as every one couple of products, every couple of materials is only exchanging half as fast as every couple of products.

In fact you perceive that a determination of the proportion in which the substances are present in such a mixture will give us a measure of the relative velocities of those particular atomic motions; and we may thus express our result:—The force of chemical combination is inversely proportional to the number of atomic interchanges.

I cannot quit this part of our subject without alluding to the fact that some few chemists, of such eminence as to be entitled to the most respectful attention, have of late years expressed an opinion that the idea of atoms is not necessary for the explanation of the changes in the chemical constitution of matter, and have sought as far as possible to exclude from their language any allusion to atoms.

It would be out of place on this occasion to enter into any discussion of the questions thus raised; but I think it right to point out:—

I. That these objectors have not shown us any inconsistency in the atomic theory, nor in the conclusions to which it leads.

II. That neither these nor any other philosophers have been able to explain the facts of chemistry on the assumption that there are no atoms, but that matter is infinitely divisible.

III. That when they interpret their analyses, these chemists allow themselves neither more nor less latitude than the atomic theory allows; in fact, they are unconsciously guided by it.

These facts need no comment from me.

Our science grows by the acquisition of new facts which have an intelligible place among our ideas of the order of nature; but in proportion as more and more facts are arranged before us in their natural order, in proportion as our view of the order of nature becomes clearer and broader, we are able to observe and describe that order more fully and more accurately—in fact, to improve our ideas of the order of nature. These more extensive and more accurate ideas suggest new observations, and lead to the discovery of truths which would have found no place in the narrower and less accurate system. Take away from Chemistry the ideas which connect and explain the multifarious facts observed, and it is no longer a science; it is nothing more than a confused and useless heap of materials.

The answer to our question respecting the meaning of the earnest work which is going on in our science must, I think, now be plain to you. Chemists are examining the combining properties of atoms, and getting clear ideas of the constitution of matter.

Admitting, then, for the present, that such is the meaning of chemical work, we have to consider the more important question of its use; and I think you will agree with me that, in order to judge soundly whether and in what manner such a pursuit is useful, we have to consider its effect upon Man. What habits of mind does it engender? What powers does it develop? Does it develop good and noble qualities and aspirations, and tend to make men more able and more anxious to do good to their fellow-men? Or is it a mere idle amusement, bearing no permanent fruits of improvement?

You will, I think, answer these questions yourselves if I can succeed in describing to you some of the chief qualities which experience has shown to be requisite for the successful pursuit of Chemistry, and which are necessarily cultivated by those who qualify themselves for such a career.

One of the first requirements on the part of an investigator is accuracy in observing the phenomena with which he deals. He must not only see the precise particulars of a process as they present themselves to his observation; he must also observe the order in which these particular appearances present themselves under the conditions of each experiment. No less essential is accuracy of memory. An experimental inquirer must remember accurately a number of facts; and he needs to remember their mutual relations, so that one of them when present to his mind may recall those others which ought to be considered with it. In fact, he cultivates the habit of remembering facts mainly by their place in nature. Accuracy in manual operations is required in all experimental inquiries; and many of them afford scope for very considerable skill and dexterity.

These elementary qualities are well known to be requisite for success in experimental science, and to be developed by careful practice of its methods; but some higher qualities are quite as necessary as these in all but the most rudimentary manipulations, and are developed in a remarkable degree by the higher work of science.

Thus it is of importance to notice that a singularly good training in the accurate use of words is afforded by experimental chemistry. Everyone who is about to enter on an inquiry, whether he be a first-year's student who wants to find the constituents of a common salt, or whether he be the most skilled and experienced of chemists, seeks beforehand to get such information from the records of previous observations as may be most useful for his purpose. This information he obtains through the medium of words; and any failure on his part to understand the precise meaning of the words conveying the information requisite for his guidance is liable to lead him astray. Those elementary exercises in analytical chemistry, in which brief directions to the students alternate with their experiments and their reports of experiments made and conclusions drawn, afford a singularly effective training in the habit of attending accurately to the meaning of words used by others, and of selecting words capable of conveying without ambiguity the precise meaning intended. Any inaccuracy in the student's apprehension of the directions given, or in the selection of words to describe his observations and conclusions, is at once detected when the result to which he ought to have arrived is known beforehand to the teacher.

Accuracy of reasoning is no less effectively promoted by the work of experimental chemistry. It is no small facility to us that the meaning of the words which we use to denote properties of matter and operations can be learnt by actual observation. Moreover each proposition comprised in chemical reasonings conveys some distinct statement susceptible of verification by similar means; and the validity of each conclusion can be tested, not only by examining whether or not it follows of necessity from true premisses, but also by subjecting it to the independent test of special experiment.

Chemists have frequent occasion to employ arguments which indicate a probability of some truth; and the anticipations based upon them serve as guides to experimental inquiry by selecting crucial tests. But they distinguish most carefully such hypotheses from demonstrated facts.

Thus a pale green solution, stated to contain a pure metallic salt, is found to possess some properties which belong to salts of Iron. Nothing else possesses these properties except salts of Nickel; and they manifest a slight difference from Iron salts in one of the properties observed.

The analyst could not see any appearance of that peculiarity which distinguishes Nickel salts; so he concludes that he has probably got iron in his solution, but almost certainly either Iron or Nickel. He then makes an experiment which will, he knows, give an entirely different result with Iron salts and Nickel salts; and he gets very distinctly the result which indicates Iron.

Having found in the green liquid properties which the presence of Iron could alone impart, he considers it highly probable that Iron is present. But he does not stop there; for, although the facts before him seem to admit of no other interpretation, he knows that, from insufficient knowledge or attention, mistakes are sometimes made in very simple matters. The analyst therefore tries as many other experiments as are known to distinguish Iron salts from all others; and if any one of these leads distinctly to a result at variance with his provisional conclusion, he goes over the whole inquiry again, in order to find where his mistake was. Such inquiries are practised largely by students of chemistry, in order to fix in their minds, by frequent use, a knowledge of the fundamental properties of the common elements, in order to learn by practice the art of making experiments, and, above all, in order to acquire the habit of judging accurately of evidence in natural phenomena. Such a student is often surprised at being told that it is not enough for him to conduct his experiments to such a point that every conclusion except one is contrary to the evidence before him—that he must then try every confirmatory test which he can of the substance believed to be present, and ascertain that the sample in his hands agrees, as far as he can see, in all properties of the known substance of which he believes it to be a specimen.

Those who tread the path of original inquiry, and add to human knowledge by their experiments, are bound to practise this habit with the most scrupulous fidelity and care, or many and grave will be the mistakes they will make.

Thus a chemist thinks it probable that he might prepare some well-known organic body of the aromatic family by a new process. He sets to work and obtains a substance agreeing in appearance, in empirical composition, in molecular weight, and in many other properties with the compound which he has in view. He is, however, not satisfied that his product is a sample of that compound until he has examined carefully whether it possesses all the properties which are known to belong to the substance in question. And many a time is his caution rewarded by the discovery of some distinct difference of melting-point, or of crystalline form, &c., which proves that he has made a new compound isomeric with the one which he expected to make. It seemed probable, from the agreement of the two substances in many particulars, that they might be found to agree in all, and might be considered to be the same compound; but complete proof of that conclusion consists in showing that the new substance agrees with all that we know of the old one.

In the most various ways chemists seek to extend their knowledge of the uniformity of nature; and their reasonings by analogy from particulars to particulars suggest the working hypotheses which lead to new observations. Before, however, proceeding to test the truth of his hypothesis by experiment, the chemist passes in review, as well as he can, all the general knowledge which has any bearing on it, in order to find agreement or disagreement between his hypothesis and the ideas established by past experience. Sometimes he sees that his hypothesis is at variance with some general law in which he has full confidence, and he throws it aside as disproved by that law. On other occasions he finds that it follows of necessity from some known law, and he then proceeds to verify it by experiment, with a confident anticipation of the result. In many cases the hypothesis does not present sufficiently distinct agreement or disagreement with the ideas established by previous investigations to justify either the rejection of it or a confident belief in its truth; for it often happens that the results of experience of similar phenomena are not embodied in a sufficiently definite or trustworthy statement to have any other effect than that of giving probability or the contrary to the hypothesis.

Another habit of mind which is indispensable for success in experimental chemistry, and which is taught by the practice of its various operations, is that of truthfulness.

The very object of all our endeavours is to get true ideas of the natural processes of chemical action; for in proportion as our ideas are true do they give us the power of directing these processes. In fact, our ideas are useful only so far as they are true; and he must indeed be blind to interest and to duty who could wish to swerve from the path of truth. But if anyone were weak enough to make the attempt, he would find his way barred by innumerable obstacles.

Every addition to our science is a matter of immediate interest and importance to those who are working in the same direction. They verify in various ways the statements of the first discoverer, and seldom fail to notice further particulars, and to correct any little errors of detail into which he may have fallen. They soon make it a stepping-stone to further discoveries. Anything like wilful misrepresentation is inevitably detected and made known.

It must not, however, be supposed that the investigator drifts unconsciously into the habit of truthfulness for want of temptation to be untruthful, or even that error presents itself to his mind in a grotesque and repulsive garb, so as to enlist from the first his feelings against it; for I can assure you that the precise contrary of these things happens. Error comes before him usually in the very garb of truth, and his utmost skill and attention are needed to decide whether or not it is entitled to retain that garb.

You will easily see how this happens if you reflect that each working hypothesis employed by an investigator is an unproven proposition, which bears such resemblance to truth as to give rise to hopes that it may really be true. The investigator trusts it provisionally to the extent of trying one or more experiments, of which it claims to predict the specific result. Even though it guide him correctly for a while, he considers it still on trial until it has been tested by every process which ingenuity can suggest for the purpose of detecting a fault.

Most errors which an experimentalist has to do with are really imperfect truths, which have done good service in their time by guiding the course of discovery. The great object of scientific work is to replace these imperfect truths by more exact and comprehensive statements of the order of nature.

Whoever has once got knowledge from Nature herself by truthful reasoning and experiment, must be dull indeed if he does not feel that he has acquired a new and noble power, and if he does not long to exercise it further, and make new conquests from the realm of darkness by the aid of known truths.

The habit of systematically searching for truth by the aid of known truths, and of testing the validity of each step by constant reference to Nature, has now been practised for a sufficiently long time to enable us to judge of some of its results.

Every true idea of the order of Nature is an instrument of thought. It can only be obtained by truthful investigation; and it can only be used effectively in obedience to the same laws. But the first idea which is formed of anything occurring in nature affords only a partial representation of the actual reality, by recording what is seen of it from a particular point of view. By examining a thing from different points of view we get different ideas of it; and when we compare these ideas accurately with one another, recollecting how each one was obtained, we find that they really supplement each other.

We try to form in our minds a distinct image of a thing capable of producing these various appearances; and when we have succeeded in doing so, we look at it from the different points of view from which the natural object has been examined, and find that the ideas so obtained meet at the central image. It usually happens that an accurate examination of the mutual bearings of these ideas on a central image suggests additions to them and correction of some particulars in them.

Thus it is that true ideas of a natural phenomenon confirm and strengthen one another; and he who aids directly the development of one of them is sure to promote indirectly the consolidation of others.

Each onward step in the search for truth has made us stronger for the work; and when we look back upon what has been done by the efforts of so many workers simply but steadily directed by truth towards further truth, we see that they have achieved, for the benefit of the human race, the conquest of a systematic body of truths which encourages men to similar efforts while affording them the most effectual aid and guidance.

This lesson of the inherent vitality of truth, which is taught us so clearly by the history of our science, is well worthy of the consideration of those who, seeing that iniquity and falsehood so frequently triumph for a while in the struggle for existence, are inclined to take a desponding view of human affairs, and almost to despair of the ultimate predominance of truth and goodness. I believe it would be impossible at the present time to form an adequate idea of the vast consequences which will follow from the national adoption of systematic measures for allowing our knowledge of truth to develop itself freely, through the labours of those who are willing and able to devote themselves to its service, so as to strengthen more and more the belief and trust of mankind in its guidance, in small matters as well as in the highest and most important considerations.

I am desirous of describing briefly the more important of those measures; but first let me mention another habit of mind which naturally follows from the effective pursuit of truth—a habit which might be described in general terms as the application to other matters of the truthfulness imparted by science.

The words which the great German poet put into the mouth of Mephistopheles when describing himself to Faust afford perhaps the most concise and forcible statement of what we may call the anti-scientific spirit:—

> Ich bin der Geist der stets verneint,
> Dem alles, was entsteht, zuwider ist.

The true spirit of science is certainly affirmative, not negative; for, as I mentioned just now, its history teaches us that the development of our knowledge usually takes place through two or more simultaneous ideas of the same phenomenon, quite different from one another, both of which ultimately prove to be parts of some more general truth; so that a confident belief in one of those ideas does not involve or justify a denial of the others.

I could give you many remarkable illustrations of this law from among ideas familiar to chemists. But I want you to consider with me its bearing on the habit of mind called toleration, of which the development in modern times is perhaps one of the most hopeful indications of moral improvement in man.

In working at our science we simply try to find out what is true; for although no usefulness is to be found at first in most of our results, we know well that every extension of our knowledge of truth is sure to prove useful in manifold ways. So regular an attendant is usefulness upon truth in our work, that we get accustomed to expect them always to go together, and to believe that there must be some amount of truth wherever there is manifest usefulness.

The history of human ideas, so far as it is written in the records of the progress of science, abounds with instances of men contributing powerfully to the development of important general ideas, by their accurate and conscientious experiments, while at the same time professing an actual disbelief in those ideas. Those records must indeed have been a dead letter to any who could stand carping at the intellectual crotchets of a good and honest worker, instead of giving him all brotherly help in the furtherance of his work.

To one who knows the particulars of our science thoroughly, and who knows also what a variety of ideas have been resorted to in working out the whole body of truths of which the science is composed, there are few more impressive and elevating subjects of contemplation than the unity in the clear and bold outline of that noble structure.

I hope that you will not suppose, from my references to chemistry as promoting the development of these habits and powers of mind, that I wish to claim for that particular branch of science any exclusive merit of the kind; for I can assure you that nothing can be further from my intention.

I conceived that you would wish me to speak of that department of science which I have had occasion to study more particularly; but much that I have said of it might be said with equal truth of other studies, while some of its merits may be claimed in a higher degree by other branches of science. On the other hand, those highest lessons which I have illustrated by chemistry are best learnt by those whose intellectual horizon includes other provinces of knowledge.

Chemistry presents peculiar advantages for educational purposes in the combination of breadth and accuracy in the training which it affords; and I am inclined to think that in this respect it is at present unequalled. There is reason to believe that it will play an important part in general education, and render valuable services to it in conjunction with other scientific and with literary studies.

I trust that the facts which I have submitted to your consideration may suffice to show you how fallacious is that materialistic idea of physical science which represents it as leading away from the study of man's noblest faculties, and from a sympathy with his most elevated aspirations, towards mere inanimate matter. The material work of science is directed by ideas towards the attainment of further ideas. Each step in science is an addition to our ideas, or an improvement of them. A science is but a body of ideas respecting the order of nature.

Each idea which forms part of physical science has been derived from observation of nature, and has been tested again and again in the most various ways by reference to nature; but this very soundness of our materials enables us to raise upon the rock of truth a loftier structure of ideas than could be erected on any other foundation by the aid of uncertain materials.

The study of science is the study of man's most accurate and perfect intellectual labours; and he who would know the powers of the human mind must go to science for his materials.

Like other powers of the mind, the imagination is powerfully exercised, and at the same time disciplined, by scientific work. Every investigator has frequent occasion to call forth in his mind a distinct image of something in nature which could produce the appearances which he witnesses, or to frame a proposition embodying some observed relation; and in each case the image or the proposition is required to be true to the materials from which it is formed. There is perhaps no more perfect elementary illustration of the accurate and useful employment of the imagination than the process of forming in the language of symbols, from concrete data, one of those admirable general propositions called equations; on the other hand, the contemplation of the order and harmony of nature as disclosed to us by science supplies the imagination with materials of surpassing grandeur and brilliancy, while at the same time affording the widest scope for its effects.

The foregoing considerations respecting the meaning and use of scientific work will, I trust, afford us aid in considering what measures ought to be taken in order to promote its advancement, and what we can do to further the adoption of such measures.

Like any other natural phenomenon, the growth of knowledge in the human mind is favoured and promoted by certain circumstances, impeded or arrested by others; and it is for us to ascertain from experience what those circumstances respectively are, and how the favourable ones can be best combined to the exclusion of the others.

The best and noblest things in this world are the result of gradual growth by the free action of natural forces; and the proper function of legislation is to systematise the conditions most favourable to the free action which is desired.

I shall consider the words "Advancement of Science" as referring to the development and extension of our systematic knowledge of natural phenomena by investigation and research.

The first thing wanted for the work of advancing science is a supply of well qualified workers. The second thing is to place and keep them under the conditions most favourable to their efficient activity. The most suitable men must be found while still young, and trained to the work. Now I know only one really effectual way of finding the youths who are best endowed by nature for the purpose; and that is to systematise and develop the natural conditions which accidentally concur in particular cases, and enable youths to rise from the crowd.

The first of these is that a young man gets a desire for knowledge by seeing the value and beauty of some which he has acquired. When he has got this desire, he exerts himself to increase his store; and every difficulty surmounted increases his love of the pursuit, and strengthens his determination to go on. His exertions are seen by some more experienced man, who helps him to place himself under circumstances favourable to further progress. He then has opportunities of seeing original inquiries conducted, perhaps even of aiding in them; and he longs to prove that he also can work out new truths, and make some permanent addition to human knowledge. If his circumstances enable him to prosecute such work, and he succeeds in making some new observations worthy of publication, he is at once known by them to the community of scientific men, and employed among them.

We want, then, a system which shall give to the young favourable opportunities of acquiring a clear and, as far as it goes, a thorough knowledge of some few truths of nature such as they can understand and enjoy—which shall afford opportunity of further and further instruction to those who have best profited by that which has been given to them, and are anxious to obtain more—which shall enable the best students to see what original investigation is, and, if possible, to assist in carrying out some research—and, finally, which shall supply to each student who has the power and the will to conduct researches, all material conditions which are requisite for the purpose.

But investigators, once found, ought to be placed in the circumstances most favourable to their efficient activity.

The first and most fundamental condition for this is, that their desire for the acquisition of knowledge be kept alive and fostered. They must not merely retain the hold which they have acquired on the general body of their science; they ought to strengthen and extend that hold, by acquiring a more complete and accurate knowledge of its doctrines and methods; in a word, they ought to be more thorough students than during their state of preliminary training.

They must be able to live by their work, without diverting any of their energies to other pursuits; and they must feel security against want in the event of illness or old age.

They must be supplied with intelligent and trained assistants to aid in the conduct of their researches, and whatever buildings, apparatus, and materials may be required for conducting those researches effectively.

The desired system must therefore provide arrangements favourable to the maintenance and development of the true student-spirit in investigators while providing them with permanent means of subsistence, sufficient to enable them to feel secure and tranquil in working at science alone, yet not sufficient to neutralise their motives for exertion; and at the same time it must give them all external aids, in proportion to their wants and powers of making good use of them.

Now I propose to describe the outlines of such a system, framed for the sole purpose of promoting research, and then to consider what other results would follow from its working.

If it should appear possible to establish a system for the efficient advancement of science, which would be productive of direct good to the community in other important ways, I think you will agree with me that we ought to do all we can to promote its adoption.

Let the most intelligent and studious children from every primary school be sent, free of expense, to the most accessible secondary school for one year; let the best of these be selected and allowed to continue for a second year, and so on, until the *élite* of them have learnt all that is to be there learnt to advantage. Let the best pupils from the secondary schools be sent to a college of their own selection, and there subjected to a similar process of annual weeding; and, finally, let those who get satisfactorily to the end of a college curriculum be supplied with an allowance sufficient for their maintenance for a year, on condition of their devoting their undivided energies to research, under the inspection of competent college authorities, while allowed such aids and facilities as the college can supply, with the addition of money-grants for special purposes. Let all who do well during this first year be allowed similar advantages for a second and even a third year.

Each young investigator thus trained must exert himself to obtain some appointment, which may enable him to do the most useful and creditable work of which he is capable, while combining the conditions most favourable to his own improvement.

Let there be in every college as many Professorships and Assistantships in each branch of science as are needed for the efficient conduct of the work there going on, and let every Professor and Assistant have such salary and such funds for apparatus, &c., as may enable him to devote all his powers to the duties of his post, under conditions favourable to the success of those duties; but let each professor receive also a proportion of the fees paid by his pupils, so that it may be his direct interest to do his work with the utmost attainable efficiency, and attract more pupils.

Let every college and school be governed by an independent body of men, striving to increase its usefulness and reputation, by sympathy with the labours of the working staff, by material aid to them when needed, and by getting the very best man they can, from their own or any other college, to supply each vacancy as it arises.

In addition to colleges, which are and always have been the chief institutions for the advancement of learning, establishments for the observation of special phenomena are frequently needed, and will doubtless be found desirable in aid of a general system for the advancement of science.

Now, if a system fulfilling the conditions which I have thus briefly sketched out were once properly established on a sufficient scale, it ought to develop and improve itself by the very process of its working; and it behoves us, in judging of the system, to consider how such development and improvement would come about.

The thing most needed at the present time for the advancement of science is a supply of teachers devoted to that object—men so earnestly striving for more knowledge and better knowledge as to be model students, stimulating and encouraging those around them by their example as much as by their teaching. Young men do not prepare themselves in any numbers for such a career:—

I. Because the chief influences which surround them at school and at college are not calculated to awaken in them a desire to obtain excellence of such kind.

II. Because they could not expect by means of such qualities to reach a position which would afford a competent subsistence.

Let these conditions be reversed, to the extent that existing teachers have powerful inducements to make their students love the study of science for its own sake, with just confidence that they will be able to earn a livelihood if they succeed in qualifying themselves to advance science, and the whole thing is changed. The first batch of young investigators will be dispersed among schools and colleges according to their powers and acquirements, and will improve their influence upon the pupils, and enable them to send up a second batch better trained than the first. This improvement will go on increasing, if the natural forces which promote it are allowed free play: and the youth of each successive generation will have better and more frequent opportunities of awakening to a love of learning, better help and guidance in their efforts to acquire and use the glorious inheritance of knowledge which had been left them, better and more numerous living examples of men devoting their whole lives to the extension of the domain of truth, and seeking their highest reward in the consciousness that their exertions have benefited their fellow-men, and are appreciated by them.

A young man who is duly qualified for the work of teaching the investigation of some particular branch of science, and who wishes to devote himself to it, will become a member of an association of men selected for their known devotion to learning, and for their ability to teach the methods of investigation in their respective subjects. Around this central group is ranged a frequently changing body of youths who trust to them for encouragement and guidance in their respective studies.

Our young investigator finds it necessary to study again more carefully many parts of his subject, and to examine accurately the evidence of various conclusions which he had formerly adopted, in order that he may be able to lead the minds of his pupils by easy and natural yet secure steps to the discovery of the general truths which are within their reach. He goes over his branch of science again and again from the foundation upwards striving each time to present its essential particulars more clearly and more forcibly, arranging them in the order best calculated to stimulate an inquiring mind to reflect upon their meaning, and to direct its efforts effectively to the discovery of the general ideas which are to be derived from them. He is encouraged in these efforts by the sympathy of his colleagues, and often aided by suggestions derived from their experience in teaching other branches of science, or by information respecting doctrines or methods which throw a light upon those of his own subject.

No known conditions are so well calculated to give a young investigator the closest and strongest grasp of his object of which he is capable as those in which he is placed while thus earnestly teaching it in a college; and inasmuch as a thorough mastery of known truths is needed by everyone who would work to advantage at the discovery of new truths of that kind, it will, in most cases, be an object of ambition to the ablest young investigators to get an opportunity of going through the work of teaching in a college, in order to improve themselves to the utmost for the work of original research. There is, however, another advantage to them in having such work to do; for the best way to ascertain at any one time what additions may be made to a science, is to examine the facts which have been discovered last, and to consider how far they confirm and extend the established ideas of the science, how far they militate against those ideas. An investigating teacher is constantly weaving new facts into the body of his science, and forming anticipations of new truths by considering the relation of these new facts to the old ones.

When our investigator has thus got a thorough mastery of his science and new ideas for its extension, he ought to have the opportunity of turning his improved powers to account by devoting more of his time to original research; in fact he ought to teach research by example more than hitherto, and less by elementary exercises upon known facts. If he has discharged the duties of his first post with manifest efficiency, he will be promoted, either in his own or some other college, to a chair affording more leisure and facility for original research by his own hands and by those of his assistants and pupils. Some investigators may find it desirable to give up after a while all teaching of previously published truths, and confine themselves to guiding the original researches of advanced pupils, while stimulating them by the example of their own discoveries. But most of them will probably prefer to do elementary teaching work from time to time, for the sake of the opportunity of going over the groundwork of their science, with a knowledge of the new facts and enlarged ideas recently established.

Now it must be observed that such a system as the above, once developed to its proper proportions, so as to send annually to secondary schools many thousands of poor children who would otherwise never enjoy such advantages, and so as to train to original investigation a corresponding proportion of them, would not only provide more young investigators than would be needed for systematic teaching functions, but would also give a partial training of the same kind to many whose abilities proved to be insufficient, or whose tastes were not congenial to such pursuit. Some would be tempted by an advantageous opening in an industrial pursuit or in the public service to break off their studies before completion, and others would find, after completing their training, a position of that kind more desirable or more attainable than a purely scientific appointment. Not only would much good of other kinds be accomplished by this circumstance, but we may say with confidence that the system could not work with full advantage for its own special purpose of promoting the advancement of science if it did not diffuse a knowledge of the truths and methods of science beyond the circle of teachers.

There is an urgent need of accurate scientific knowledge for the direction of manufacturing processes, and there could not be a greater mistake than to suppose that such knowledge need not go beyond the elementary truths of science. In every branch of manufacture improvements are made from time to time, by the introduction of new or modified processes which had been discovered by means of investigations as arduous as those conducted for purely scientific purposes, and involving as great powers and accomplishments on the part of those who conducted them.

Any manufacturer of the present day who does not make efficient arrangements for gradually perfecting and improving his processes ought to make at once enough money to retire; for so many are moving onwards in this and other countries, that he would soon be left behind.

It would be well worth while to establish such a system of scientific education for the sake of training men to the habits of mind which are required for the improvement of the manufacturing arts; and I have no doubt that the expense of working the system would be repaid a hundred times over by the increase of wealth of the community; but I only mention this as a secondary advantage of national education.

A system of the kind could not expand to due dimensions, nor could it, once fully established, maintain itself in full activity, without intelligent sympathy from the community; and accordingly its more active-minded members must be taught some good examples of the processes and results of scientific inquiry, before they can be expected to take much interest in the results achieved by inquirers, and to do their share of the work requisite for the success of the system. I need hardly remind you that there are plenty of other strong reasons why some such knowledge of the truths of nature, and of the means by which they are found out, should be diffused as widely as possible throughout the community.

You perceive that in such educational system each teacher must trust to his own exertions for success and advancement; and he will do so if he is sure that his results will be known and compared impartially with those attained by others. Each governing body must duly maintain the efficiency of their school or college, if its support depend in some degree on the evidences of that efficiency; and they will try to improve their school if they know that every improvement will be seen and duly appreciated.

The keystone of the whole structure is the action of the State in distributing funds carefully among schools and colleges proportionally to the evidence of their doing good work, which could not be continued without such aid.

I am inclined to think that the State ought, as far as possible, to confine its educational grants to the purpose of maintaining and continuing good work which is actually being done, and rarely if ever to initiate educational experiments: first, because it is desirable to encourage private exertions and donations for the establishment of schools and colleges upon new systems, or in new localities, by giving the public full assurance that if any new institution establishes its right to existence, by doing good work for a while, it will not be allowed to die off for want of support; and, secondly, because the judicial impartiality required in the administration of public funds, on the basis of results of work, is hardly compatible with an advocacy of any particular means of attaining such results.

On the other hand, experience has shown that special endowments, which tie up funds in perpetuity for a definite purpose, commonly fail to attain their object under the altered circumstances which spring up in later generations, and not unfrequently detract from the efficiency of the institutions to which they are attached, by being used for objects other than those which it is their proper function to promote.

When there is felt to be a real want of any new institution for the promotion of learning, men are usually willing enough to devote time and money to the purpose of establishing it and giving it a fair trial. It is desirable that they should leave the State to judge of their experiment by its results, and to maintain it or not, according to the evidences of its usefulness. No institution ought, for its own sake, to have such permanent endowments as might deprive its members of motives for exertion.

The State could not, however, discharge these judicial functions without accurate and trustworthy evidence of the educational work done at the various schools and of its success. For this purpose a record must be kept by or under the direction of every teacher of the weekly progress of each pupil, showing what he has done and how he has done it. Official inspectors would have to see to these records being kept upon a uniform scale, so that their results might be comparable. The habit of keeping such records conduces powerfully to the efficiency of teachers; and, for the sake of the due development of the teaching system, it ought to prevail generally. Having such full and accurate means of knowing what opportunities of improvement pupils have enjoyed, and what use they have made of those opportunities, Government ought to stimulate their exertions and test their progress by periodical examinations. It is of the utmost importance to allow any new and improved system of instruction to develop itself freely, by the exertions of those who are willing to undertake the labour and risk of trying it on a practical scale; and the pupils who acquire upon such new system a command of any branch of science, ought to have a fair opportunity of showing what they have achieved and how they have achieved it. An able and impartial examiner, knowing the new systems in use, will encourage each candidate to work out his results in the manner in which he has been taught to work out results of the kind.

Examinations thus impartially conducted with a view of testing the success of teachers in the work which they are endeavouring to do, have a far higher value, and consequent authority, than those which are conducted in ignorance or disregard of the process of training to which the candidates have been subjected; and we may safely say that the examination system will not attain its full usefulness until it is thus worked in intimate connection with a system of teaching.

In order to give every one employed in the educational system the utmost interest in maintaining and increasing his efficiency, it is essential that a due measure of publicity be given to the chief results of their respective labours. Schools and colleges ought, to a considerable extent, to be supported by the fees paid by pupils for the instruction received; and every Professor being in part dependent upon the fees of his pupils will have a direct interest in attracting more pupils to his classes or laboratories. The fame of important original investigations of his own or his pupils, published in the scientific journals, is one of the natural means by which a distinguished Professor attracts disciples, and the success of his pupils in after life is another His prospects of promotion will depend mainly on the opinion formed of his powers from such materials as these by the governing bodies of colleges and by the public; for if each college is dependent for success upon the efficiency of its teaching staff, its governing body must do their best to fill up every vacancy as it arises by the appointment of the ablest and most successful Professor whom they can get; and any college which does not succeed in obtaining the services of able men will soon lose reputation, and fall off in numbers.

There are, however, further advantages to the working of the system to be derived from full publicity of all its more important proceedings. It will supply materials for the formation of a sound public opinion respecting the proceedings of the authorities in their various spheres of action. A claim for money might be made upon Government by the rulers of some college upon inadequate grounds; or a just and proper claim of the kind might be disregarded by Government. Neither of these things will be likely to happen very often if the applications, together with the evidence bearing on them, are open to public scrutiny and criticism; and when they do occasionally happen, there will be a natural remedy for them.

If I have succeeded in making clear to you the leading principles of the plan to be adopted for the advancement of science, including, as it necessarily must do, national education generally, you will, I think, agree with me that, from the very magnitude and variety of the interests involved in its action, such system must of necessity be under the supreme control of Government. Science will never take its proper place among the chief elements of national greatness and advancement until it is acknowledged as such by that embodiment of the national will which we call the Government. Nor can the various institutions for its advancement develop duly their usefulness until the chaos in which they are now plunged gives place to such order as it is the proper function of Government to establish and maintain.

But government has already taken, and is continuing to take action in various matters affecting elementary popular education and higher scientific education, and it would be difficult to arrest such action, even if it were thought desirable to do so. The only practical question to be considered is how the action of Government can be systematised so as to give free play to the natural forces which have to do the work.

By establishing official examinations for appointments and for degrees Government exerts a powerful influence on the teaching in schools and colleges, without taking cognizance, except in some few cases, of the systems of teaching which prevail in them. Again, they give grants of public money from time to time in aid of colleges or universities, or for the establishment of a high school under their own auspices. Sometimes they endow a Professorship. In taking each measure of the kind they are doubtless influenced by evidence that it is in itself a good thing, calculated to promote the advancement of learning. But a thing which is good in itself may produce evil effects in relation to others, or good effects incommensurate with its cost. Thus examinations afford most valuable aid to educational work when carried on in conjunction with earnest teachers; yet when established in the absence of a good system of education, they are liable to give rise to a one-sided training contrived with a special view of getting young men through the examinations. If no properly educated young men were found for a particular department of the public service, and an examination of all candidates for such appointments were to be established for the purpose of improving the system of training, candidates would consider their power of answering such questions as appeared likely to be set as the condition of their obtaining the appointments, and they would look out for men able and willing to train them to that particular work in as direct and effective a manner as possible. The demand for such instruction would soon be supplied. Some teachers would undertake to give instruction for the mere purpose of enabling candidates to get through the examination; and by the continued habit of such work would gradually come to look upon the examiners as malignant beings who keep youths out of office, and whose vigilance ought to be evaded by such means as experience might show to be most effective for the purpose. Once this kind of 'direct examination-teaching has taken root, and is known to produce the desired effect of getting young men through the examinations, its existence encourages the tendency on the part of the candidates to look merely to the examination as the end and aim of their study; and a class of teachers is developed whose exertions are essentially antagonistic to those of the examiners.

There are, no doubt, teachers with a sufficiently clear apprehension of their duty, and sufficient authority, to convince some of the candidates that the proper object of their study should be to increase their power of usefulness in the career for which they are preparing themselves, by thoroughly mastering up to a prescribed point certain branches of knowledge; and that until they had honestly taken the means to do this and believed they had done it effectually, they ought not to go up for examination nor to wish to commence their career.

But it is desirable that all teachers be placed in such circumstances that it may become their interest as well as their duty to co-operate to the utmost of their powers in the object for which the examiners are working. For this purpose their records of the work done under their guidance by each pupil ought to be carefully inspected by the examiners before framing their questions, and ought to be accepted as affording the chief evidence of the respective merits of the pupils.

This is not the place for considering how the general funds for an effective system of national education can best be raised, nor how existing educational endowments can best be used in aid of those funds. It is well known that some colleges of Oxford and Cambridge are possessed of rich endowments, and that many distinguished members of those universities are desirous that the annual proceeds of those endowments should be distributed upon some system better calculated to promote the advancement of learning than that which generally prevails. Indeed we may confidently hope that, true to their glorious traditions, those colleges will be led, by the high-minded and enlightened counsels of their members, to rely upon improving usefulness in the advancement of learning as the only secure and worthy basis of their action in the use of their funds, so that they may take a leading part in such system of national education as may be moulded out of the present chaos.

But the foundations of a national system of education ought to be laid independently of the present arrangements at Oxford and Cambridge, for we may be sure that the more progress the system makes the more easy will become the necessary reforms in the older universities and colleges.

It is clearly undesirable that Government should longer delay obtaining such full and accurate knowledge of the existing national resources for educational purposes, and of the manner in which they are respectively utilised, as may enable them to judge of the comparative prospects of usefulness presented by the various modes of distributing educational grants. They ought to know what has been done and what is doing in the various public educational establishments before they can judge which of them would be likely to make the best use of a grant of public money.

We have official authority for expecting such impartial administration of educational grants; and it cannot be doubted that, before long, due means will be taken to supply the preliminary conditions.

You are no doubt aware that a Royal Commission was appointed some time ago in consequence of representations made to Government by the British Association on this subject, and it is understood that their instructions are so framed as to direct their particular attention to the manner in which Government may distribute educational grants. The Commission is moreover composed of most distinguished men, and we have every reason to anticipate from their labours a result worthy of the nation and of the momentous occasion.

In speaking of public educational establishments, I refer to those which by their constitution are devoted to the advancement of learning without pecuniary profit to their respective governing bodies. The annual expenditure requisite for keeping up a national system of popular education will necessarily be considerable from the first, and will become greater from year to year; but once Englishmen are fully alive to the paramount importance of the subject, and see that its attainment is within their reach, we may be sure that its expense will be no impediment. England would not deserve to reap the glorious fruits of the harvest of knowledge if she grudged the necessary outlay for seed and tillage, were it even ten times greater than it will be. It is no use attempting to establish a national system on any other than a truly national basis. Private and corporate funds inevitably get diverted from popular use, after a few generations, to the use of the influential and rich. A national system must steadily keep in view the improvement of the poor, and distribute public funds each year in the manner best calculated to give to the youths of the poorest classes full opportunities of improvement proportional to their capacities, so that they may qualify themselves for the utmost usefulness to their country of which they are capable. The best possible security for the proper administration of the system will be found in the full and speedy publicity of all the particulars of its working.

It has been frequently remarked that a great proportion of English investigators are men of independent means, who not only seek no advancement as a reward of their labours, but often sacrifice those opportunities of improving their worldly position which their abilities and influence open up to them, for the sake of quietly advancing human knowledge. Rich and powerful men have very great temptations to turn away from science, so that those who devote their time and money to its service prove to us how true and pure a love of science exists in this country, and how Englishmen will cultivate it when it is in their power to do so.

Now and then a youth from the poorer classes is enabled by fortunate accidents and by the aid of a friendly hand to climb to a position of scientific activity, and to give us, as Faraday did, a sample of the intellectual powers which lie fallow in the great mass of the people.

Now, the practical conclusion to which I want to lead you is that it rests with you, who represent the national desire for the advancement of science, to take the only measures which can now be taken towards the establishment of a system of education worthy of this country, and adapted to the requirements of science. In the present stage of the business the first thing to be done is to arouse public attention by all practicable means to the importance of the want, and to get people gradually to agree to some definite and practicable plan of action. You will, I think, find that the best way to promote such agreement is to make people consider the natural forces which have to be systematised by legislation, with a view of enabling them to work freely for the desired purpose. When the conditions essential to any national system come to be duly appreciated by those interested in the cause of education, means will soon be found to carry out the necessary legislative enactments.

The highest offices in the State are on our present system filled by men who, whatever their political opinions and party ties, almost infallibly agree in their disinterested desire to signalise their respective terms of office by doing any good in their power. Convince them that a measure desired by the leaders of public opinion is in itself good and useful, and you are sure to carry it.

And, on the other hand, England is not wanting in men both able and willing to come forward as the champions of any great cause, and to devote their best powers to its service.

I may well say this at Bradford after the results achieved by your Member in the Elementary Education Act.

Objections will of course be raised to any system on the score of difficulty and expense, more especially to a complete and good system. Difficult of realisation it certainly must be, for it will need the devoted and indefatigable exertions of many an able and high-minded man for many a long year. Only show how such exertions can be made to produce great and abiding results, and they will not be wanting. And as for expense, you will surely agree with me that the more money is distributed in such frugal and effective manner, the better for the real greatness of our country.

What nobler privilege is attached to the possession of money than that of doing good to our fellow men? and who would grudge giving freely from his surplus, or even depriving himself of some comforts, for the sake of preparing the rising generation for a life of the utmost usefulness and consequent happiness?

I confidently trust that the time will come when the chief item in the annual budget of the Chancellor of the Exchequer will be the vote for National Education; and when in some later age our nation shall have passed away, when a more true civilisation has grown up and has formed new centres for its throbbing life, when there are but broken arches to tell of our bridges and crumbling ruins to mark the sites of our great cathedrals— then will the greatest and noblest of England's works stand more perfect and more beautiful than ever; then will some man survey the results of Old England's labours in the discovery of imperishable truths and laws of nature, and see that her energy and wealth were accompanied by some nobler attributes—that while Englishmen were strong and ambitious enough to grasp power, they were true enough to use it for its only worthy purpose—that of doing good to others.

I must not, however, trespass longer upon your time and your kind attention. My subject would carry me on, yet I must stop without having done half justice to it.

If I have succeeded in convincing you that a National system of Education is now necessary and possible, and in persuading you to do what you respectively can to prepare the way for it, I shall feel that the first step is made towards that great result.

SECTIONAL PROCEEDINGS

SECTION B.—CHEMICAL SCIENCE.

ADDRESS OF THE PRESIDENT, W. J. RUSSELL, F.R.S.

OF late years it has been the custom of my predecessors in this chair to open the business of the section with an address, and the subject of this address has almost invariably been a review of the progress of chemistry during the past year. I purpose, with your leave, to-day to deviate somewhat from this precedent, and to limit my remarks, as far as the progress of chemistry is concerned, to the history of one chemical substance. The interest and the use of an annual survey, at these meetings, of the progress of chemistry, has to a certain extent passed away, for the admirable extracts of all important chemical papers, now published by the Chemical Society, has in a great measure taken its place, and offers to the chemical student a much more thorough means of learning what progress his science is making than could possibly be done by the study of a presidential address. Doubtless these abstracts of chemical papers are known to others than professional chemists, but I cannot pass them over without recording the great use they have proved to be, how much they have done already in extending in this country an exact knowledge of the progress of science on the continent, and in helping and in stimulating those who are engaged in scientific pursuits in this country. I believe few grants made by this Association have done more real good than those which have enabled the Chemical Society to publish these abstracts.

I dwell for a moment on the doings of the Chemical Society, for I believe in the progress of this Society we have a most important indication of the progress of chemical science in this country. The number of original papers communicated to the Society during the past year has far exceeded that of previous years: during last year fifty-eight papers were read to the Society, whereas the average number for the last three years is only twenty-nine. Further, I may say, there is every appearance of this increased activity not only continuing, but even increasing. Another matter connected with the Society deserves a passing word, I mean its removal from its old rooms at Burlington House, which afforded it very insufficient accommodation, to its new ones in the same building. This transference which is now taking place, will give to the Society a great increase of accommodation, and thus admit of larger audiences attending the lectures, of the proper development of the library, and of the full illustration by experiment of the communications made to it. These improvements must act most beneficially on the Society, and stimulate its future development; even now it numbers some 700 members, and certainly is not one of the least active or least useful of the many scientific societies in London. Since our last meeting, at Brighton, we have lost the most renowned of modern chemists—Liebig. His influence on chemistry through a long and most active life has yet to be written. Publishing his first paper fifty years ago, it is difficult for chemists of the present day to realise the changes in chemical thought, in chemical knowledge, and in chemical experiments which he lived through, and was more than any other chemist active in promoting. His activity was unwearied: he communicated no less than 317 papers to different scientific journals, and almost every branch of chemistry received some impetus from his hand.

Liebig took an active interest in this Association, and I believe the last paper he wrote was one in answer to a communication made at the last meeting of this Association. On two occasions he attended the meetings of the British Association, and has communicated many papers to this section. The meeting at Liverpool in 1837 was the first at which he was present; he there communicated to this section a paper on the products of the decomposition of uric acid, and further gave an account of his most important discovery, made in conjunction with Wohler, of the artificial formation of urea. At this meeting Liebig was requested to prepare a report on the state of our knowledge of isomeric bodies. This request, although often repeated, was never complied with. He was also requested to report on the state of organic chemistry and organic analysis—thus our section was evidently desirous of giving him full occupation. At the meeting in 1840 at Glasgow, a paper on "Poisons, Contagions, and Miasms," by Liebig was read; it was in fact an abstract of the last chapter in his book on "Chemistry in its applications to Agriculture and Physiology," and the work itself appeared about the same time, dedicated to this Association. Liebig says:—"At one of the meetings of the Chemical Section of the British Association for the advancement of Science, the honourable task of preparing a report upon the state of Organic Chemistry was imposed upon me. In this present work I present the Association with a part of this report" At the next meeting, which was held at Plymouth, in 1841, there was an interesting letter from Liebig to Dr. Playfair read to our section; in it, among other matters, Liebig describes an "excellent method" devised by Drs. Will and Varrentrapp for determining the amount of nitrogen in organic bodies; he also says we have repeated all the expressions of Dr. Brown on the production of silicon from paracyanogen, but we have not been able to confirm one of his results; what our experiences prove is that paracyanogen is decomposed by a strong heat into nitrogen gas, and a residue of carbon which is exceedingly difficult of combustion.

To the next meeting—it was at Manchester, and Dalton was the president of this section—Dr. Playfair communicated an abstract of Professor Liebig's report "On Organic Chemistry applied to Physiology and Pathology." This abstract is printed in our proceedings, and the complete work is looked upon as the second part of the report on Organic Chemistry. This Association may therefore fairly consider that it exercised some influence on Liebig in the production of the most important works that he wrote. Playfair's abstract must have been listened to with the greatest interest, and I doubt not the statements made sharply criticised, specially by the physiologists then at Manchester. Playfair concludes his abstract with these words, thus summing up the special objects of these reports:—"In the opinion of all, Liebig may be considered a benefactor to his species, for the interesting discoveries in agriculture, published by him in the first part of his report. And having in that pointed out means by which the food of the human race may be increased, in the work now before us he follows up the chain in its continuation, and shows how that food may be best adapted to the nutrition of man. Surely there are no two subjects more fitted than these for the contemplation of the philosopher; and by the consummate sagacity with which Liebig has applied to their elucidation the powers of his mind, we are compelled to admit that there is no living philosopher to whom the Chemical Section could have more appropriately entrusted their investigation."

At the meeting at Glasgow, in 1855, Liebig was also present, but then only communicated to this section a short paper on fulminuric acid, and some remarks on the use of lime water in the manufacture of bread. Such I believe is the history of the direct relationship which has existed between Liebig and this Association. Indirectly we can hardly recognise how much we owe to him. Interested as he was in the work of this Association I could not but to-day record the instances of direct aid and support which this section has received from him.

I pass on now to the special subject to which I wish to ask your attention.

It is the history of the vegetable colouring matter found in madder. It has been in use from time immemorial, and is still one of the commonest and most important of dyes. It is obtained from a plant largely cultivated in many parts of the world for the sake of the colour it yields, and the special interest which now attaches to it is, that the chemist has lately shown how this natural colouring matter can be made in the laboratory as well as in the fields; how by using a bye-product, which formerly was without value, thousands of acres can be liberated for the cultivation of other crops, and the colouring matter which they formerly produced be cheaper and better prepared in the laboratory or in the manufactory. That a certain colouring matter could be obtained from the roots of the *Rubia tinctorum*, and other species of the same plant, has been so long known, that apparently no record of its discovery remains.

Pliny and Dioscorides evidently allude to it. The former, referring to its value as a dyeing material, says, "It is a plant little known except to the sordid and avaricious, and this because of the large profits obtained from it, owing to its employment in dyeing wool and leather." He further says, "The madder of Italy is the most esteemed, and especially that grown in the neighbourhood of Rome, where and in other places it is produced in great abundance." He further describes it as being grown among the olive-trees, or in fields devoted especially to its growth. The madder of Ravenna, according to Dioscorides, was the most esteemed. Its cultivation in Italy has been continued till the present time, and in 1863 the Neapolitan provinces alone exported it to the value of more than a quarter of a million sterling. At the present day we are all very familiar with this colouring matter as the commonest that is applied to calicoes. It is capable of yielding many colours, such as red, pink, purple, chocolate, and black. The plant in which is the source of this colouring matter is nearly allied botanically and in appearance to the ordinary Galiums, or bed-straws. It is a native probably of Southern Europe as well as Asia. It is a perennial with herbaceous stem, which dies down every year; its square-jointed stalk creeps along the ground to a considerable distance, and the stem and leaves are rough with sharp prickles. The root, which is cylindrical, fleshy, and of a pale yellow colour, extends downwards to a considerable depth. It is from this root, which, when dried, is known as madder, that the colouring matter is obtained. The plant is propagated from suckers or shoots. These require some two or three years to come to full maturity and yield the finest colours, although in France the crop is often gathered after only eighteen months' growth. From its taking so long to develop, it is evidently a crop not adapted to any ordinary series of rotation of crops. The plant thrives best in a warm climate, but has been grown in this country and in the north of Europe.

In India it has been grown from the earliest times, and, as before stated, has been abundantly cultivated in Italy, certainly since the time of Pliny; he also mentions its cultivation in Galilee. In this country its culture has often been attempted, and has been carried on for a short time, but never with permanent success. The madder now used in England is imported from France, Italy, Holland, South Germany, Turkey, and India. In 1857 the total amount imported into this country was 434,056 cwts., having an estimated value of 1,284,989*l.*, and the average annual amount imported during the last seventeen years is 310,042 cwts.; while the amount imported last year, 1872, was 283,274 cwts., valued at 922,244*l.* In 1861, it was estimated that in the South Lancashire district alone, 150 tons of madder were used weekly, exclusive of that required for preparing garancine. I quote these figures as showing the magnitude of the industry that we are dealing with. Another point of much interest is the amount of land required for the cultivation of this plant. In England it was found that an acre yielded only from 10 to 20 cwt. of the dried roots, but in South Germany and in France the same amount of land yields about twice that quantity. The madder cultivator digs up the roots in autumn, dries them, in some cases peels them, by beating them with a flail, and exports them in the form of powder, whole root, or, after treatment with sulphuric acid, when it is known as garancine.

The quality of the root varies much, that from the Levant, known as Turkey root, is most valued. According, however, to the colour to be produced, is the madder from one source or another preferred.

To obtain the colouring matter, which is but very slightly soluble in water, from these roots, they are mixed, after being ground, with water in the dye-vessel, and sometimes a little chalk is added. The fabric to be dyed is introduced, and the whole slowly heated; the colouring matter gradually passes from the root to the water, and from the water to the mordanted fabric, giving to it a colour dependent of course on the nature of the mordant.

To trace the chemical history of this colouring matter, we have to go back to the year 1790, when a chemist of the name of Watt precipitated the colouring matter of madder by alum from neutral, alkaline, and acid solutions: he obtained two different colouring matters, but could not isolate them, and many different shades of colours. Charles Batholdi asserted that madder contained much magnesic sulphate, and Hautmann observed the good effect produced on madder by the addition of calcic carbonate. In 1823, F. Kuhlmann made evidently a careful analysis of the madder-root, and describes a red and a fawn colouring matter; but the first really important advance made in our knowledge of the chemical constitution of this colouring matter was by Colin and Robiquet in 1827. They obtained what they believed to be, and what has since really proved to be, the true colouring principle of madder, and obtained it in a state of tolerable purity. Their process for preparing it was very simple. They took Alsace madder in powder, digested it with water, obtaining thus a gelatinous mass, which they treated with boiling alcohol, then evaporated off four-fifths of the alcohol, and treated the residue with a little sulphuric acid, to diminish its solubility. Then, after washing it with several litres of water, they got a yellowish substance remaining. Lastly, they found that on moderately heating this product in a glass tube, they obtained a yellowish vapour formed of brilliant particles, which condensed, giving a distinct zone of brilliant needles, reflecting a colour similar to that from the native lead chromate. They named this substance alizarine, from the Levant name for madder, Alizari, the name by which it is still known there.

A few years later we find other chemists attacking this same subject; in 1831 Gaultier de Clanbry and J. Persoz published the account of a long research on the subject; they described two colouring matters, a red and a rose one—the red one was alizarine and the rose one was another body nearly allied to it, and now well known as purpurine. Runge also made an elaborate examination of the madder root; he found no less than five different colouring matters in it—madder-red, madder-purple, madder-orange, madder-yellow, and madder-brown. The first three he considers to be suited for dyeing purposes, but not the last two.

Runge's madder-red is essentially impure alizarin, and his madder-purple impure purpurine. He does not give any analysis of these substances. During the next ten years this subject seems to have attracted but little attention from chemists, but in 1846 Shiel prepared the madder-red and madder-purple of Runge, by processes very similar to those employed by Runge, and analysed these substances. For madder-red he gives the formula $C_{28}H_{18}O_{9}$, which differs only by H_2O from the formula now adopted. For the madder-purple he gives the formula $C_{28}H_{20}O_{15}$, and for the same substance, after being sublimed, $C_7H_8O_4$. The chemist who has worked most on this subject, and to whom we are principally indebted for what we know with regard to the different constituents contained in the madder root, is Dr. Schunk of Manchester. In Liebig's Annalen for 1848 he gives a long and interesting account of his examination of madder; he isolated and identified several new substances which are most important constituents of the root, and has since this time added much to our knowledge of the chemical constitution of madder. In the paper above alluded to he confirms the presence of the alizarine, and gives to it the formula $C_{14}H_{10}O_4$. The principal properties of this body may best be sketched here. Its volatility and brilliant crystalline appearance have already been mentioned; it is but slightly soluble in cold water, but much more so in alcohol, in ether, and in boiling-water. The colour of its solution is yellow, and when it separates out from a liquid it has a yellow flocculent appearance, differing thus greatly from the red brilliant crystalline substance before described. In order to obtain this latter body heat had always been used, so until the elaborate experiments of Schunk it was a question whether the heat did not produce a radical change in the substance, whether, in a word, these two bodies were really identical. Schunk's experiments proved that they were, and consequently that this beautiful colouring matter alizarine existed as such in madder. If, however, we go one step further back and examine the fresh root of *Rubia tinctorum*, that is, as soon as it is drawn from the ground, for some time we shall find no trace of alizarine there. On slicing the root it is seen to be of a light carroty colour, and an almost colourless liquid can be squeezed out of it, but this is entirely free from the colouring matters of madder. Let the roots, however, be kept if only for a short time, and then they will give abundant evidence of the presence of alizarine; if simply heated alizarin may be volatilised from them. It appears then that the whole of the tinctorial power of this root is developed after the death of the plant. Schunk explains this curious phenomenon as follows:—That in the cells of the living plant there is a substance which he has isolated, and has named rubian; it is easily soluble in water and in alcohol; the solution is of a yellow colour, and has an intensely bitter taste; when dry it is a hard, brown, gum-like body. It has none of the properties of a dye stuff, but if we take a solution of it, add some sulphuric or hydrochloric acid to it, and boil, a yellow flocculent substance will slowly separate out, and on filtering it off and washing it, it will be found to have the tinctorial properties of madder and to contain alizarine. In the liquid filtered from it there is, with the acid added, an uncrystallisable sugar, so that in this way the original product in the root, the rubian, has apparently been split up into alizarine and into sugar. To apply this reaction to what goes on in the root after its removal from the ground, we have to find if any other substances can take the place of the boiling dilute acid, and Schunk has shown there exists in the root itself a substance which is eminently fitted to produce this splitting up of the rubian. He obtained this decomposing agent from madder simply by digesting it in cold water, and then adding alcohol to the liquid; this threw down a reddish flocculent substance, and if only a small portion of this be added to an aqueous solution of rubian and allowed to stand for a few hours in a warm place, it was found that the rubian was gone, and in place of it there was a thick tenaceous jelly; this, treated with cold water, gave to it no colour, no bitter taste, but much sugar. From the jelly, remaining insoluble, alizarine could be extracted. In fact, of all known substances this very one found in the madder itself is best suited for effecting this decomposition of the rubian.

It appears, then, that these two bodies must exist in the root. The history then seems complete. The two substances are kept apart during the life of the plant in some way of which we know nothing, but as soon as it dies they begin slowly to act on one another, developing thus the colouring matters in madder. It has long been known to dyers that the amount of colouring matter in madder will increase on keeping it; even for years it will go on improving in quality, and an experiment of Schunk's shows that the ordinary madder as used by the dyer has not all the rubian converted into colouring matter, for on taking a sample of it and extracting it with cold water he got an acid solution devoid of dyeing properties, but on allowing this solution to stand some time it gelatinised and then possessed dyeing properties.

Coincident with the appearance of Schunk's first paper was one by Debus on the same subject. He looked upon alizarine as a true acid, and gave it the name of Lizaric acid, but as far as the composition of it was concerned the percentage which he obtained agreed closely with those given by Schunk. One other investigation concludes all that is important in the history of alizarine as obtained from madder. This last research is of great interest; it was by Julius Wolff and Adolph Strecker, and published in 1850 They confirm the results of others so far that there are in the madder root two distinct colouring substances—this important one alizarin, and the other one purpurine. They prepare these colouring matters much in the same way that Schunk did, and very carefully purify and analyse them; the formulæ which they give for them differ, however, from Schunk's: for alizarine they give the formula $C_{20}H_{12}O_6$ and for purpurine $C_{18}H_{12}O_6$. Further, they suggest that by the process of fermentation the former is converted into the latter, and they show that by oxidation they both yielded phthalic acid. Since the publication of this research, until the last year or two, this formula for alizarine has been generally adopted by chemists, and in most modern books we find it given as expressing the true composition of that body. It was not only the careful and elaborate work which they devoted to the subject, but also the ingenious and apparently well-founded theory on the subject which carried conviction with it. Laurent had shown, not many years before, that when naphthalin, that beautiful white crystalline substance obtained from coal tar, was acted on by chlorine, and then treated with nitric acid, a body known as chlornaphthalic acid and having the composition $C_{20}H_{10}Cl_2O_6$ was obtained, and on comparing this formula with the one they had obtained for alizarine, Wolff and Strecker at once concluded that it really was alizarine, only containing two atoms of chlorine in place of two of hydrogen; make this replacement, an operation generally easily performed, and from naphthalin, they had prepared alizarine. Further, this relationship between chlornaphthalic acid, and alizarine is borne out in many ways; it, like alizarine, has the power of combining with different basic substances, has a yellow colour, is insoluble in water, melts at about the same temperature, is volatile, and when acted on by alkalis gives strongly coloured solutions. Taking then all these facts into consideration, can we wonder that these chemists feel convinced that they have established the composition of alizarine, and have shown the source from which it is to be obtained artificially? Apparently but one very simple step remains to crown their work with success, that of replacing the chlorine by hydrogen. Melsens had only shortly before shown how this substitution could easily be made in the case of chloracetic acid by acting on it with potassium amalgam, and Kolbe had used the battery for the same purpose. Both these processes, and doubtless all others that the authors can think of, are tried upon the chloronaphthalic acid, but chloronaphthalic acid it remains, and they are obliged to confess they are unable to make this substitution. Still they are strong in the belief that it is to be done and will be done, and conclude the account of their researches by pointing out the great technical advantage it will be to get alizarine from a worthless substance such as naphthalin. One cannot help even now sympathising with these chemists in their not being able to confirm what they had really the strongest evidence for believing must prove to be a great discovery. We now know, however, that had they succeeded in effecting this substitution, or had they in any other way obtained this chloronaphthalic acid without the chlorine, if I may so speak of it, which since their time has been done by Martius and Griess, alizarine would not have been obtained, but a body having a remarkable parallelism in properties to it would have been This body, like alizarine, is of a yellowish colour, but slightly soluble in water, easily in alcohol and in ether, is volatile, and on oxidation yields the same products, it is, in fact, an analogous body, but belonging to another group. We also know that the formula proposed by Wolff and Strecker, and so long in use, is not the correct one. But little more remains to be added with regard to the history of alizarine as gathered from the study of the natural substance. Schutzenberger and Paraf suggested doubling Wolff and Strecker's formula for alizarine, and Bolley suggested the formula $C_{28}H_{11}O_6$, which owing to the uneven number of hydrogen atoms was soon rejected. If we compare our present knowledge of alizarine with what it was when these researches on the natural product were completed, it is as lightness compared to darkness, and we may well ask whence has come this influx of knowledge? the answer I hope to show you is undoubtedly that it has come from the careful and accurate study of abstract chemistry. I know of no history in the whole of chemistry which more strikingly illustrates how the prosecution of abstract science lays the foundation for great practical improvements. My object now, is then to show you, as shortly as I can, how by indirect means the composition of alizarine was discovered, how it has been built up artificially, and how it is superseding for manufacturing purposes the long-used natural product.

To trace this history from its source we must go back to 1785, when an apothecary of the name of Hofmann obtained the calcium salt of an acid called quinic acid from cinchona bark. This acid is now known to be of common occurrence in plants, it exists in the bilberry and in coffee, in holly, ivy, oak, elm, and ash leaves, and probably many others. Liebig also prepared the calcium salt, and was the first to give a complete analysis of it; the formula he gave for it was $C_{15}H_{24}O_{12}$. Baup on repeating Liebig's experiments arrived at a somewhat different conclusion, and gave the formula $C_{15}H_{24}O_{10}$. In 1835 at Liebig's suggestion, to determine which formula was correct, Alexander Woskrensky, from St. Petersburg, then a student at Giessen, undertook the further investigation of this subject, and established the formula $C_{14}H_{24}O_{12}$ the one in fact now in use. In the course of this investigation, which he carried further than merely settling the percentage composition of this acid, he describes what to us now is of most interest, a new substance having peculiar and very marked properties. He says that when a salt of quinic acid is burnt at a gentle heat he gets aqueous vapour, the vapour of formic acid, and a deposit of golden needles which are easily sublimed. Afterwards he describes how this same golden substance may be obtained from any salt of quinic acid by heating it with manganic dioxide and dilute sulphuric acid; it then distils over, condensing in golden yellow needles on the sides of the receiver, and may be rendered pure by resublimation. The composition of this body he finds to be C_3H_2O, and names it quinoyl, a name strongly objected to by Benzelius, as conveying a wrong impression of the nature of the body; he proposes in place of it the name quinone, by which it is still known. Far as this body would seem to be removed from alizarine, yet is the study of its properties which led to the artificial production of alizarine.

Some years afterwards Wöhler also explained them by the decomposition of quinic acid; he prepares again this quinone and follows exactly the process described by Woskrensky. He states that with regard to the properties of this remarkable body he has nothing particular to add. However, he proposes a different formula for it, and discovers and describes other bodies allied to it. Among these is Hydroquinone $C_6H_6O_2$. Laurent afterwards shows that the formula proposed by Wöhler is inconsistent with his and Gerhard's views, and by experiment confirms the former formula for this body. Although many other chemists devoted much attention to this substance, still its real constitution and relation to other compounds remained unknown.

Thus Wöhler, Laurent, Hofmann, Städler, and Hesse, all had worked at it, and much experimental knowledge with regard to it had been acquired. One important point in its history was first the discovery of chloranil by Erdmann in 1841, and then Hofmann, showing that by heating quinone with potassic chlorate and hydrochloric acid chloranil could be obtained from it; that, in fact, chloranil was quinone in which all the hydrogen had been replaced by chlorine. Perhaps the most general impression among chemists was that in constitution it was a kind of aldehyde, certainly its definite place among chemical compounds was unknown.

Kekulé suggests a rational formula for it, but it is to Carl Graebe that we owe our knowledge of its true constitution. In 1868 he published a remarkable and very able paper on the quinone group of compounds, and then first brought forward the view that quinone was a substitution derivative of the hydrocarbon benzol (C_6H_6). On comparing the compounds of these two bodies it is seen that the quinone contains two atoms of oxygen more and two atoms of hydrogen less than benzol, and Graebe, from the study of the decomposition of the quinone, and from the compounds it forms, suggested that the two atoms of oxygen form in themselves a group which is divalent, and thus replace the two atoms of hydrogen. This supposition he very forcibly advocates and shows its simple and satisfactory application to all the then known reactions of this body. This suggestion really proved to be the key, not only to the explanation of the natural constitution of quinone and its derivatives, but to much important discovery besides. At this time quinone seemed to stand alone, no other similarly constituted body was known to exist; but what strikingly confirms the correctness of Graebe's views, and indicates their great value, is that immediately he is able to apply his lately gained knowledge, and to show how other really analogous bodies, other quinones in fact, already exist. He studied with great care this quinone series of compounds and the relation they bore to one another, the relation the hydrocarbon, benzole, bore to its oxidised derivative, quinone, and its relation to the chlorine substitution products derivable from it. At once this seems to have led Graebe to the conclusion, that another such series already existed ready formed, and that its members were well known to chemists, that in fact naphthalin ($C_{10}H_8$) was the parent hydrocarbon and that the chloroxynaphthalin chloride ($C_{10}H_4Cl_2O_2$) and the perchloroxynaphthalin chloride ($C_{10}Cl_6O_2$) were really chlorine substitution compounds of the quinone of this series, corresponding to the bichloroquinone and to chloranil. That the chloroxynaphthalic acid $C_{10}H_4Cl(HO)O_2$ and the perchloroxynaphthalic acid $C_{10}Cl_5(HO)O_2$, all compounds previously discovered by Laurent, were really bodies belonging to that series, and further the supposed isomeric of alizarin discovered by Martius and Griess was really related to this last compound, having the composition $C_{10}H_5(HO)O_2$. Further he was able to confirm this by obtaining the quinone itself of this series, the body having the formula $C_{10}H_6(O_2)$ containing also two atoms less of hydrogen, and two atoms more of oxygen than the hydrocarbon naphthalin, and to the body he gave the characteristic name of naphthoquinone. The chlorine compounds just named are thus chloro-naphthoquinone, or chloroxynaphthoquinone, and correspond to the former chloroquinones, Martius and Griess compound will be an oxynaphthoquinone; many other compositions of this series are also known. Another step confirmatory of this existence of a series of quinones was made by Graebe and Borgmann, as the chloranil could be formed by treating phenol by potassic chlorate and hydrochloric acid and quinone derived from it, they showed that in the next higher series to the phenol series, viz. with cressol, the same reaction held good, and by treating it in the same way they obtain a di- and a tri-chlorotolu-quinone $C_7\begin{cases}CH_3\\(O_2)''\\Cl_2\\H\end{cases}$ $C_7\begin{cases}CH_3\\(O_2)''\\Cl_3\end{cases}$ which in physical properties very closely resemble the corresponding compounds in the lower series. Other compounds have also been prepared. In the next step we have the application, which connects these series of discoveries with alizarine. Following the clue of a certain analogy which they believed to exist between the chloranilic acid $C_6Cl_2\begin{matrix}(O_2)''\\(HO)_2\end{matrix}$ and the chloroxynaphthalic acid $C_{10}H_4Cl\begin{matrix}(O)_2\\(HO)\end{matrix}$ which they had proved to be quinone compounds and alizarine, believing that a certain similarity of properties indicated a certain similarity of constitution, Graebe and Liebermann were lead to suppose that alizarine must also be a derivative from a quinone, and have the formula $C_{14}H_4\begin{matrix}(O_2)\\(HO_2)\end{matrix}$ This theory they were able afterward to prove; the first thing was to find the hydrocarbon from which the quinone might be derived; this was done by taking alizarine itself, and heating it with a very large excess of zinc powder in a long tube, sealed at one end. A product distilled over, and condensed in the cool part of the tube, and collecting it and purifying it by recrystallisation, they found they had not a new substance, but a hydrocarbon discovered as long ago as 1832 by Dumas and Laurent, and obtained by them from tar. They had given it the formula $C_{15}H_{12}$, and as apparently it thus contained one and a half times as many atoms of carbon and hydrogen as naphthalin did, they named it Paranaphthalin; afterwards Laurent changed its name to Anthracene, by which it is still known. Fritzsche, in 1857, probably obtained the same body, but gave it the formula $C_{14}H_{10}$. Anderson also met with it in his researches, established its composition and found some derivatives from it. Limprich in 1866 showed it could be formed synthetically by heating benzolchloride (C_7H_7Cl) with water and Berthelot has since proved that it is formed by the action of heat on many hydrocarbons. This first step was thus complete and most satisfactory; from alizarin they had obtained its hydrocarbon, and this hydrocarbon was a body already known, and with such marked properties that it was easy to identify it. But would the next requirement be fulfilled, would it like benzol and naphthalin yield a quinone? The experiment had not to be tried, for when they found that anthracene was the hydrocarbon found, they recognised in a body already known to exist, the quinone derivable from it. It had been prepared by Laurent by the action of nitric acid on anthracene, and called by him anthraceneuse, and the same substance was also discovered by Anderson and called by him oxanthracene. The composition of this body was proved by Anderson and Laurent to be $C_{14}H_8O_2$, and it thus bears the same relation to its hydrocarbon anthracene, that quinone and naphthaquinone do to their hydrocarbons. Graebe gave to it the systematic name of anthraquinone. We have then, now, three hydrocarbons C_6H_6, $C_{10}H_8$, and $C_{14}H_{10}$, differing by C_4H_2, and all forming starting points for these different quinone series. Anthroquinone acted upon by chlorine gave substitution products such as might have been foretold. It is an exceedingly stable compound, not attacked even by fusion with potassic hydrate. Bromine does not act upon it in the cold, but at 100° it forms a bibromanthraquinone. Other bromine compounds have also been found. Now, if the analogies which have guided them so far still hold good, they would seem to have the means of forming alizarine artificially. Their theory is that it is dioxyanthraquinone $C_{14} H_6 \begin{smallmatrix}(O_2)'' \\ (HO)_2\end{smallmatrix}$ and if so, judging from what is known to take place with other quinone derivatives it should be formed from this dibromoanthraquinone on boiling it with potash or soda and then acidulating the solution. They try the experiment, and describe how, contrary at first to their expectations, on boiling the dibromanthraquinone with potash no change occurred, but afterwards, on using stronger potash and a higher temperature, they had the satisfaction of seeing the liquid little by little become of a violet colour; this shows the formation of alizarine. Afterwards, on acidifying this solution, the alizarine separated out in yellowish flocks. On volatalising it they got it in crystals, like those obtained from madder. On oxidising it with nitric acid, they get phthalic acid; and on precipitating it with the ordinary mordants or other metallic solutions, they get compounds exactly comparable to those from the natural product. Every trial confirms their success, so by following firmly theoretical considerations, they have been led to the discovery of the means of artificially forming this important organic colouring matter. A special interest must always attach itself to this discovery, for it is the first instance in which a natural organic colouring matter has been built up by artificial means; now the chemist can compete with Nature in its production. Although the first, it is a safe prediction that it will not long be the only one; which colouring matter will follow next it is impossible to say, but sooner or later that most interesting one, scientifically and practically, indigo will have to yield to the scientific chemist the history of its production. Returning for a moment to the percentage composition of alizarine, now that we know its constitution, its formula is established, and on comparing it ($C_{14} H_8 O_4$) with all the different formulæ which have been proposed, we see that the one advocated by Schunk was most nearly correct, in fact that it differs from it only by two atoms of hydrogen. It is not without interest to note that the next most important colouring matter in madder. Purpurine, which so pertinaciously follows alizarine, is in constitution very nearly allied to it, and is also an anthraecne derivative.

Scientifically then the artificial production of this natural product was complete, but the practical question, can it be made in the laboratory cheaper than it can be obtained from the root, had yet to be dealt with. The raw material, the anthracene, a bye-product in the manufacture of coal gas, had as yet only been obtained as a chemical curiosity; it had no market value, its cost would depend on the labour of separating it from the tar, and the amount obtainable. But with regard to the bromine necessary to form the bibromantharquinone it was different; the use of such an expensive re-agent would preclude the process becoming a manufacturing one. But could no cheaper re-agent be used in place of the bromine, and thus crown this discovery by utilising it as a manufacturing process? It was our countryman, Mr. Perkin, who first showed how this could be done, and has since proved the very practical and important nature of his discovery by carrying it out on the manufacturing scale. The nature of Perkin's discovery was the forming in place of a bibromanthraquinone, a disulphoanthraquinone, in a word he used sulphuric acid in place of bromine, obtaining thus a sulpho acid in place of a bromine substitution compound. The properties of these sulpho acids, containing the monovalent groups $H SO_3$ which is the equivalent to the atom of bromine, is that on being boiled with an alkali they are decomposed, and a corresponding alkaline salt formed; thus the change from the anthraquinone to the alizarine was effected by boiling it with sulphuric acid. At a high temperature, it dissolves, becoming a sulpho acid $C_{14} H_6 \left\{\begin{matrix}(O_2)'' \\ HSO_3 \\ HSO_3\end{matrix}\right.$ and the further changes follow, as they did with the bromine compound: the sulphuric acid boiled with potash is decomposed, and a potash salt of alizarin and potassic sulphite are formed; acid then precipitates the alizarin as a bright yellow substance. While Perkin was carrying on these researches in this country, Caro, Graebe, Liebermann, were carrying on somewhat similar ones in Germany; and in both countries have the scientific experiments developed into manufacturing industries. My knowledge extends only to the English manufactory, and if any excuse be necessary for having asked your attention to-day to this long history of a single substance, I think I must plead the existence of that manufactory as my excuse, for it is not often that purely scientific research so rapidly culminates in great practical undertakings. Already has the artificial become a most formidable opponent to the natural product; and in this struggle already begun there can be no doubt which will come off victorious. In the manufactory is rigidly carried out, the exact process I have already described to you. In tar there is about 1 per cent. of the anthracene; this, in a crude, impure state, is obtained from it by the tar-distiller, and sent by him to the colour works; here it is purified by pressure by dissolving from it many of its impurities, and lastly by volatalising it. Then comes the conversion of it into the anthraquinone by oxidising agents, nitric or chromic acid being used. Then the formation of the sulphur compound by heating it with sulphuric acid to a temperature of about 260° C. The excess of acid present is then neutralised by the addition of lime, and the insoluble calcic sulphate is filtered off; to the filtered liquid sodic carbonate is added, and thus the calcic salt of the sulpho acid is changed into the sodic salt $C_{14} H_6 \left\{\begin{matrix}(O_2)'' \\ Na SO_3 \\ Na SO_3\end{matrix}\right.$ This is afterwards heated to about 180° C. with caustic soda, thus decomposing the sulphuric acid and forming the soda salt of alizarin; and sodic sulphite; the alizarine salt so formed, remains in solution, giving to the liquid a beautiful violet colour; from this solution sulphuric acid precipitates the alizarine as an orange yellow substance. It is allowed to settle in large tanks, and then is run in the form of a yellowish sand, which contains either 10 or 15 per cent. of dry alizarine; into barrels, and is in this form sent to the print works, and used much in the same way as the original ground madder was used.

This alizarine mud, as I have called it, containing but 10 per cent. of dry alizarine is equal in dyeing power to about eight times its weight of the best madder, and is the pure substance required for the dyeing in place of a complicated mixture containing certain constituents which have a positively injurious effect on the colours produced.

The scientific knowledge and energy which Mr. Perkin has brought to bear on the manufacture of this colouring matter, seems already to have worked wonders, the supply and demand for artificial alizarine are increasing at a most rapid rate, and yet the manufacture of it seems hardly to have commenced. The value of madder has much decreased, and in fact, judging by what occurred in the year of revolution and commercial depression, 1848, when the price of madder fell for a time to a point at which it was considered it would no longer remunerate the growers to produce it, that point has now been again reached, but certainly from very different reasons. Last year artificial alizarine, equal in value to about one-quarter of the madder imported into England, was manufactured in this country. This year the amount will be much larger. Thus is growing up a great industry, which far and wide must exercise most important effects; old and cumbersome processes must give way to better, cheaper, newer ones, and lastly thousands of acres of land in many different parts of the world will be relieved from the necessity of growing madder and be ready to receive some new crop. In this sense may the theoretical chemist be said even to have increased the boundaries of the globe.

SECTION C.—GEOLOGICAL SECTION

ADDRESS OF THE PRESIDENT, JOHN PHILLIPS, F.R.S.

MORE than half the life of an octogenarian separates us from the birthday of the British Association in Yorkshire; and few of those who then helped to inaugurate a new scientific power can be here to-day to estimate the work which it accomplished, and judge of the plans which it proposes to follow in future. Would that we might still have with us the wise leading of Harcourt, and the intrepid advocacy of Sedgwick, names dear to Geology and always to be honoured in Yorkshire!

The natural sciences in general, and Geology in particular, have derived from the British Association some at least of the advantages so boldly claimed at its origin: some impediments have been removed from their path; society looks with approbation on their efforts; their progress is hailed among national triumphs, though achieved for the most part *by voluntary* labour; and the results of their discoveries are written in the prosperous annals of our native industry. . . .

Turning from topics which involve industrial interests, to other lines of geological research, we remark how firmly since 1831 the great facts of rock-stratification, succession of life, earth-movement, and changes of oceanic areas have been established and reduced to laws—laws, indeed, of phenomena at present, but gradually acquiring the character of laws of causation.

Among the important discoveries by which our knowledge of the earth's structure and history has been greatly enlarged within forty years, place must be given to the results of the labours of Sedgwick and Murchison, who established the Cambro-Silurian systems, and thus penetrated into ancient time-relics very far toward the shadowy limit of palæontological research. Stimulated by this success, the early strata of the globe have been explored with unremitting industry in every corner of the earth; and thus the classification and the nomenclature which were suggested in Wales and Cumberland are found to be applicable in Russia and India, America and Australia, so as to serve as a basis for the general scale of geological time, founded on organic remains of the successive ages.

This great principle, the gift of William Smith, is also employed with success in a fuller study of the deposits which stand among the latest in our history and involve a vast variety of phenomena, touching a long succession of life on the land, changes of depth in the sea, and alterations of climate. Among these evidences of physical revolution, which, if modern as geological events, are very ancient if estimated in centuries, the earliest monuments of men find place—not buildings, not inhabited caves or dwellings in dry earth-pits, not pottery or fabricated metal, but mere stones shaped in rude fashion to constitute apparently the one tool and one weapon with which, according to Prestwich, and Evans, and Lubbock, the poor inhabitant of northern climes had to sustain and defend his life.

Nothing in my day has had such a decided influence on the public mind in favour of geological research, nothing has so clearly brought out the purpose and scope of our science, as these two great lines of inquiry, one directed to the beginning, the other to the end of the accessible scale of earthly time; for thus has it been made clear that our purpose can be nothing less than to discover the history of the land, sea, and air, and the long sequence of life, and to marshal the results in a settled chronology—not, indeed, a scale of years to be measured by the rotations or revolutions of planets, but a series of ages slowly succeeding one another through an immensity of time.

There is no question of the truth of this history. The facts observed are found in variable combinations from time to time, and the interpretations of these facts are modified in different directions; but the facts are all natural phenomena and the interpretations are all derived from real laws of these phenomena—some certified by mathematical and mechanical research, others based on chemical discovery, others due to the scalpel of the anatomist, or the microscopic scrutiny of the botanist. The grandest of early geological phenomena have their representatives, however feeble, in the changes which are now happening around us; the forms of ancient life most surprising by their magnitude or singular adaptations can be explained by analogous though often rare and abnormal productions of to-day. Biology is the contemporary index of Palæontology, just as the events of the nineteenth century furnish explanations of the course of human history in the older times. . . .

During the long course of geological time the climates of the earth have changed. In many regions evidence of such change is furnished by the forms of contemporary life. Warm climates have had their influence on the land, and favoured the growth of abundant vegetations as far north as within the arctic circle; the sea has nourished reef-making corals in northern Europe during Palæozoic and Mesozoic ages; crocodiles and turtles were swimming round the coasts of Britain, among islands clothed with Zamiæ and haunted by marsupial quadrupeds. How have we lost this primæval warmth? Does the earth contribute less heat from its interior stores? does the atmosphere obstruct more of the solar rays or permit more free radiation from the land and sea? has the sun lost through immensity of time a sensible portion of its beneficent influence? or, finally, is it only a question of the elevation of mountains, the oceanic currents, and the distribution of land and sea?

The problems thus suggested are not of easy solution, though in each branch of the subject some real progress is made. The globe is slowly changing its dimensions by cooling; thus inequalities and movements of magnitude have arisen and are still in progress on its surface: the effect of internal pressure, when not resulting in mass-movement, is expressed in the molecular action of heat which Mallet applies to the theory of volcanoes. The sun has no recuperative auxiliary known to Thomson for replacing his decaying radiation; the earth, under his influence, as was shown by Herschel and Adhemar, is subject to periods of greater and less warmth, alternately in the two hemispheres and generally over the whole surface; and finally, as Hopkins has shown, by change of local physical conditions the climate of northern zones might be greatly cooled in some regions and greatly warmed in others.

One is almost frozen to silence in presence of the vast sheets of ice which some of my friends (followers of Agassiz) believe themselves to have traced over the mountains and vales of a great part of the United Kingdom, as well as over the kindred regions of Scandinavia. One shudders at the thought of the innumerable icebergs with their loads of rock, which floated in the once deeper North Sea, and above the hills of the three Ridings of Yorkshire, and lifted countless blocks of Silurian stone from lower levels, to rest on the precipitous limestones round the sources of the Ribble.

Those who, with Professor Ramsay, adopt the glacial hypothesis in its full extent, and are familiar with the descent of ice in Alpine valleys where it grinds and polishes the hardest rocks, and winds like a slow river round projecting cliffs, are easily conducted to the further thought that such valleys have been excavated by such ice-rubbers, and that even great lakes on the course of the rivers have been dug out by ancient glaciers which once extended far beyond their actual limits. That they did so extend is in several instances well ascertained and proved; that they did in the manner suggested plough out the valleys and lakes is a proposition which cannot be accepted until we possess more knowledge than has yet been attained regarding the resistance offered by ice to a crushing force, its tensile strength, the measure of its resistance to shearing, and other data required for a just estimate of the problem. At present it would appear that under a column of its own substance 1000 ft. high, ice would not retain its solidity; if so, it could not propagate a greater pressure in any direction. This question of the excavating effect of glaciers is distinctly a mechanical problem, requiring a knowledge of certain data; and till these are supplied, calculations and conjectures are equally vain.

A distinguishing feature of modern geology is the greater development of the doctrine that the earth contains in its burial-vaults, in chronological order, forms of life characteristic of the several successive periods when stratified rocks were deposited in the sea. This idea has been so thoroughly worked upon in all countries, that we are warranted to believe in something like one universal order of appearance in time, not only of large groups but even of many genera and species. The Trilobitic ages, the Ammonitic, Megalosaurian, and Palæotherian periods are familiar to every geologist. What closed the career of the several races of plants and animals on the land and in the sea, is a question easily answered for particular parts of the earth's surface by reference to "physical change;" for this is a main cause of the presence or absence, and in general of the unequal distribution of life. But what brought the succession of different races in something like a constant order, not in one tract only, but one may say generally in oceanic areas, over a large portion of the globe?

Life unfolds itself in every living thing, from an obscure, often undistinguishable cell germ, in which resides a potential of both physical and organic change—a change which, whether continual or interrupted, gradual or critical, culminates in the production of similar germs, capable under favourable conditions of assuming the energy of life.

How true to their prototypes are all the forms with which we are familiar, how correctly they follow the family pattern for centuries, and even thousands of years, is known to all students of ancient art and explorers of ancient catacombs. But much more than this is known. Very small differences separate the elephant of India from the mammoth of Yorkshire, the *Waldheimia* of the Australian shore from the *Terebratula* of the Cotswold oolite, the dragon-fly of our rivers from the *Libellula* of the Lias, and even the *Rhynchonellæ* and *Lingulæ* of the modern sea from the old species which swarm in the Palæozoic rocks.

But concurrently with this apparent perpetuity of similar forms and ways of life, another general idea comes into notice. No two plants are more than alike; no two men have more than the family resemblance; the offspring is not in all respects an exact copy of the parent. A general reference to some earlier type, accompanied by special diversity in every case ("descent with modification"), is recognised in the case of every living being.

Similitude, not indentity, is the effect of natural agencies in the continuation of life-forms, the small differences from identity being due to limited physical conditions, in harmony with the general law that organic structures are adapted to the exigencies of being. Moreover, the structures are adaptable to new conditions; if the conditions change, the structure changes also, but not suddenly; the plant or animal may survive in presence of slowly altered circumstances, but must perish under critical inversions. These adaptations, so necessary to the preservation of a race, are they restricted within narrow limits? or is it possible that in course of long-enduring time, step by step and grain by grain, one form of life can be changed and has been changed to another, and adapted to fulfil quite different functions? It is thus that the innumerable forms of plants and animals have been "developed" in the course of ages upon ages from a few original types?

This question of development might be safely left to the prudent researches of Physiology and Anatomy, were it not the case that Palæontology furnishes a vast range of evidence on the real succession in time of organic structures, which on the whole indicate more and more variety and adaptation, and in certain aspects a growing advance in the energies of life. Thus at first only invertebrate animals appear in the catalogues of the inhabitants of the sea, then fishes are added, and reptiles and the higher vertebrata succeed; man comes at last, to contemplate and in some degree to govern the whole.

The various hypothetical threads by which many good naturalists hope to unite the countless facts of biological change into an harmonious system have culminated in Darwinism, which takes for its basis the facts already stated, and proposes to explain the analogies of organic structure by reference to a common origin, and their differences to small, mostly congenital, modifications which are integrated in particular directions by external physical conditions, involving a "struggle for existence." Geology is interested in the question of development, and in the particular exposition of it by the great naturalist whose name it bears, because it alone possesses the history of the development *in time*, and it is to inconceivably long periods of time, and to the accumulated effect of small but almost infinitely numerous changes in certain directions, that the full effect of the transformations is attributed.

For us, therefore, at present it is to collect with fidelity the evidence which our researches must certainly yield, to trace the relation of forms to time generally and physical conditions locally, to determine the life-periods of species, genera, and families in different regions, to consider the cases of temporary interruption and occasional recurrence of races, and how far by uniting the results obtained in different regions the alleged "imperfection of the geological record" can be remedied.

The share which the British Association has taken in this great work of actually reconstructing the broken forms of ancient life, of repeopling the old land and older sea, of mentally reviving, one may almost say, the long-forgotten past, is considerable, and might with advantage be increased. We ask, and wisely, from time to time, for the combined labour of naturalists and geologists in the preparation of reports on particular classes or families of fossil plants and animals, their true structure and affinities, and their distribution in geological time and geographical space. Some examples of this useful work will, I hope, be presented to this meeting. Thus have we obtained the aid of Agassiz and Owen, and have welcomed the labours of Forbes and Morris, and Lycett, and Huxley, of Dawkins and Egerton, of Davidson, Duncan, and Wright, of Williamson and Carruthers and Woodward, and many other eminent persons, whose valuable results have for the most part appeared in other volumes than our own.

Among these volumes let me in a special manner recall to your attention the priceless gift to Geology which is annually offered by the Palæontographical Society, a gift which might become even richer than it is, if the literary and scientific part of our community were fortunate enough to know what a perpetual treasure they might possess in return for a small annual tribute. The excellent example set and the good work recorded in the Memoirs of the Society referred to have not been without influence on foreign men of science. We shall soon have such Memoirs from France and Italy, Switzerland and Germany, America and Australia; and I trust the effect of such generous rivalry will be to maintain and increase the spirit of learned research and of original observation which it is our privilege and our duty to foster, to stimulate, and to combine.

On all the matters, indeed, which have now been brought, to your thoughts the one duty of geologists is to collect more and more accurate information; the one fault to be avoided is the supposition that our work in any department is complete. We should speak modestly of what has been done; for we have completed nothing, except the extinction of a crowd of errors, and the discovery of right methods of proceeding toward the acquisition of truth. We may speak hopefully of what is to be accomplished; for the right road is before us. We have taken some steps along it; others will go beyond us and stand on higher levels. But it will be long before anyone can reach the height from which he may be able to survey the whole field of research and collect the results of ages of labour.

SECTION D.—BIOLOGY

OPENING ADDRESS BY THE PRESIDENT, PROF. ALLMAN

The present Aspects of Biology and the Method of Biological Study

FOR some years it has been the practice at the meetings of this Association for the special presidents to open the work of their respective sections with an address which is supposed to differ, in the greater generality of its subject, from the ordinary communications to the sections. Finding that during the present meeting this duty would devolve on myself, I thought over the available topics, and concluded that a few words on the present aspect of Biology and the method of Biological Study would best satisfy the conditions imposed.

I shall endeavour to be as little technical as my subject will allow, and though I know that there are here present many to whom I cannot expect to convey any truths with which they are not already familiar, yet in an address of this kind the speaker has no right to take for granted any large amount of scientific knowledge in his audience. Indeed, one of the chief advantages which result from these meetings of the British Association consists in the stimulus they give to inquiry—in the opportunity they afford to many of becoming acquainted for the first time with the established truths of Science, and the initiation among them of new lines of thought.

And this is undoubtedly no small gain; for how many are there who, though they may have reaped all the advantages which our established educational systems can bestow, are yet sadly deficient in a knowledge of the world of life which surrounds them. It is a fair and wonderful world, this on which we have our dwelling-place, and yet how many wander over it unheedingly? by how many have its lessons of wisdom never been read? how many have never spared a thought on the beauty of its forms, the harmony of its relations, the deep meaning of its laws?

And with all this there is assuredly implanted in man an undying love of such knowledge. From his unshaken faith in causation he yearns to deduce the unknown from the known, to look beyond what is at hand and obvious to what is remote and unseen.

Conception of Biology and Function of the Scientific Method

Under the head of Biology are included all those departments of scientific research which have as their object the investigation of the living beings—the plants and the animals—which tenant the surface of our earth, or have tenanted it in past time

It admits of being divided under two grand heads: Morphology, which treats of Form, and Physiology, which treats of Function; and besides those there are certain departments of Biological study to which both Morphology and Physiology contribute, such as Classification, Distribution, and that department of research which is concerned with the origin and causes of living and extinct forms.

By the aid of observation and experiment we obtain the elements which are to be combined and developed into a science of living beings, and it is the function of the scientific method to indicate the mode in which the combinations are to be effected, and the path which the development must pursue. Without it the results gained would be but a confused assemblage of isolated facts and disconnected phenomena; but aided by a philosophic method, the observed facts become scientific propositions, what was apparently insignificant becomes full of meaning, and we get glimpses of the consummate laws which govern the whole.

Importance of Anatomy

The first step in our morphological study of human beings is to obtain an accurate and adequate knowledge of the forms of the individual objects which present themselves to us in our contemplation of the animal and vegetable kingdoms. For such knowledge, however, much more is needed than an acquaintance with their external figure. We must subject them to a searching scrutiny; we must make ourselves familiar with their anatomy, which involves not only a knowledge of the forms and disposition of their organs, internal as well as external, but of their histology, or the microscopic structure of the tissues of which these organs, are composed. Histology is nothing more than Anatomy carried to its extreme term, to that point where it meets with the Morphological Unit, the ultimate element of form, and the simplest combinations of this out of which all the organs in the living body are built up.

Among the higher animals Anatomy, in the ordinary sense of the word, is sufficiently distinct from Histology to admit of separate study; but in the lower animals and in plants the two become confounded at so many points as to render their separate study often impracticable.

Now the great prominence given to Anatomy is one of the points which most eminently distinguish the modern schools of Biology.

Development

Another order of morphological facts of scarcely less importance than those obtained from anatomical study is that derived from the changes of form which the individual experiences during the course of its life. We know that every organised being commences existence as a simple sphere of protoplasm, and that from this condition of extreme generalisation all but the very lowest pass through phases of higher and higher specialisation acquiring new parts and differentiating new tissues. The sum of these changes constitutes the development of the organisms, and no series of facts is more full of significance in its bearing on Biological Science than that which is derived from the philosophical study of Development.

Classification an Expression of Affinities

Hitherto we have been considering the individual organism without any direct reference to others. But the requirements of the biological method can be satisfied only by a comparison of the various organisms one with the other. Now the grounds of such comparison may be various, but what we are at present concerned with will be found in anatomical structure and in developmental changes; and in each of these directions facts of the highest order and of great significance become apparent.

By a carefully regulated comparison of one organism with another, we discover the resemblances as well as the differences between them. If these resemblances be strong, and occur in important points of structure or development, we assert that there is an affinity between the compared organisms, and we assume that the closeness of the affinity varies directly with the closeness of the resemblance.

It is on the determination of these affinities that all philosophic classification of animals and plants must be based. A philosophical classification of organised beings aims at being a succinct statement of the affinities between the objects so classified, these affinities being at the same time so set forth as to have their various degrees of closeness and remoteness indicated in the classification.

Affinities have long been recognised as the grounds of a natural biological classification, but it is only quite lately that a new significance has been given to them by the assumption that they may indicate something more than simple agreement with a common plan—that they may be derived by inheritance from a common ancestral form, and that they therefore afford evidence of a true blood relationship between the organisms presenting them.

The recognition of this relationship is the basis of what is known as the Descent Theory. No one doubts that the resemblances we notice among the members of such small groups as those we name species are derived by inheritance from a common ancestor, and the Descent Theory is simply the extension to the larger groups of this same idea of relationship.

If this be a true principle, then biological classification becomes an exposition of family relationship—a genealogical tree in which the stem and branches indicate various degrees of relationship and direct and collateral lines of descent. It is this conception which takes classification out of the domain of the purely Morphological.

Affinity determined by the Study of Anatomy and Development

From what has just been said it follows that it is mainly by a comparison of organisms in their anatomical and developmental characters that their affinities are discoverable. The structure of an organism will in by far the greater number of cases be sufficient to indicate its true affinity, but it sometimes happens that certain members of a group depart in their structure so widely from the characters of the type to which they belong, that without some other evidence of their affinities no one would think of assigning them to it. This evidence is afforded by development.

An example or two will serve to make the subject clear, and we shall first take one from a case where, without a knowledge of anatomical structure, we should easily go astray in our attempts to assign to the forms under examination their true place in the classification.

If we search our coasts at low water we shall be sure to meet with certain plant-like animals spreading over the rocks or rooted to the fronds of sea-weeds, all of which present so close a resemblance to one another as to have led to their being brought together into a single group to which, under the name of "Polypes," a definite place was assigned in the classification of the animal kingdom.

They are all composite animals consisting of an association of buds or zooids, which remain organically united to one another, and give to the whole assemblage the appearance in many cases of a little branching tree. Every bud carries a delicate transparent cup of chitine within which is contained the principal part of the animal, and from which this has the power of spontaneously protruding itself; and when thus protruded it will be seen to present a beautiful crown of tentacles surrounding a mouth through which food is taken into a stomach. As long as no danger threatens, the little animal will continue displayed with its beautiful corona of tentacles expanded; but touch it ever so lightly, and it will instantly close up its tentacles, retract its whole body, and take refuge in the recesses of the protecting cup.

So far then there is a complete agreement between the animals which have been thus associated under the designation of Polypes, and in all that concerns their external form no one point can be adduced in opposition to the justice of this association. When, however, we pass below the surface and bring the microscope and dissecting needle to bear on their internal organisation, we find that among the animals thus formed so apparently alike, we have two totally distinct types of structure; that while in one the mouth leads into a simple excavation of the body on which devolves the whole of the functions which represent digestion, in the other there is a complete alimentary tract entirely shut off from the proper cavity of the body and consisting of distinctly differentiated oesophagus, stomach, and intestine; while in the one the muscular system consists of an indistinct layer of fibres intimately united in its whole extent with the body walls, in the other there are distinctly differentiated free bundles of muscles for the purpose of effecting special motions in the economy of the animal; while in the one no differentiated nervous system can be detected, in the other there is a distinct nervous ganglion with nervous filaments. In fact the two forms are shown by a study of their anatomical structure to belong to two entirely different primary divisions of the animal kingdom; for while the one has a close affinity with the little fresh-water Hydra, and is therefore referred to the Hydroida among the sub-kingdom Coelenterata, the other is referable to the group of the Polyzoa; it has its immediate affinities with the Ascidians, and belongs to the sub-kingdom of the Mollusca.

We shall next take an example in which the study of development rather than of anatomy affords the clue to the true affinities of the organism.

Attached to the abdomen of various crabs may often be seen certain soft fleshy sacs to which the name of *Sacculina* has been given. They hold their place by means of a branching root-like extension which penetrates the abdomen of the crab and winds itself round its intestine or dives into its liver, within which its fibres ramify like the roots of a tree.

Now the question at once presents itself: what position in the animal kingdom are we to assign to this immoveably rooted sac destitute of mouth and of almost every other organ with which we are in the habit of associating the structure of an animal?

Anatomy will here be powerless in helping us to arrive at a conclusion, for the dissecting knife shows us little more than a closed sac filled with eggs and fixed by its tenacious roots in the viscera of its victim. Let us see, however, what we learn from development. If some of the eggs with which the Sacculina is filled be placed in conditions suited to their development, they give origin to a form as different as can well be imagined from the sacculina. It is an active, somewhat oval-shaped little creature, covered with a broad dorsal shield or carapace, and furnished with two pairs of strong swimming feet which carry long bristles, and also with a pair of anterior limbs or antennæ. It is, in fact, identical with a form known to zoologists by the name "Nauplius," and which has been proved to be one of the young states of the Barnacle and of other lower crustacea; while even some of the higher crustacea have been observed to pass through a similar stage.

After a short time the Nauplius of our Sacculina changes its form; the carapace folds down on each side and assumes the shape of a little bivalve shell; while six new pairs of swimming feet are developed. The little animal continues its active natatory life, and in this stage it is again identical in all essential points with one of the young stages of the Barnacle.

In the meantime a remarkable change takes place in the two antennæ; they become curiously branched and converted into prehensile organs. The young Sacculina now seeks the crab on which it is to spend parasitically the rest of its life; it loses its bivalve shell, the prehensile antennæ takes hold of its victim, penetrates the soft skin of its abdomen in order to seek within it the nutriment with which it can be there so plentifully supplied, locomotion is gone for ever, and the active and symmetrical Nauplius becomes converted into the inert and shapeless Sacculina.

The nearest affinities of Sacculina are thus undoubtedly with the Barnacles, which have been proved both on anatomical and developmental grounds to belong to the great division of the Crustacea.

A Philosophical Classification cannot form a single Rectilineal Series

A comparison of animals with one another having thus resulted in establishing their affinities, we may arrange them into groups, some more nearly, others more remotely related to one another. The various degrees and directions of affinity will be expressed in every philosophical arrangement, and as these affinities extend in various directions, it becomes at once apparent that no arrangement of the animal or vegetable kingdom in a straight line ascending like the steps of a ladder from lower to higher forms, can give a true idea of the relations of living beings to one another. These relations, on the contrary, can be expressed only by a ramified and complex figure which we have already compared to that of a genealogical tree.

Homology

In the comparison of organised beings with one another, certain relations of great interest and significance become apparent between various organs. These are known by the name of Homologies, and organs are said to be homologous with one another when they can be proved to be constructed on the same fundamental plan, no matter how different they may be in form and in the functions which they may be destined to execute. Organs not constructed on the same fundamental plan may yet execute similar functions, and then, whether they do or do not resemble one another in form, they are said to be merely analogous; and some of the most important steps in modern Biology have resulted from attention to the distinction between Homology and Analogy, a distinction which was entirely disregarded by the earlier schools.

The nature of Homology and its distinction from Analogy will be best understood by a few examples.

Compare the wing of a bird with that of an insect; there is a resemblance between them in external form; there is also an identity of function, both organs being constructed for the purpose of flight, and yet they are in no respect homologous, for they are formed on two distinct plans which have nothing whatever in common. The relation between them is that simply of analogy.

On the other hand, no finer illustrations of Homology can be adduced than those which are afforded by a comparison with one another of the anterior limbs among the various members of the vertebrata. Let us compare, for example, the bird's wing with the anterior limb of man. Here we have two organs between which the ordinary observer would fail to recognise any resemblance—organs, too, whose functions are entirely different, one being formed for prehension and the other for flight. When, however, they are compared in the light which a philosophic anatomy is capable of throwing on them, we find, between the two, a parallelism which points to one fundamental type on which they are both constructed.

There is first the shoulder-girdle, or system of bones by which, in each case, the limb is connected with the rest of the skeleton. Now this part of the skeleton in man is very different in form from the same part in the bird, and yet a critical comparison of the two shows us that the difference mainly consists in the fact that the coracoid which in man is a mere process of the scapula, is in the bird developed as an independent bone; and in the further fact that the two clavicles in man are, in the bird, united into a single V-shaped bone or "furcula." Then, if we can compare the arm, fore-arm, wrist, and hand in the human skeleton with the various parts which follow one another in the same order in the skeleton of the bird's wing, we shall find between the two series a correspondence which the adaptations to special functions may in some regions mask, but never to such an extent as to render the fundamental unity of plan difficult of detection by the method of the higher anatomy. As far as regards the arm and fore-arm, these in the bird are nearly repetitions of their condition in the human skeleton; but the parts which follow appear at first sight so different as to have but little relation with one another, and yet a common line can be traced with great distinctness through the two. Thus the wrist is present in the bird's wing as well as in the anterior limb of man, but while in man it is composed of eight small irregularly-shaped bones arranged in two rows, in the wing it has become greatly modified, the eight bones being reduced to two. Lastly, the hand is also represented in the wing, where it constitutes a very important part of the organ of flight, but where it has undergone such great modification as to be recognisable only after a critical comparison; for the five metacarpal bones of the human hand are reduced to two consolidated with one another at their proximal and distal ends; and then the five fingers of the hand are reduced in the wing to three, which represent the middle finger, fore-finger, and thumb. The fore-finger in the bird consists of only one phalanx, the middle of two, and the thumb forms a small stiletto-like bone springing from the proximal end of the united metacarpals.

In the case now adduced we have an example of the way in which the same organ in two different animals may become very differently modified in form, so as to fit it for the performance of two entirely different functions, and yet retain sufficient conformity to a common plan to indicate a fundamental unity of structure.

Let us take another example, and this I shall adduce from the vegetable kingdom, which is full of beautiful instances of the relations with which we are now occupied.

There are the parts known as tendrils, thread-like organs, usually rolling themselves into spirals, and destined, by twining round some fixed support, to sustain climbing plants in their efforts to raise themselves from the ground. We shall take two examples of these beautiful appendages, and endeavour to determine their homological significance.

There is the genus *Smilax*, one species of which adorns the hedges of the south of Europe, where it takes the place of the Bryony and *Tamus* of our English country lanes. From the point where the stalks of its heart-shaped leaves spring from the stem, there is given off a pair of tendrils by means of which the Smilax clings to the surrounding vegetation in an inextricable entanglement of branches and foliage.

With the tendrils of the Smilax let us compare those of the *Lathyrus aphaca*, a little vetch occasionally met with in waste places and the margins of corn-fields. The leaves are represented by arrow-shaped leaf-like appendages, which are placed opposite to one another in pairs upon the stem, but instead of each of these carrying two tendrils at its origin like the leaves of the Smilax, a single tendril springs from the middle point between each pair.

The tendrils in the two cases, though similar in appearance and in function, differ thus in number and arrangement, and the questions occur: are they homologous with one another, or are they only analogous? and if they be only analogous, can we trace between them and any other organ homologous relations?

To enable us to decide on this point, we must bear in mind that a leaf when typically developed consists of three portions, the lamina or blade, the petiole or leaf-stalk, and a pair of foliaceous appendages or stipules, which are placed at the base of the leaf-stalk. Now this typical leaf affords the key to the homologies of the tendrils in the two cases under examination.

Take the Smilax: in this case there are no stipules of the ordinary form, but the two tendrils hold exactly the position of the stipules in our type-leaf, and must be regarded as representing them. We have only to imagine these stipules so modified in their form as to become reduced to two long spiral threads, and we shall at once have the tendrils of the Smilax; on the other hand let the stipules in our type remain as leaf-like organs, and let the rest of the leaf—the lamina and petiole—lose its normal character, and become changed into a spiral thread, and we shall then have the stipules of our type-leaf retained in the two opposite leaf-like organs of the Lathyrus, while the remainder of the type-leaf will present itself in the condition of the Lathyrus tendril which springs from the central point between them.

The tendrils of the Smilax and of the *Lathyrus aphaca* are thus not homologous with one another, but only analogous, while those of the Smilax are homologous with a pair of stipules and those of the Lathyrus homologous with the lamina and petrile of a leaf.

Besides the homology discoverable between the organs of different animals and plants, a similar relation can be traced between organs in the same animal or plant; as, for example, that between the different segments of the vertebral column, which can be shown to repeat one another homologically; and that between the parts composing the various verticals of the flower and the leaves in the plant.

The existence of homological relations such as have been just illustrated admits of an easy explanation by the application of the doctrine of descent, according to which the two organs compared would originate from a common ancestral form. In accordance with this hypothesis, homology would mean an identity of genesis in two organs, as analogy would mean an identity of function.

Distribution and Evolution

Another very important department of biological science is that of the Distribution of organised beings. This may be either Distribution in Space, Geographical Distribution: or Distribution in Time, Palæontological Distribution. Both of these have of late years acquired increased significance, for we have begun to get more distinct glimpses of the laws by which they are controlled, of the origin of Faunas and Floras, and of the causes which regulate the sequence of life upon the earth. Time, however, will not allow me to enter upon this subject as fully as its interest and importance would deserve, and a few words on Palæontological Distribution is all that I can now venture on.

The distribution of organised beings in time has lately come before us in a new light by the application to it of the hypothesis of evolution. According to this hypothesis, the higher groups of organised beings now existing on the earth's surface have come down to us with gradually increasing complexity of structure by continuous descent from forms of extreme simplicity which constituted the earliest life of our planet.

In almost every group of the animal kingdom the members which compose it admit of being arranged in a continuous series passing down from more specialised, or higher, to more generalised or lower forms; and if we have any record of extinct members of the group, the series may be carried on through these. Now while the descent hypothesis obliges us to regard the various terms of the series as descended from one another, the most generalised forms will be found among the extinct ones, and the further back in time we go the simpler do the forms become.

By a comparison of the forms so arranged we obtain as it were the law of the series, and can thus form a conception of the missing terms and continue the series backwards through time, even where no record of the lost forms can be found, until from simpler to still simpler terms we at last arrive at the conception of a term so generalised that we may regard it as the primordial stock, the ancestral form from which all the others have been derived by descent.

This root form is thus not actually observed, but is rather obtained by a process of deduction, and is therefore hypothetical. We shall strengthen, however, its claims to acceptance by the application of another principle. The study of embryology shows that the higher animals, in the course of their development, pass through transitory phases which have much in common with the permanent condition of lower members of the type to which they belong, and therefore with its extinct representatives. We are thus enabled to lay down the further principle that the individual, in the course of its own development from the egg to the fully formed state, recapitulates within that short period of time the various forms which its ancestry presented in consecutive epochs of the world's history; so that if we knew all the stages of its individual development, we should have a long line of its descent. Through the hypothesis of evolution, palæontology and embryology are thus brought into mutual bearing on one another.

Let us take an example in which these two principles seem to be illustrated. In rocks of the Silurian age there exist in great profusion the remarkable fossils known as graptolites. These consist of a series of little cups or cells arranged along the sides of a common tube, and the whole fossil presents so close a resemblance to one of the Sertularian hydroids which inhabit the waters of our present seas as to justify the suspicion that the graptolites constitute an ancient and long since extinct group of the Hydroida. It is not, however, with the proper cells or hydrothecæ of the Sertularians that the cells of the graptolite most closely agree, but rather with the little receptacles which in certain Sertularinæ belonging to the family of the Plumularida we find associated with the hydrothecæ, and which are known as "Nematophores," a comparison of structure then shows that the graptolites may with considerable probability be regarded as representing a Plumularia in which the hydrothecæ had never been developed and in which their place had been taken by the nematophores.

Now it can be shown that the nematophores of the living Plumularida are filled with masses of protoplasm which have the power of throwing out pseudopodia, or long processes of their substance, and that they thus resemble the Rhizopoda, whose soft parts consist entirely of a similar protoplasm and which stand among the Protozoa or lowest group of the animal kingdom. If we suppose the hydrotheca suppressed in a plumularian, we should thus nearly convert it into a colony of Rhizopoda, from which it would differ only in the somewhat higher mophological differentiation of its cœnosarc or common living bond by which the individuals of the colony are organically connected. And just such a colony would, under this view, a graptolite be, waiting only for the development of hydrotheca to raise it into the condition of a plumularian.

Bringing now the evolution hypothesis to bear upon the question, it would follow that the graptolite may be viewed as an ancestral form of the Sertularian hydroids, a form having the most intimate relations with the Rhizopoda; that hydranths and hydrothecæ became developed in its descendants; and that the rhizopodal graptolite became thus converted in the lapse of ages into the hydroidal Sertularian.

This hypothesis would be strengthened if we found it agreeing with the phenomena of individual development. Now such Plumularida as have been followed in their development from the egg to the adult state do actually present well-developed nematophores before they show a trace of hydrothecæ, thus passing in the course of their embryological development through the condition of a graptolite, and recapitulating within a few days stages which it took incalculable ages to bring about in the palæontological development of the tribe.

I have thus dwelt at some length on the doctrine of evolution because it has given a new direction to biological study and must powerfully influence all future researches. Evolution is the highest expression of the fundamental principles established by Mr. Darwin, and depends on the two admitted faculties of living beings—*heredity*, or the transmission of characters from the parent to the offspring; and *adaptivity*, or the capacity of having these characters more or less modified in the offspring by external agencies, or it may be by spontaneous tendency to variation.

The hypothesis of evolution may not, it is true, be yet established on so sure a basis as to command instantaneous acceptance, and for a generalisation of such vast significance no one can be blamed for demanding for it a broad and indisputable foundation of facts. Whether, however, we do or do not accept it as firmly established, it is at all events certain that it embraces a greater number of phenomena and suggests a more satisfactory explanation of them than any other hypothesis which has yet been proposed.

With all our admiration, however, for the doctrine of Evolution as one of the most fertile and comprehensive of philosophic hypotheses, we cannot shut our eyes to the difficulties which lie in the way of accepting it to the full extent which has been sometimes claimed for it. It must be borne in mind that though among some of the higher vertebrata we can trace back for some distance in geological time a continuous series of forms which may safely be regarded as derived from one another by gradual modification—as has been done, for example, so successfully by Prof. Huxley in the case of the horse—yet the instances are very few in which such a sequence has been actually established; while the first appearance in the earth's crust of the various classes presents itself in forms which by no means belong to the lowest or most generalised of their living representatives. On this last fact, however, I do not lay much stress, for it will admit of explanation by referring it to the deficiency of the geological record, and then demanding a lapse of time—of enormous length, it is true—during which the necessary modifications would be in progress before the earliest phase of which we have any knowledge could have been reached.

Again, we must not lose sight of the hypothetical nature of those primordial forms in which we regard the branches of our genealogical tree as taking their origin; and while the doctrine of the recapitulation of ancestral forms has much probability, and harmonises with the other aspects of the Evolution doctrine into a beautifully symmetrical system, it is one for which a sufficient number of actually observed facts has not yet been adduced to remove it altogether from the region of hypothesis.

Even the case of the graptolites already adduced is an illustration rather than a proof, for the difficulty of determining the true nature of such obscure fossils is so great that we may be altogether mistaken in our views of their structure and affinities.

To me, however, one of the chief difficulties in the way of the doctrine of Evolution, when carried out to the extreme length for which some of its advocates contend, appears to be the unbroken continuity of inherited life which it necessarily requires through a period of time whose vastness is such that the mind of man is utterly incapable of comprehending it. Vast periods, it is true, are necessary in order to render the phenomena of Evolution possible; but the vastness which the antiquity of life, as shown by its remains in the oldest fossiliferous strata, requires us to give to these periods may be even greater than is compatible with continuity.

We have no reason to suppose that the reproductive faculty in organised beings is endowed with unlimited power of extension, and yet to go no farther back than the Silurian period—though the seas which bore the Eozoon were probably as far anterior to those of the Silurian as these are anterior to our own—the hypothesis of Evolution requires that in that same Silurian period the ancestors of the present living forms must have existed, and that their life had continued by inheritance through all the ramifications of a single genealogical tree down to our own time; the branches of the tree, it is true, here and there falling away, with the extinction of whole genera and families and tribes, but still some always remaining to carry on the life of the base through a period of time to all intents and purposes infinite. It is true that in a few cases a continuous series of forms regularly passing from lower to higher degrees of specialisation, and very probably connected to another by direct descent, may be followed through long geological periods, as for example, the graduated series already alluded to, which may be traced between certain mammals of the Eocene and others living in our own time, as well as the very low forms which have come down to us apparently unmodified from the epoch of the Chalk. But incalculably great as are these periods, they are but as the swing of the pendulum in the Millennium, when compared to the time which has elapsed since the first animalisation of our globe.

Is the faculty of reproduction so wonderfully tenacious as all this, that through periods of inconceivable duration, and exposed to influences the most intense and the most varied, it has still come down to us in an unbroken stream? Have the strongest which had survived in the struggle for existence necessarily handed down to the strongest which should follow them the power of continuing as a perpetual heirloom the life which they had themselves inherited? Or have there been many total extinctions and many renewals of life—a succession of genealogical trees, the earlier ones becoming old and decayed, and dying out, and their place taken by new ones which have no kinship with the others? Or, finally, is the doctrine of Evolution only a working hypothesis which, like an algebraic fiction, may yet be of inestimable value as an instrument of research? For as the higher calculus becomes to the physical inquirer a power by which he unfolds the laws of the inorganic world, so may the hypothesis of Evolution, though only a hypothesis, furnish the biologist with a key to the order and hidden forces of the world of life. And what Leibnitz and Newton and Hamilton have been to the physicist, is it not that which Darwin has been to the biologist?

But even accepting as a great truth the doctrine of Evolution, let us not attribute to it more than it can justly claim. No valid evidence has yet been adduced to lead us to believe that inorganic matter has become transformed into living, otherwise than through the agency of a pre existing organism, and there remains a residual phenomenon still entirely unaccounted for. No physical hypothesis founded on any indisputable fact has yet explained the origin of the primordial protoplasm, and, above all, of its marvellous properties which render Evolution possible.

Accepting, then, the doctrine of Evolution in all freedom and in all its legitimate consequences, there remains, I say, a great residuum unexplained by physical theories. Natural Selection, the Struggle for Existence, the Survival of the Fittest, will explain much, but they will not explain all. They may offer a beautiful and convincing theory of the present order and fitness of the organic universe, as the laws of attraction do of the inorganic, but the properties with which the primordial protoplasm is endowed—its heredity and its adaptivity—remain unexplained by them, for these properties are their cause and not their effect.

For the cause of this cause we have sought in vain among the physical forces which surround us, until we are at last compelled to rest upon an independent volition, a far-seeing intelligent design. Science may yet discover even among the laws of Physics the cause it looks for; it may be that even now we have glimpses of it; that those forces among which recent physical research has demonstrated so grand a unity—Light, Heat, Electricity, Magnetism—when manifesting themselves through the organising protoplasm, become converted into the phenomena of life, and that the poet has unconsciously enunciated a great scientific truth when he tells us of

> "Gay lizards glittering on the walls
> Of ruined shrines, busy and bright
> As though they were *alive with light*."

But all this is only carrying us one step back in the grand generalisation. All science is but the intercalation of causes, each more comprehensive than that which it endeavours to explain, between the great primal cause and the ultimate effect.

I have thus endeavoured to sketch for you in a few broad outlines the leading aspects of biological science, and to indicate the directions which biological studies must take. Our science is one of grand and solemn import, for it embraces man himself and is the exponent of the laws which he must obey. Its subject is vast, for it is Life, and Life stretches back into the illimitable past, and forward into the illimitable future. Life, too, is everywhere. Over all this wide earth of ours, from the equator to the poles, there is scarcely a spot which has not its animal or its vegetable denizens—dwellers on the mountain and on the plain, in the lake and on the prairie, in the arid desert and the swampy fen; from the tropical forest with its strange forms and gorgeous colours, and myriad voices, to the ice-fields of polar latitudes and those silent seas which lie beneath them, where living things unknown to warmer climes congregate in unimaginable multitudes. There is life all over the solid earth; there is life throughout the vast ocean, from its surface down to its great depths, deeper still than the lead of sounding-line has reached.

And it is with these living hosts, unbounded in their variety, infinite in their numbers, that the student of biology must make himself acquainted. It is no light task which lies before him—no mere pastime on which he may enter with trivial purpose, as though it were but the amusement of an hour; it is a great and solemn mission to which he must devote himself with earnest mind and with loving heart, remembering the noble words of Bacon:—

"Knowledge is not a couch whereon to rest a searching and restless spirit; nor a terrace for a wandering and variable mind to walk up and down with a fair prospect; nor a tower of state for a proud mind to raise itself upon; nor a fort or commanding-ground for strife and contention; nor a shop for profit and sale; but a rich storehouse for the glory of the Creator, and the relief of man's estate."

SECTION G.—Mechanical Science

Opening Address by the President, W. H. Barlow, C.E., F.R.S.

In the observations which I have to address to you I shall not attempt a general survey of a subject so vast and so varied as the manufactures of this country, nor shall I attempt to describe the many new and beautiful inventions and mechanical appliances which form a distinguishing feature of the age in which we live; but I shall endeavour to draw your attention to one of the new materials, namely *modern steel*—a material which, though of comparatively recent origin, has already become an important industry, and whose influence in the future seems destined to vie in importance with that resulting from the introduction of iron.

I have used the term "modern steel," because, although the great movement in simplifying and cheapening the process of producing steel is necessarily associated with the name of Mr. Bessemer, yet we have further important steps taken in a forward direction as to the production and treatment of steel by Dr. Siemens and Sir Joseph Whitworth and others, both in this country and abroad.

It is now seventeen years since Mr. Bessemer read a paper at the meeting of the British Association at Cheltenham, which was entitled, "On the Manufacture of Iron and Steel without Fuel."

It is satisfactory to know that Mr. Bessemer has often expressed his firm conviction that had it not been for the publicity given to his invention through the paper which he read before the Mechanical Section of the British Association in 1856, and the great moral support afforded him by men of science whose attention was thereby directed to it, he believes that he would not have succeeded in overcoming the strong opposition with which his invention was met in other quarters.

About this time, or perhaps a little later, a material was produced called "puddled steel," and about the same time the metal known as "homogeneous iron."

The movement which had begun in the production of cheap steel was further assisted and developed by the regenerative furnace of Dr. Siemens, by the introduction of the Siemens-Martin process of making steel, and further and most important progress is suggested by the recent process introduced by Dr. Siemens in making steel direct from the ore.

According to the returns published by the Jury of the International Exhibition of 1852, the total annual produce of steel in Great Britain at that time was 50,000 tons. At the present time there are more than 500,000 tons made by the Bessemer process alone, added to which Messrs. Siemens's works at Landore produce 200,000 tons, besides further quantities which are made by his process at Messrs. Vickers, Messrs. Cammells, the Dowlais, and other works.

I shall not, however, detain you by attempting to trace up the history and progress of steel, nor attempt to notice the various steps by which this branch of industry has been brought to its present important position. My object is to draw attention to this material as to its use and application for *structural and engineering* purposes.

The steel produced by the Bessemer process was at a very early stage employed in rails and wheel-tires. In both these applications the object sought was endurance to resist the effects of wear, and toughness to prevent fracture by blows. There does not exist at present sufficient information to determine accurately the relative values of steel and iron when used for these purposes. As used for wheel-tires, steel had to compete with iron of the highest quality, but it is nevertheless introduced on most of our railways. The iron used in rails was not of such a high quality, and the difference in duration shows a very marked advantage in the employment of steel, the duration of steel rails being variously estimated at from three to six times that of iron.

Steel is also extensively used for ships' plates, and by the War Department for lining the interior of the heaviest guns; while Sir Joseph Whitworth and Messrs. Krupp make guns entirely of steel, though for these purposes the metal is of different quality and differently treated, in order to withstand the enormous concussions to which it is subjected.

And, further, we have steel used in railway-axles, crank-axles for engines, in boilers, in piston-rods, in carriage-springs, and for many other purposes.

But notwithstanding these various employments of steel, there has been, and there continues to be, a difficulty in applying it to engineering structures in this country.

The want of knowledge of the physical properties of steel having been the subject of remark at a discussion at the Institution of Civil Engineers in 1868, a committee, composed of Mr. Fowler, Mr. Scott Russell, Captain Galton, Mr. Berkley, and myself, undertook to conduct a series of experiments upon this subject.

The first were made for the Committee by Mr. Kirkaldy with his testing-machine in London, and were chiefly directed to ascertain the relation which subsists between the resistance of tension, compression, torsion, and transverse strain.

In this series of experiments twenty-nine bars, 15 ft. long, were used, each bar being cut into lengths, and turned or planed into suitable forms for the respective tests, so that a portion of each bar was subjected to each of the above-mentioned tests.

The tensile resistance varied in the different qualities of steel from 28 to 48 tons per inch, and the experiments established conclusively that the relation subsisting between the several resistances of tension, compression, and transverse strain is throughout practically the same as in wrought-iron; that is to say, that a bar of steel whose tensile strength is 50 per cent. above that of wrought-iron will exhibit about the same relative increase of resistance under the other tests.

They further showed that the limit of elasticity in steel is, like that of wrought-iron, rather more than half its ultimate resistance. The total elongation under tensile strain, and the evidences of malleability and toughness, will be referred to hereafter.

The second series recorded in the book published by the Committee gave the results of tempering steel in oil and water. They were made by the officers of the gun-factory at the Royal Arsenal at Woolwich, and show a remarkable increase of strength obtained by this process. This property of steel is now fully recognised and made use of in the steel which forms the lining of the largest guns.

The third series of experiments was made by the Committee upon bars 14 ft. long, 1½ in. in diameter, with the skin upon the metal as it came from the rolls.

The object of these experiments was specially directed to ascertain the *modulus of elasticity*. They were made with the testing-machine at H.M. Dockyard at Woolwich, which machine was placed at our disposal by the Admiralty. The bars were obtained, with some exceptions, in sets of six from each maker, three bars of each set being used in tension and three in compression.

Bars of iron of like dimensions were also tested in the same way, in order to obtain the relative effects in steel and iron. In these experiments sixty-seven steel bars were tested whose tensile strength varied from 32 to 53 tons per inch, and twenty-four iron bars varying from 22 to 29 tons per inch.

The amount of the extensions and compressions were ascertained by *direct measurement*, verniers being for this purpose attached to the bar itself, 10 ft. apart, so that the readings gave the absolute extensions and compressions of this length of the bar.

These experiments, which were very accurately made, showed that the extension and compression of steel per ton per inch was a little less than wrought-iron, that the extension and compression were very nearly equal to each other, and that the modulus of elasticity of steel may be taken at 30,000,000, which result agrees with the conclusions arrived at by American engineers on this subject.

This property of the metal is important in two respects. First, because inasmuch as the extension per ton per inch is practically equal to the compression, it follows that the neutral axis of a structure of steel, strained transversely, will be in the centre of gravity of its section, and that the proper proportion to give to the upper and lower flanges of a girder, when made of the same quality of steel throughout, will be the same as in wrought-iron. Secondly, because the modulus of elasticity of steel is practically equal to that of wrought-iron, and the limit of elasticity is greater, it follows that in a girder of the same proportions as wrought-iron, and strained with an equal proportion of its ultimate tensile strength, the deflection will be greater in the steel than in the iron girder, in the rate of the strength of the metals; so that if it is necessary to make a steel girder for a given span deflect under its load the same amount as an iron girder of the same span, the steel girder must be made of greater depth.

The fourth series of experiments were made by the Committee on riveted steel, and show clearly that the same rules which pply to the riveting of iron apply equally to steel; that is to ay, that the total shearing area of the rivets must be the same, r rather must not be less, than the sectional area of the bar iveted. . . .

We know from established mechanical laws that the limiting pans of structures vary directly as the strength of the material mployed in their construction when the proportion of depth to pan and all other circumstances remain the same. We know also that, taking an ordinary form of open wrought-iron detached girder (as, for example, when the depth is one-fourteenth of the pan), the limiting span in iron, with a strain of 5 tons to the nch upon the metal, is about 600 ft.; and it follows that a steel girder of like proportions, capable of bearing 8 tons to the inch, would have theoretically a limiting span of 960 ft.

This theoretical limiting span of 960 ft. would, however, be educed by some practical considerations connected with the ninimum thickness of metal employed in certain parts, and it would, in effect, become about 900 ft. for a girder of the before-nentioned construction and proportions.

The knowledge of the limiting span of a structure, as has een explained elsewhere, enables us to estimate very quickly, nd with close approximation to the truth, the weight of girders equired to carry given loads over given spans; and although he limiting spans vary with every form of structure, we can btain an idea of the effect of introducing steel by the relative weights of steel and iron required in girders of the kind above nentioned.

Assuming a load in addition to the weight of the girder of one on to the foot, the relative weights under these conditions would be as follows:—

Span	Weight of steel girder. tons	Weight of iron girder tons
200	57	100
300	150	300
400	320	800

It is not alone in the relative weight or in the relative cost that he advantage of the stronger material is important, but with teel we shall be enabled to cross openings which are absolutely mpracticable in iron.

It will naturally be asked why it is that steel is not used in hese structures, if such manifest advantages would result from s employment.

The reason is twofold:—

1st. There is a want of confidence as to the reliability of steel n regard to its toughness and its power to resist fracture from udden strain.

2nd. Steel is produced of various qualities, and we do not ossess the means, without elaborate testing, of knowing whether he article presented to us is of the required quality for structural urposes. A third reason, arising probably out of those before nentioned, is found in the fact that in the regulations of the Board of Trade relative to railway structures, although rules are iven for the employment of cast-iron and wrought-iron, steel as not, up to the present time, been recognised or provided for.

Now, as regards the question of toughness and malleability, nd referring again to Mr. Kirkaldy's experiments, it appears at in the tests of "Bessemer steel" 18 samples were tried under ensile strain, the length of the samples being in round numbers 0 in. and the diameter 1·382 in.; and that when these were ubjected to ultimate strain, the elongation at the moment of acture was in the most brittle example 2¾ in., but generally aried from 4½ to 9½ inches.

In the experiments on transverse strain, in which the bars ere nearly 2 in. square and only 20 in. between the points of upport, all the "Bessemer steel" samples, except two, bent in. without any crack. Again, in the experiments made by he Committee on bars 14 ft. long and 1½ in. in diameter, out of 0 bars of the milder quality of steel, 16 extended more than in., and of these 10 extended more than 12 in. . . .

The treatment by comparison is especially important where netal is required in large masses and of great ductility because he larger the mass and the greater the ductility, the larger and nore numerous are the air-cells, and the effect of the pressure to completely close these cells and render the metal perfectly olid.

By this process mild steel can be made with a strength of 40 ons to the inch, having a degree of ductility equal to that of he best iron.

The more highly carbonised qualities show a decrease of uctility somewhat in the same ratio as the strength increases.

Without going into the numerous achievements of Sir Joseph Whitworth, resulting from the employment of steel, in connection with the extreme accuracy of workmanship produced at his works, or doing more than mention the flat-ended steel shot and shell which pass through iron plates when fired obliquely or penetrate ships' sides below the level of the water, I would call attention to those applications of steel which bear upon its strength and toughness.

In the first place, there are small arms made entirely of steel, of wonderful range and accuracy, capable of penetrating 34 half-inch planks, which is about three times the penetrating power of the Enfield rifle.

Secondly, there are the large guns, also entirely of steel, throwing projectiles from 250 lbs. to 310 lbs. in weight, and burning from 40 to 50 lbs. of powder at a charge, with which a range of nearly 6¼ miles is obtained.

In both these cases the degree of strength and toughness required in the metal is much greater than is necessary for engineering structures.

It is unnecessary to occupy more time in multiplying examples of the toughness of steel. It is well known to manufacturers, and must also be well known to many others here present, that steel of the strength of 33 or 36 tons per inch can be made, and is made in large quantities at moderate price, possessing all the toughness and malleability required in engineering structures.

I will proceed, therefore, to the second part of the subject —namely, the want of means of knowing that a given sample of steel is of the quality suited for structural purposes.

With most other metals chemical analysis is in itself a complete and sufficient test of quality, but in steel it is not so. The toughness of steel may be altered by sudden cooling; and although the effect of this operation, and generally the effects of tempering, are greater when the quantity of carbon is considerable, yet it acts more or less in the mild qualities of steel; so that we cannot rely entirely on the aid of the chemist, but must fall back on mechanical tests. And in point of fact, seeing that the qualities required are mechanical, it is no more than reasonable that the test should be mechanical; for this includes not only the test of material but of workmanship.

Now there are two descriptions of mechanical testing, which may be distinguished as destructive and non-destructive—the one being beyond and the other within the elastic limit of the material. The destructive test is that usually applied to a part of an article manufactured, as, for example, a piece cut off a boiler plate and tested by absolute rupture, or by bending or otherwise, whereby the strength and quality of the material in the plate is known.

The non-destructive test is that usually applied to the finished work, as in the test of a boiler by hydraulic pressure, or the testing of a gun by the proof-charge. The strain in this case is made greater than that which will arise in the daily use of the article, but is not so greatly in excess as to be beyond the elastic limit of the material.

As regards engineering structures, this second test is easy of application; but it affords no sufficient criterion that the metal possesses that degree of toughness necessary to resist the action of sudden strains.

It may be said that engineers may ascertain for themselves, by inspection and testing at the works, that they are being supplied with the material that they require; but assuming that the tests and mode of testing were in all respects satisfactory to them, and that the metal supplied was of the right quality, we have still to comply with the conditions of the Act for the Regulation of Railways, and we must satisfy the Government Inspector.

It is not to be supposed that he can attend all the required tests at the works; and the question remains, how is the Inspecting Officer of the Board of Trade to be enabled to distinguish the quality of metal in a finished bridge, when he is called upon to give a certificate that it is safe for public traffic?

If we could adduce clear and distinct evidence that the metal used for a bridge was of a quality which would bear 8 tons to the inch with as much safety as common iron can bear 5 tons, there can be no reasonable doubt that the Board of Trade would make suitable provision in its regulations for the employment of such material.

The difficulty lies in the want of something whereby the quality of the metal may be known and relied upon with confidence by others besides those who made the article.

In gold and silver this is accomplished by the stamp put upon them, in guns and small arms we have the proof-mark, but in iron and steel we have nothing whereby the one quality of metal can be distinguished from another; and until some sufficient means be devised for this purpose, it is difficult to see how we are to escape from the position in which we are now placed—namely, that while we possess a material by which we can increase considerably the spans and diminish the weight and cost of engineering works, we are restricted to make designs and construct our works by a rule made for wrought iron, and adapted to the lowest quality of that material.

As the rule made by the Board of Trade in respect of wrought-iron railway structures may not be generally known, I here give it:—

"In a wrought-iron bridge the greatest load which can be brought upon it, added to the weight of the superstructure, should not produce a greater strain on any part of the material than 5 tons per inch."

It will be observed that this 5 tons per inch is the governing element, irrespective entirely of the quality of metal used; and it is obvious that a rule so framed must act as a discouragement to any endeavour to improve the quality of metal, while it tends to induce the employment of the cheapest and most inferior descriptions which can be made under the name of wrought-iron.

In endeavouring to seek an amendment of the rules, which will permit of the employment of steel or other metal of higher strength than 5 tons to the inch, I feel bound to say that I do not consider that the Board of Trade is alone responsible for the position in which the question now stands; and as regards the Government Inspecting Officers, I can only say that in the numerous transactions I have had with them, and although differences of opinion have occasionally arisen, yet, considering the responsibility which rests upon them, I have found them anxious to afford all reasonable facilities so far as their instructions permitted.

The first step to be taken is to put our testing on a systematic and satisfactory basis.

The second is to establish some means whereby metal which has been tested can have its quality indicated upon it in such manner that it can be practically relied upon.

The experiments before referred to establish, sufficiently for all practical purposes, that the relation or proportion between the resistances to tension, compression, torsion, and transverse strain, is about the same in steel as in wrought-iron.

The testing required is therefore reduced to that necessary for ascertaining two properties, namely the strength and the toughness or ductility.

The strength may be readily ascertained, and no difficulty arises on that head.

The whole question turns upon the test for ductility, or the resistance to fracture by blows or sudden strain; and it must be admitted that the tests employed for this purpose are not framed on any regular or satisfactory basis.

Without, however, attempting to say what description of test may be found the best for ascertaining the property of ductility, it may be observed that what is required for this test is a definite basis to act upon, and that the samples should be so made as to render the test cheap, expeditious, and easy of application.

The next requirement is that when a piece of metal has been tested, and its qualities of strength and toughness ascertained, there should be some means of denoting its quality in an authentic manner.

To a certain extent this is already done in iron by the mark of the maker; but something more than this is necessary to fulfil the required conditions in steel.

What is termed steel, is iron with a small proportion of carbon in it. These two ingredients are necessary to constitute steel; and there may or may not be present in very small quantities graphite, silicon, manganese, sulphur, and phosphorus.

In connection with the experiments made by the Committee, fourteen of the samples were tested by Mr. E. Richards, of the Barrow Steel Works, five of which were kindly repeated by Dr. Odling.

Although there are some discrepancies in the results which we cannot account for, yet some of the characteristics are brought out clearly.

It appears that manganese may be present to the extent of four-tenths per cent. without injury either to the strength or ductility, but sulphur and phosphorus, except in extremely small quantities, are fatal to ductility.

In the samples tried by the Committee and Mr. Kirkaldy, the quantity of carbon varied from ¼ per cent. to nearly 1 per cent.; yet with this small variation in the carbon the strength ranged from thirty-three tons to nearly fifty-three tons per in.; and the ductility, represented by the ratio which the fractured area bore to the original section of the bar, varied from five-tenths in the tough qualities, until in the harder samples there was no diminution perceptible.

All these materials are called steel, and have the same external appearance; but possessing, as they do, such a range of strength and such a variation in ductility, it becomes absolutely essential that there should be some classification or means of knowing the respective qualities among them.

The want of such classification casts an air of uncertainty over the whole question of steel, and impedes its application. To this want of knowledge is to be ascribed the circumstance that many professional men regard the material as altogether unreliable; while large consumers of steel, in consequence of the uncertainty of the quality they buy in the market, seek to establish works on their own premises and make their own steel.

I ought, I know, to apologise for detaining you so long on this one question of steel, but I consider that the difficulties under which it is placed are affecting interests of considerable importance.

Not only is a large and useful field for the employment of steel practically closed, but the progress of improvement in engineering structures is impeded both in this country and in other parts of the world where English engineers are engaged.

For in consequence of the impediments to its employment in England, very few English engineers turn their attention to the use of steel. They are accustomed to make their designs for iron, and when engaged in works abroad where the Board of Trade rules do not apply, they continue for the most part to send out the old-fashioned ponderous girders of common iron, in cases where the freight and difficulties of carriage make it extremely desirable that structures of less weight and more easy transport should be employed.

In conclusion, and while thanking you for the patience with which you have heard me on this subject, I would observe that we possess in steel a material which has been proved, by the numerous uses to which it is applied, to be of great capability and value: we know that it is used for structural purposes in other countries, as, for example, in the Illinois and St Louis Bridge in America, a bridge of three arches, each 500 ft. span; yet in this country, where "modern steel" has originated and has been brought to its present state of perfection, we are, obstructed by some deficiency in our arrangements, and by the absence of suitable regulations by the Board of Trade, from making use of it in engineering works.

And I have considered it right to draw your attention to the position in which this question stands, well knowing that I could not address any body of gentlemen more capable of improving and systematising our methods of testing, or better able to devise effectual means for removing the impediments to the use of steel, than are to be found in the scientific and practical men who form the Mechanical Section of the British Association.

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THURSDAY, SEPTEMBER 25, 1873

AFRICAN TRAVEL

The Lands of Cazembe. Lacerda's Journey to Cazembé in 1798. Translated and annotated by Captain R. F. Burton, F.R.G.S.; also, Journey of the Pombeiros, P. J. Baptista and Amaro José across Africa from Angola to Tette on the Zambize. Translated by B. A. Beadle; and a *Résumé* of the Journey of MM. Monteiro and Gamitto. By Dr. C. T. Beke. (Published by the Royal Geographical Society; John Murray, 1873.)

The African Sketch Book. By Winwood Reade, with maps and illustrations, in two volumes. (Smith, Elder and Co., 1873.)

THESE are extremely different kinds of books, though both are valuable. The first is almost unreadable except by geographical students; the second is thoroughly popular and amusing. The pending explorations of Livingstone have given a special interest to the various journeys of Portuguese explorers, and the Royal Geographical Society have done well in making the records of these journeys accessible to English readers. The earliest and most important is that of Dr. De Lacerda, who went on a Government mission to the capital of Cazembé, situated at the southern extremity of Lake Moero, about 500 miles north-west of Lake Nyassa. He died on the way, but the journey was concluded under the second in command. The Journal is given at length, and is very dull reading, except for the insight it gives into the character of the numerous Portuguese and half-castes who accompanied the expedition, and who were in a continual state of squabble from the first day to the last. Dr. De Lacerda was evidently an amiable and intelligent man, and his notes are comparatively pleasant reading, and give some little notion of the country and the people. The Journal of his successor, an ecclesiastic (Fr. Pinto), is, however, so exclusively occupied with a record of the disputes among the members of the expedition, that it was hardly worth printing. Capt. Burton's translation is very free, and no doubt very accurate, but he is so idiomatic as almost to require translating himself; and such terms as "loot," "dash," "notions," and "magotty heads," which are repeatedly used, are hardly characteristic of the serious and matter-of-fact diary of the Portuguese explorers. His notes are very copious, often considerably exceeding the text, and some of them are instructive; but we find in them too many onslaughts on Mr. Cooley, and endless minute criticisms on African orthography. The free statement of Capt. Burton's peculiar views on civilisation, religion, polygamy, and other matters, is also rather out of place. We are told for instance that, to Capt. Burton, "Alexander is the first person of the triad which humanity has as yet produced; the other two being Julius Cæsar and Napoleon Bonaparte," and that "Blakeley guns and railways" are the indices of true progress.

If, however, this part of the book is dull, the second part—the Route Journal of the Pombeiros—is dreary in the extreme. We have page after page of such entries as these:—"Friday, 12th—At seven in the morning we got up and left the top of the hill. We passed seven narrow streams which run into the Luapula. We came to another desert near a narrow river where we found a circle made. We met nobody and walked with the sun in our front." In the third part we are spared the detailed journals and are given a *résumé* by Dr. Beke, in which we have all that is of interest compressed into a few pages. These journals show that African travel was beset with the same difficulties and troubles seventy years ago as it is now, and that the custom of exacting presents and causing delays at every village is an ancient African institution. The work is illustrated by an excellent map, in which all the geographical information to be extracted from these journeys is laid down, and the routes of all the travellers, as well as those of Livingstone, distinctly marked. It will therefore be of great value in tracing the future progress of that illustrious traveller.

Mr. Winwood Reade's well-named "African Sketch Book" is a work of an altogether novel kind. In a series of picturesque and sparkling chapters he gives us sketches of the various pictures of African life and scenery, episodes of travel, the slave trade, the history of African exploration, and other subjects; and interspersed with these are little tales illustrative of the various phases of native life or of European life in Africa. Mr. Reade has twice visited Africa. The first time, in 1862-63, he went over Du Chaillu's ground, and enabled us to separate the true from the imaginative in that traveller's book; and he also visited Angola and Senegambia. The second time, in 1868—70, he spent two years in Africa, on the Gold Coast and Liberia, and made an adventurous journey from Sierra Leone to the Niger, at a point never before reached by a European traveller. The narrative of this journey occupies about half the second volume, and is very interesting; although it is perhaps a little marred by the sketchy style in which it is written (in the form of letters to a young lady), and by the prominence given to the author's fears, hopes, and ambitions, all of which will, however, prove attractive to many readers. When within about fifty miles of the Niger, at Falaba, the traveller was stopped by a native king, Sewa, who kept him in his court, as Speke was kept, for several months, and then allowed him to return to Sierra Leone, sending with him an embassy and his own nephew, as an escort. Mr. Reade then endeavoured to get the Governor of Sierra Leone to send him on an expedition to the Niger, in which case Sewa would not have dared to stop him; but finding that there would be great delays before this could be arranged, he took the bold resolution, although seriously ill, to return at once with the king's nephew He did so, and telling the king, who was greatly surprised to see him, that he was now a traveller going to the Niger, but would stay with him three days, he was allowed to go on, and not only succeeded in reaching the Niger at a point about forty miles from its source, but went down its course to the north-east to the Bouré gold works, never before visited by any European. This journey undoubtedly stamps Mr. Reade as a thorough African explorer.

The six years' interval between his two journeys was devoted to a study of the literature of African travel, some of the results of which are embodied in a large and very useful map, showing at a glance the portion of the country visited by each traveller, as well as the various authorities which may be consulted on each district; and the comparative importance of these is indicated by the type in which the name is printed. The chapter entitled "The African Pioneers," is a very interesting one, giving a spirited sketch of the life and labours of each of the important African travellers from Ledyard to Livingstone; and we think Mr. Reade could do no better or more popular work than to give us in a compact and readable form, and as much as possible in each author's own words, the concentrated essence of those vast piles of volumes on Africa, which he appears to have waded through.

There is a very great improvement in this work over Mr. Reade's earlier writings, and he himself recognises that his opinions are now changed for fairer and truer ones. He now speaks of the Negro race with respect, and often uses the term "native gentleman." He believes that "if boys were removed at an early age from uncivilised society and brought up with the sons of gentlemen at home, they would acquire something better than book-learning—namely the sentiment of honour. My long and varied experience of the African Race has brought me to believe that they can be made white men in all that is more than skin-deep." He speaks well of the native Missionaries, and says of one of them at Sierra Leone, of whom he saw a good deal, that he "does not differ, so far as I can see, from an English gentleman and clergyman in manners, speech, or disposition." Such men have far more influence with the natives than English clergymen can have. "An ordained Negro is a walking sermon, a theological advertisement. The savages regard an Oxford Master of Arts as a being fearfully and wonderfully made, belonging to a different species from himself. His argument invariably is, 'White man's God, he good for white man; black man's God, he good for black man.' But when he beholds a man as black as himself with a shiny hat, a white cravat, glossy garments, and shoes a yard long, wearing a gold watch in his fob, blowing his nose in a cloth, and 'making leaves speak;' and when he is informed that these are the results of being baptised, he also aspires to become a white man, and allows himself to be converted."

Good service is done by pointing out that what is usually called the typical Negro with jet-black skin, thick lips, and flat nose, is by no means typical, but is an extreme and exceptional type; that coffee colour of various shapes is the characteristic colour of Negroes, that their features are often finely formed, and of quite a European cast. Blackness of skin is said to be most prevalent where heat and moisture are combined, but it is recognised that this is not necessarily, or even probably, the cause of the blackness.

Mr. Reade's book is full of brilliant or witty sayings. Of the gorilla he says that "there is little doubt that some day or other this renowned ape will make its appearance at the Zoological Gardens, to brighten the holiday of the artisan, and to alleviate the sabbath of the fashionable world." Relating how a man once refused to guide him to a plantation about three miles off, for fear he should kill some game on the way and compel him to carry it, he remarks, "And yet it is often asserted that the Negroes are incapable of foresight." The natives of the interior firmly believe that Europeans buy slaves to eat, and an old cannibal Fan was anxious to know why they took the trouble to send so far for people to eat. Were the black men nicer than the white men? Mr. Reade's answer was dictated by motives of policy, as he was in a cannibal country. He assured his questioner that white men's flesh was a deadly poison, and so they were obliged to import their supplies! Of Livingstone it is remarked that "only twice in his life since he was a youth has he visited England, returning after a while to his true home in the wilderness, with his health shattered by the toils of literary composition."

We find also many passages of good or of doubtful philosophy. Mr. Reade seems impressed with the strange idea that if we could by any means double the number of our tall chimneys in the cotton districts, we should necessarily advance our civilisation and benefit the human race. For example, among arguments for opening up the Niger we are told:—"The country which lies beyond the confluence of the Quorra and the Binué is one of the largest cotton-growing areas of the world. At present the people dress themselves. But when the Niger trade is once established, our cheap cotton goods will soon destroy the native industry, and the people will export their raw cotton instead of weaving it themselves." And as one of the main results of the blood and treasure expended on African soil, we are told that "new markets have been opened for British manufactures." But does it not occur to Mr. Reade, that to destroy native industries instead of improving them may not advance a people; and that to increase the already large proportion of our population who pass their lives in a monotonous routine amid the smoke of furnaces and the din of machinery, and helpless as infants if their own source of living fails them (as it has failed them and may again), may not really advance us on the road to civilisation?

As an example of the manner in which our author often compresses into a few lines the results of much labour, take the following passage summarising the results of Nile exploration and the relative share of the two great branches in forming the River Nile and the Land of Egypt:—"Thus the Nile is created by the rainfall of the Equator, and Egypt by the rainfall of the Tropics. If the White Nile did not exist, the Black Nile would be nothing—it would perish in the sand. But if the Black Nile did not exist, the White Nile would be merely a barren river in a sandy plain, with some Arab encampments on its banks."

The arrangement of this book seems to be its weakest point. We are taken up and down the coast, and back again over old ground, till we hardly know where we are; and the confusion is increased by the insertion of the illustrative tales in the body of the work. It would have been far better if these tales had been kept together, and the rest of the work arranged in systematic geographical order. The work is provided with numerous good woodcuts; and the maps, which illustrate in a novel and ingenious manner the slave trade, the religions of Africa, African discovery, and African literature, are very valuable. The tales themselves are clever, and some admirably illustrative of African life; but most of them are melancholy in their catastrophes, and indicate that the author takes a somewhat gloomy view of human life and human nature. Of these, "Ananga" is the best. It is the story of a daughter of the King of Cazembé, who marries a Portuguese officer and runs away with him; and, arriving in the Cape Colony, is so overwhelmed by the rush of new ideas excited by one after another of the wonders of civilisation, that she dies, like the Lady of Burleigh, overcome

> "By the burthen of an honour unto which she was not born."

It is altogether a charming story, and is written in a style which we hope Mr. Reade will cultivate.

In justice to the author, it must be stated that the present work is intended for family reading, and to popularise a knowledge of modern Africa. He promises a more serious book, treating of many subjects in connection with the native races, of great interest to students of man; and this will be looked forward to with interest, since few men are now better qualified than Mr. Reade, both by travel and study, to tell us the real truth about the Negro.

ALFRED R. WALLACE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Tait and Tyndall

[WE have received further communications from Professors Tyndall and Tait on the subject of the correspondence that has appeared in our columns. We feel that we are only consulting the true interests of Science in declining to print further communications on a subject which has assumed somewhat of a personal tone, and in this idea we are supported by many of the best friends of both parties, who, however, will approve of our giving the following brief extract from Dr. Tyndall's communication:—"My letter was rapidly written, and the proof of it reached me, not on the Tuesday evening, as I expected, but on the Wednesday morning when I was in the midst of my preparations for Bradford. I had therefore little time to give it the calm thought which it ought to have received. On re-reading it I find two passages in it which I think it desirable to cancel. The first is that in which I speak of lowering myself to the level of Prof. Tait; the second that in which I reflect upon his manhood. These passages I wish to retract."—Ed. NATURE.]

On the Males and Complemental Males of certain Cirripedes, and on Rudimentary Structures.

I BEG permission to make a few remarks bearing on Prof. Wyville Thomson's interesting account of the rudimentary males of *Scalpellum regium*, in your number of August 28th. Since I described in 1851, the males and complemental males of certain cirripedes, I have been most anxious that some competent naturalist should re-examine them; more especially as a German, without apparently having taken the trouble to look at any specimens, has spoken of my description as a fantastic dream. That the males of an animal should be attached to the female, should be very much smaller than, and differ greatly in structure from her, is nothing new or strange. Nevertheless, the difference between the males and the hermaphrodites of *Scalpellum vulgare* is so great, that when I first roughly dissected the former, even the suspicion that they belonged to the class of cirripedes did not cross my mind. These males are half as large as the head of a small pin; whereas the hermaphrodites are from an inch to an inch and a quarter in length. They consist of little more than a mere sack, containing the male reproductive organs, with rudiments of only four of the valves; there is no mouth or alimentary canal, but there exists a rudimentary thorax with rudimentary cirri, and these apparently serve to protect the orifice of the sack from the intrusion of enemies. The males of Alcippe and Cryptophialus are even more rudimentary; of the seventeen segments which ought to be fully developed, together with their appendages, only three remain, and these are imperfectly developed; the other fourteen segments are represented by a mere slight projection bearing the probosci-formed penis. This latter organ, on the other hand, is so enormously developed in Cryptophialus, that when fully extended it must have been between eight and nine times the length of the animal! There is another curious point about these little males, viz., the great difference between those belonging to the several species of the same genus Scalpellum: some are manifestly pedunculated cirripedes, differing by characters which in an independent creature would be considered as of only generic value; whereas others do not offer a single character by which they can be recognised as cirripedes, with the exception of the cast-off prehensile, larval antennæ, preserved by being buried in the natural cement at the point of attachment. But the fact which has interested me most is the existence of what I have called Complemental Males, from their being attached not to females, but to hermaphrodites; the latter having male organs perfect, although not so largely developed as in ordinary cirripedes. We must turn to the vegetable kingdom for anything analogous to this; for, as is well known, certain plants present hermaphrodite and male individuals, the latter aiding in the cross-fertilisation of the former. The males and complemental males in some of the species of three out of the four very distinct genera in which I have described their occurrence, are, as already stated, extremely minute, and, as they cannot feed, are short-lived. They are developed like other cirripedes, from larvæ, furnished with well-developed natatory legs, eyes of great size and complex prehensile antennæ; by these organs they are enabled to find, cling to, and ultimately to become cemented to the hermaphrodite or female. The male larvæ, after casting their skins and being as fully developed as they ever will be, perform their masculine function, and then perish. At the next breeding season they are succeeded by a fresh crop of these annual males. In *Scalpellum vulgare* I have found as many as ten males attached to the orifice of the sack of a single hermaphrodite; and in Alcippe, fourteen males attached to a single female.

He who admits the principle of evolution will naturally inquire why and how these minute rudimentary males, and especially the complemental males, have been developed. It is of course impossible to give any definite answer, but a few remarks may be hazarded on this subject. In my "Variation under Domestication," I have given reasons for the belief that it is an extremely general, though apparently not quite universal law, that organisms occasionally intercross, and that great benefit is derived therefrom. I have been laboriously experimenting on this subject for the last six or seven years, and I may add, that with plants there cannot be the least doubt that great vigour is thus gained; and the results indicate that the good depends on the crossed individuals having been exposed to slightly different conditions of life. Now as cirripedes are always attached to some object, and as they are commonly hermaphrodites, their intercrossing appears, at first sight, impossible, except by the chance carriage of the spermatic fluid by the currents of the sea, like pollen by the wind; but it is not probable that this can often happen, as the act of impregnation takes place within the well-enclosed sack. As, however, these animals possess a probosci-formed penis capable of great elongation, two closely attached hermaphrodites could reciprocally fertilise each other. This, as I have elsewhere proved, does sometimes, perhaps often, actually occur. Hence perhaps it arises, that most cirripedes are attached in clusters. The curious Anelasma, which lives buried in the skin of sharks in the northern seas, is said always to live in pairs. Whilst reflecting how far cirripedes usually adhered to their support in clusters, the case of the genus Acasta occurred to me, in which all the species are embedded in sponges, generally at some little distance from each other; I then turned to my description of the animal, and found it stated, that in several of the species the proboscl-formed penis is "remarkably long;" and this I think can hardly be an accidental coincidence. With respect to the habits of the genera which are provided with true males or complemental males:—all the species of Scalpellum, excepting one, are specially modified for attachment to the delicate branches of corallines: the one species of Ibla, about which I know anything, lives attached, generally two or three together, to the peduncle of another cirripede, viz. a Pollicipes: Alcippe and Cryptophialus are embedded in small cavities which they excavate in shells. No doubt in all these cases two or more full-grown individuals might become attached close together to the same support; and this sometimes occurs with *Scalpellum vulgare*, but the individuals in such groups are apt to be distorted and to have their peduncles twisted. There would be much difficulty in two or more individuals of Alcippe and Cryptophialus living embedded in the same cavity. Moreover, it might well happen that sufficient food would not be brought by the currents of the sea to several individuals of these species living close together. Nevertheless in all these cases it would be a manifest advantage to the species, if two individuals could live and flourish close together, so as occasionally to intercross. Now if certain individuals were reduced in size and transmitted this character, they could readily be attached to the other and larger individuals; and as the process of reduction was continued, the smaller individuals would be enabled to adhere closer and closer to the orifice of the sack, or, as actually occurs with some species of Scalpellum and with Ibla, within the sack of the larger individual; and thus the act of fertilisation would be safely effected. It is generally admitted that a division of physiological labour is an advantage to all organisms; accordingly, a separation of the sexes would be so to cirripedes, that is if this could be effected with full security for the propagation of the species. How in any case a tendency to a separation of the sexes first arises, we do not know; but we can plainly see that if it occurred in the present case, the smaller individuals would almost necessarily become males, as there would be much less expenditure of organic matter in the production of the spermatic fluid than of ova. Indeed with *Scalpellum vulgare* the whole body of the male is smaller than a single one of the many ova produced by the hermaphrodite. The other and larger individuals would on the same principle either remain hermaphrodites, but with their masculine organs more or less reduced, or would be converted into females. At any rate, whether these views are correct or not, we see at the present time within the genus Scalpellum a graduated series: first on the masculine side, from an animal which is obviously a pedunculated cirripede with well-proportioned valves, to a mere sack enclosing the male organs, either with the merest rudiments of valves, or entirely destitute of them; and secondly on the feminine side, we have either true females, or hermaphrodites with the male organs perfect, yet greatly reduced.

With respect to the means by which so many of the most important organs in numerous animals and plants have been greatly reduced in size and rendered rudimentary, or have been quite obliterated, we may attribute much to the inherited effects of the disuse of parts. But this would not apply to certain parts, for instance to the calcareous valves of male cirripedes which cannot be said to be actively used. Before I read Mr. Mivart's acute criticisms on this subject, I thought that the principle of the economy of growth would account for the continued reduction and final obliteration of parts; and I still think, that during the earlier periods of reduction the process would be thus greatly aided. But if we consider, for instance, the rudimentary pistils or stamens of many plants, it seems incredible that the reduction and final obliteration of a minute papilla, formed of mere cellular tissue, could be of any service to the species. The following conjectural remarks are made solely in the hope of calling the attention of naturalists to this subject. It is known from the researches of Quetelet on the height of man, that the number of individuals who exceed the average height by a given quantity is the same as the number of those who are shorter than the average by the same quantity; so that men may be grouped symmetrically about the average with reference to their height. I may add, to make this clearer, that there exists the same number of men between three and four inches above the average height, as there are below it. So it is with the circumference of their chests; and we may presume that this is the usual law of variation in all the parts of every species under ordinary conditions of life. That almost every part of the body is capable of independent variation we have good reason to believe, for it is this which gives rise to the individual differences characteristic of all species. Now it does not seem improbable that with a species under unfavourable conditions, when, during many generations, or in certain areas, it is pressed for food and exists in scanty numbers, that all or most of its parts should tend to vary in a greater number of individuals towards diminution than towards increment of size; so that the grouping would be no longer symmetrical with reference to the average size of any organ under consideration. In this case the individuals which were born with parts diminished in size and efficiency, on which the welfare of the species depended, would be eliminated; those individuals alone surviving in the long run which possessed such parts of the proper size. But the survival of none would be affected by the greater or less diminution of parts already reduced in size and functionally useless. We have assumed that under the above stated unfavourable conditions a larger number of individuals are born with any particular part or organ diminished in size, than are born with it increased to the same relative degree; and as these individuals, having their already reduced and useless parts still more diminished by variation under poor conditions, would not be eliminated, they would intercross with the many individuals having the part of nearly average size, and with the few having it of increased size. The result of such intercrossing would be, in the course of time, the steady diminution and ultimate disappearance of all such useless parts. No doubt the process would take place with excessive slowness; but this result agrees perfectly with what we see in nature; for the number of forms possessing the merest traces of various organs is immense. I repeat that I have ventured to make these hypothetical remarks solely for the sake of calling attention to this subject.

CHARLES DARWIN

Down, Beckenham, Kent, Sept. 20

Reflection of the Rainbow

DRAW a circle to represent a rain-drop, or rather a section of it, by a plane passing through its centre, the sun, and the eye. Draw a straight line through the centre to represent a solar ray of mean refrangibility. At the front and back of the drop reflection occurs, and the incidence being normal, the incident and reflected beams will coincide after the emergence of the latter from the drop. Now suppose the ray through the centre to move parallel to itself, the incidence grows more and more oblique, refraction occurs at entrance and at emergence, the ray finally becoming a tangent to the drop. Let the incident and the twice refracted and once reflected rays be produced backwards till they intersect behind the drop: the angle enclosed between them augments with the obliquity, reaches a maximum, and then diminishes. The ray corresponding in obliquity with this maximum angular value, and those in its immediate vicinity, quit the drop sensibly parallel, and these are the rays which are effectual in the rainbow. This angle being for red light 42°, and for violet light 40°, for light of mean refrangibility it is 41°.

If those parallel rays before reaching the observer's eye impinge upon a surface of calm water, they are, in part, reflected according to the usual law, and *a* rainbow is then seen by reflection. But the absolute position of the bow changes with every change in the position of the observer's eye; hence the bow seen mirrored in the pool is *not* the reflection of that seen at the same time directly in the heavens Suppose the shower to be fixed in space, then the drops which produce the bow seen directly, would not be those which produce the bow as seen by reflection.

In the paragraph to which your correspondent "Z.X.Y." has called attention, I meant to combat the notion, entertained by many, that the rainbow is reflected after the fashion of an ordinary floating cloud which emits light in all directions, and which, by the light thus emitted, paints its image in the water. A few additional words might have made my meaning clearer; but as I was dealing at the time more with historic statement than with scientific exposition, I desired to be brief. I can hardly think, however, that your correspondent will be angry with me for giving him what must have been agreeable as well as successful occupation at the Falls of the Rhine.

Royal Institution, Sept. 15 JOHN TYNDALL

Original Research at the Universities

MY attention has been arrested by the following sentence in the extract given by you from Prof. Frankland's evidence before the Science Commission:—"I believe that one cause (of the slow progress of original research in England) lies in the entire non-recognition of original research by any of our Universities. Even the University of London, which has been foremost in advancing instruction in experimental science, gives its highest degree in Science without requiring any proof that the candidate possesses the faculty of original research, or is competent to extend the boundaries of the science in which he graduates."

It may interest Dr. Frankland and those who take the same view as he does, to know that this subject has engaged the attention of the graduates of the University of London. At a meeting of the Annual Committee of Convocation in December last, it was moved by Prof. Guthrie—

"That every candidate for the degree of Doctor of Science shall be required to submit to his respective Examiners a written dissertation embodying some original research in one or more of the subjects of his intended examination; and that such dissertation be approved before the candidate be allowed to proceed to examination."

This motion I had the honour of seconding; but the degree of acceptance which the principle involved in it met with from the Committee is seen by the sequel, as stated in the printed minutes, that it was "rejected by a large majority." The exact numbers, if my memory serves me rightly, were Ayes, 3; Noes, 16; among the Noes were two Doctors and one Bachelor of Science, and at least five Doctors of Medicine. The "Annual Committee," it may be stated, is a representative body elected annually by the graduates in Convocation, but has no legislative or administrative power, this resting entirely with the Senate.

ALFRED W. BENNETT

Endowment of Research

WITH regard to the Endowment of Scientific Research, could not this be well placed in the hands (as it now is, to a very limited extent) of a Committee of the British Association? the committee being authorised to supply funds for experimental purposes, and the members, say three or four in number, to have a permanent salary for the time spent in the examination of claims from applicants.

It might possibly be desirable that one or more of the committee should retire every two or three years and not be eligible for re-election until after the lapse of three years; and also, to prevent waste of time, that all applications for help should be presented only through one or more gentlemen of known scientific attainments, and not of necessity at the instigation of the person to whom the assistance was to be rendered. I believe that this would be a good practical arrangement as regards the poorer class, who are compelled to throw up valuable original researches to supply themselves and those depending on them with homes and food.

The abuse of a trust of this kind would hardly be possible, as the help would of necessity be given in those cases where a certain amount of work had already been done under difficulties, and where the natural instinct for original research was of necessity strongly developed. The presentation of an annual sum for, say five years, renewable at the end of the time if necessary, would be a godsend to many a man who has allowed himself to starve for the benefit of posterity.

THOS. FLETCHER

FERTILISATION OF FLOWERS BY INSECTS [*]

III.

On the co-existence of two forms of flowers in the same species or genus,—a more conspicuous one adapted to cross-fertilisation by insects, and a less conspicuous one adapted to self-fertilisation.

SINCE Darwin, in his admirable work on Orchids,[†] had proved that the flowers of this family are endowed with an immense variety of comtrivances for cross-fertilisation by insects, it was almost generally admitted by botanists that cross-fertilisation is the rule throughout the whole vegetable kingdom. Darwin's well-known aphorism, that "Nature abhors perpetual self-fertilisation" was exaggerated by his successors in this field of research, Hildebrand in Germany and Delpino in Italy, who, in their various elaborate memoirs on the fertilisation of flowers, repeatedly expressed their strong belief that nature abhors self-fertilisation at all. In direct opposition to this opinion, Axell [‡] propounded the doctrine that the development of the fertilising arrangements in phanerogams has been always an advance, and still continues to advance, in one and the same direction, towards a perfection which affords more and more facilities for self-fertilisation.

My own observations on the contrivances of our flowers and on the insects really visiting and fertilising them, have convinced me, that neither Hildebrand's and Delpino's, nor Axell's opinion is a thoroughly adequate one, but that under certain conditions the facility for self-fertilisation is most advantageous to a plant, while, under other conditions, the inevitableness of cross-fertilisation by the visits of insects is the more advantageous.

To all plants the flowers of which possess such a degree of attractiveness for insects that cross-fertilisation by these transporters of pollen is never wanting, the possibility of self-fertilisation is quite useless, and from this cause, not being subjected to the effects of natural selection, may be lost, like any useless peculiarity, and in many instances, indeed, has been lost. On the contrary, to those plants the flowers of which possess so slight a degree of attractiveness for insects, that the transportation of the pollen to the stigma by insects is effected in but very few cases, the possibility of self-fertilisation is most advantageous, and indeed we find in most cases such plants well adapted for self-fertilisation.

Among many facts which I could appeal to as proofs of my statements, there are, I believe, none more instructive than those alluded to in the superscription of this article.

In some species of our wild plants I have found on different plants two different forms of flowers, evidently showing the connection above stated between attractiveness for insects and adaptation for inter-crossing or for self-fertilisation. As nobody before, for aught I know, has observed this phenomenon, I will give some details of the most important instances hitherto observed.

Lysimachia vulgaris

Of this species specimens with more conspicuous flowers are found in sunny localities. The petals of this form are dark yellow with red at the base, on an average about 12 mm. long, and 6 mm. wide, opening widely and bending outwards and backwards; the filaments are red-coloured towards their end; the style overtops the longest stamens by some millimetres. A species of bee, *Macropis labiata* Pz., frequently visits these flowers for pollen. It comes first into contact with the stigma, and supplies it with pollen from previously visited flowers, thus regularly effecting cross-fertilisation. But if we prevent the visits of insects by covering over the stems by a net, self-fertilisation scarcely takes place, in consequence of the style overtopping all the stamens.

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[*] Continued from p. 406.

[†] "On the Various Contrivances by which British and Foreign Orchids are Fertilised by Insects." (London, 1863)

[‡] In his work: "Om anordningarna för fanerogama växternas befruktning." (Stockholm, 1869)

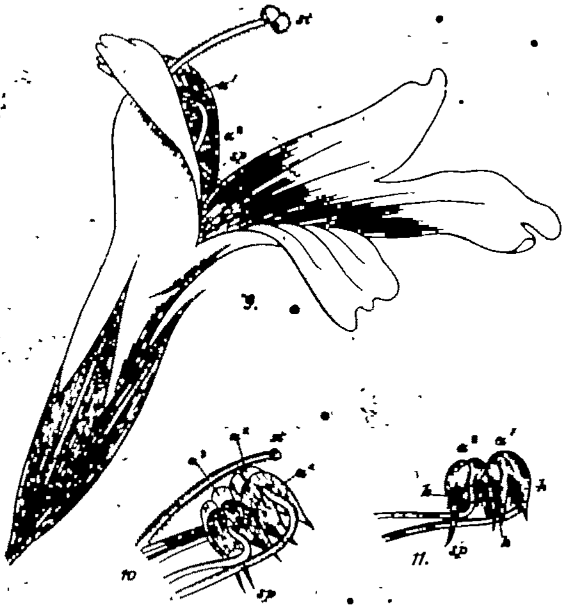

FIG. 9.—*Euphrasia officinalis.* Lateral view of a flower of the largest form, just opened

FIG. 10—Position of the stigma (*st*), and of the anthers (*a¹*, *a²*) of the same flower in a more advanced state.

FIG. 11.—Two anthers, seen from the inner side, showing the slits fringed with hairs.

Specimens of the same species with less conspicuous flowers are found in shady ditches. The petals of these plants are lighter yellow, uniform in colour, without any red at the base, on an average 10 mm. long, and 5 mm. wide; they only open slightly, remaining nearly upright, but diverging obliquely; the filaments are greenish yellow, without any red towards their end; the style hardly equals the two lowest and longest stamens. The stigma comes without any external agency into contact with the pollen of the same flower thus regularly experiencing self-fertilisation. This manner of producing seeds is an indispensable condition for preservation of this variety of *Lysimachia vulgaris.* For in consequence of its shady habitat, and of its lower degree of attractiveness for insects, its flowers are but very rarely visited, and it would be exposed to extinction without the possibility of propagation by self-fertilisation. I but once observed a little fly of the family of Syrphidæ, *Syritta pipiens* L., eating the pollen of this shady form of flowers. Although this fly might possibly transport pollen from one flower to the stigma of another, cross-fertilisation was nevertheless by no means more probable than self-fertilisation.

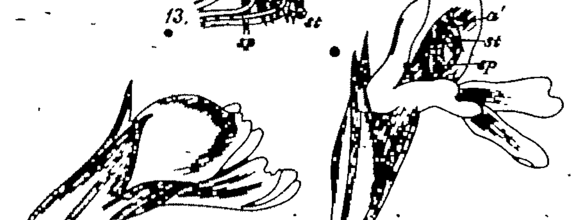

FIG 12—Lateral view of a flower of the smallest form, just opening.

FIG. 13—Position of the stigma (*st*) and the anthers (*a¹*, *a²*) in this flower.

FIG 14.—Front view of a flower of the same form, in a more advanced state.

(All the figures are magnified in the proportion of 7 : 1. The hairs of the calyx and the coloured spots and lines of the corolla are omitted.)

The two forms here described of the flowers of *Lysimachia vulgaris* graduate into each other by connecting forms, which are met with in intermediate localities, for instance on the sunny edges of ditches.

Another example of the same sort of dimorphism, even more striking than that just mentioned, is presented by *Euphrasia officinalis.* Of this species flowers are found in different localities of a very different size. But the more the attractiveness for insects is increased by the size of the corolla, the more is cross-fertilisation secured in case insects visit the flowers, self-fertilisation at the same time being prevented; while on the contrary, the smallest flowers regularly fertilise themselves, even without the visits of insects. I will attempt to explain these peculiarities by drawings of the largest and of the smallest form of flowers I have hitherto been able to find.

In the flower just opened of the largest form (as shown in Fig. 9), the stigma, already in a mature condition, greatly overtops the anthers. Therefore an insect,* inserting its proboscis into the tubular corolla in order to gain the nectar contained at the bottom of its tube, first grazes the stigma charging it with pollen-grains from flowers previously visited, and then pushes against the two hairs (*sp*) which project from the two lower anthers (*a²*) into the middle of the entrance to the corolla. This shaking of the hairs is transmitted to all the four anthers, which lie close together and are soldered together by their upper margins, and a small quantity of the smooth powdery pollen-grains falls out of all the pollen sacs. The slits in the pollen sacs being fringed with hairs directed downwards (as shown in Fig 11) a lateral dispersion of the pollen-grains is prevented; all the pollen-grains shaken out fall directly downwards upon the proboscis, enabling it to fertilise the next flower visited by the insect.

In the state just described the corolla has not yet attained its full size. Growing farther, it at length equals the stigma by which it was at first so much overtopped, and now the mutual position of the stigma and the anthers is that shown in Fig. 10. When occupying this position, the stigma is always already shrivelled and brownish coloured, and is no longer capable of being fertilised. Self-fertilisation is therefore quite impossible.

The probability of cross-fertilisation and of self-fertilisation is directly opposite in the flowers of the smallest form, presented by Fig. 12—14. Whilst in the flowers of the largest form, as just described, the anthers remain soldered together, and do not scatter their pollen unless the hairs are shaken, in the flowers of the smallest form the anthers separate from each other, and scatter nearly all their pollen long before the corolla has fully opened. The end of the style, moreover, bends inwards so much as to bring the stigma (as Fig. 13 shows) close beneath the upper anthers. Therefore, on examining a flower hardly half-opened (Fig. 12), we always find the stigma already largely charged with pollen-grains of the same flower. When fully opened, the flowers of the smallest form show the stigma in a shrivelled and brownish coloured condition, lying between the separated and emptied pollen-sacs (as shown in Fig. 14). Hence cross-fertilisation could scarcely be effected, even if insects (which I never

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[*] I observed four species of bees and three species of Diptera visiting the flowers of *Euphrasia officinalis* for honey.

observed) should visit these very inconspicuous flowers. The fringing hairs in the flowers of the largest form, so nicely securing the perpendicular falling of the pollen-grains upon the proboscis, are quite useless in a flower regularly restricted to self-fertilisation; indeed, in the anthers of the smallest form we find no fringing hairs at all, or only a few isolated ones.

The two extreme forms here described graduate into each other by various intermediate forms. When publishing my book on "Fertilisation of Flowers by Insects," I had never observed either the largest or the smallest form here described. From this cause the figures in page 291 of my work, drawn from other varieties, differ in some points from the description here given.

In *Lysimachia vulgaris* the two forms here described are so closely allied, that no botanist, for aught I know, has considered them worthy of being distinguished as varieties by separate names; in *Euphrasia officinalis* the difference between the two forms is somewhat greater, and some botanists, although overlooking the different manner of fertilisation, have distinguished them as varieties, (for instance, Ascherson in his "Flora der Provinz Brandenburg").

In a third example of the same dimorphism of flowers, presented by *Rhinanthus crista galli*, the divergence of the two forms has proceeded so far that most botanists distinguish them by separate names, some as varieties (*Rh. crista galli* α and β of Linnæus), others as distinct species (*Rh. major* Ehrh. and *Rh. minor* Ehrh). These two forms differ with respect to their fertilisation, nearly in the same manner as the largest and the smallest form of *Euphrasia officinalis*. *Rh. minor* having a smaller corolla, and therefore being but rarely visited by insects, regularly fertilises itself when insects do not visit it, by bending the stigma beneath the pollen-sac, which at last opens spontaneously, and covers the stigma with its pollen-grains. In *Rh. major* the stigma so far overtops the pollen-sac that self-fertilisation is excluded. It is, however, a remarkable difference between *Rh. minor* and the smallest form of *Euphrasia officinalis*, that the former is regularly cross-fertilised, when visited by insects, if this happens not too late, and that it only has recourse to self-fertilisation if altogether unvisited by insects.*

Lippstadt, Sept. 9 HERMANN MULLER

THE 'POLARIS' ARCTIC EXPEDITION

THE missing link in the story of the *Polaris* Expedition has been picked up, and the narrative, as a whole, is one of the strangest in the whole history of Arctic adventure. Our readers may remember the story we gave of the 19 persons who were left on the ice-floe when the *Polaris* broke from her moorings in about N. lat. 79°, on the night of October 19, 1872, and who were all miraculously rescued six months later off the coast of Labrador. Eleven more of the crew arrived at Dundee last Friday afternoon in the whaling vessel, *Arctic*, Capt. Adams. Among these eleven are, Capt. S. O. Buddington, sailing and ice master, Dr. Emil Bessels, H. C. Chester, first mate, W. Martin, second mate, Emil Schumann, chief engineer, A. Odell, second engineer, besides a fireman, the carpenter, and three seamen.

After the ship drifted away from the floe she ultimately reached Lifeboat Cove, where it was resolved to beach her, which was done after much trouble. From the timbers of the ship a house was constructed on shore, and by the help of a few friendly Esquimaux, and the provisions and coals saved from the *Polaris*, the fourteen men spent the winter much more comfortably than might have been expected under the circumstances. Towards the end of the winter, however, it was resolved to make an attempt to push southwards, and for this purpose under the superintendence of the energetic first mate, Mr. Chester, of whom all the crew speak in high terms, two boats were, amid many hardships, constructed out of some of the cabin-timbers of the *Polaris*. About the middle of last June, the boats having been completed and packed with what provision could be had, as well as ammunition, the party bade adieu to Lifeboat Cove and proceeded to make their way southwards. After many anxieties Cape York was reached on June 21. Here the boats were quite beset among the ice. But the greatest possible excitement and fear were experienced when, on the 23rd, a vessel was espied. She turned out to be the *Ravenscraig* whaler, of Dundee, Capt. Allan. All hands determined to reach the ship with the least possible delay, but in doing so they were greatly assisted by Capt. Allan, who had sent his crew to help them in carrying what things they had in their possession. They brought one boat with them and left the other. On reaching the ship they were very kindly treated, but subsequently, so that the fishing operations might be interrupted as little as possible, Capt. Allan shipped a few on the *Arctic*. The latter vessel having completed her fishing earlier than expected, and knowing that the crew of the *Polaris* would be anxious to return home as speedily as possible, Captain Adams, her commander, went in search of the *Ravenscraig*. Finding her, he took on board those of the survivors it contained, but Capt. Allan had previously put on board the *Intrepid*—R. W. D. Bryan, astronomer and chaplain; J. B. March, seaman; and John W. Booth, fireman. The *Intrepid* is expected in the course of a few weeks. The men state that the privations which they suffered were by no means of a serious character. The life was rough, laborious, and monotonous, and although danger occasionally presented itself in a way well calculated to inspire the greatest fear, yet no accident of any importance occurred to the adventurers.

Capt. Markham, R.N., accompanied Capt. Adams, of the *Arctic*, on his whaling voyage with the view of making investigations in the northern regions. The captain left Dundee on Friday, and was present in the Geographical Section at Bradford on Saturday, where he was received with great enthusiasm, and where he announced himself as heart and soul a convert to the Smith Sound route to the Pole.

The men connected with the *Polaris* Arctic expedition left Dundee on Monday, and Liverpool on Tuesday, for New York. All were in excellent health and spirits, and some of them say that they would have no objection to go on another such enterprise. Capt. Buddington states that Capt. Hall was buried in lat. 81·38 N., and long. 61·44 W. The vice-consul examined the crew of the *Polaris* on Monday, and transmitted their depositions to America, so that their statements may be extant should any accident befall themselves.

Dr. Bessel, who was the chief of the scientific party connected with the expedition, states that zoological, meteorological, botanical, and geological specimens were collected, but many of them were lost when the crew separated in October last. Careful and minute observations were also made, and after the explorers were picked up by the *Ravenscraig* they were continued. These surveys, of course, were not so exact as was to be desired, there being little convenience and very few instruments. The specimens taken on board the whalers are all preserved, and it is believed that, from a scientific point of view, they will be of very great value. The opinion of Dr. Bessel is that, had no accident occurred to the *Polaris*, the expedition would have been prosecuted. Regarding statements which had been made respecting the causes which led to the death of Captain Hall, he asserts that the captain was carried off by an attack of apoplexy. The doctor declines to enter into the question as to the management of the expedition after the death of Capt. Hall, but there is every likelihood the matters involved will be made the subject of judicial inquiry in America.

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* A further explanation of these two forms is given in my book "Die Befruchtung der Blumen durch Insekten," pp. 294-296.

Taking all the circumstances into account, it is astonishing that both divisions of the crew have escaped without the loss of an individual and with so comparatively little hardship. The complete narrative of the *Polaris* Expedition, with the important scientific results obtained, will be looked for with impatient interest.

NOTES

We regret exceedingly to announce that Prof. Donati, Director of the Astronomical Observatory in Florence, died of cholera on the 20th inst. at Vienna, where he had arrived only two days previously.

Dr. Nelaton, the eminent surgeon, died at Paris on the 21st inst. at the age of 66 years.

The death is also announced at Paris of M. Coste, the well-known naturalist and member of the French Institute, at the age of sixty-six. He first devoted himself chiefly to the study of comparative embryogeny, and his earlier works attracted so much attention that a special professorship was created for him at the College of France. Of late years he had chiefly applied himself to the science of the artificial production of fish, and it was on his recommendation that the Government in 1851 founded the breeding ponds at Huningen for stocking the Rhône with salmon and trout, and which in two years produced 600,000 young fry in that river. As inspector-general of fluvial and coast fisheries, he also made numerous experiments for the propagation of oysters, but the expectations which had been raised by his theories have not so far been realised by the results obtained. M. Coste was the author of numerous physiological works and reports to the Academy of Sciences.

Our list is not yet complete. Prof. Czermak, the eminent physiologist, died at Leipzic on Tuesday, the 16th inst.

By the death of Prof. Barker, M.D., the professorship of Experimental Physics in the Royal College of Science for Ireland, Dublin, has become vacant. The chair is in the gift of the Lords of the Committee of Council on Education, South Kensington. It is of the value of 200*l.* per annum, besides a share in the fees paid by the students.

Prof. Hughes Bennett, of Edinburgh, has been elected Corresponding Member of the National Academy of Medicine of France.

There will be an election at Magdalen College, Oxford, in October next, to a Fellowship in Natural Science, the holder of which will not be required to take holy orders. In the examination, which will be held in common with Merton College, preference will be given to proficiency in Biology, the College reserving to themselves the power of taking candidates in any other branch of Natural Science, if it shall seem expedient to do so. Candidates must have passed all the examinations required by the University of Oxford or the University of Cambridge for the degree of Bachelor of Arts, and must not be in possession of any ecclesiastical benefice, or of any property, Government pension, or office tenable for life or during good behaviour (not being an academical office within the University of Oxford), the clear annual value of which shall exceed 230*l.* They must also produce testimonials of their fitness to become Fellows of the College as a place of religion, learning, and education, and these must be sent to the president on or before Monday, September 29. Candidates for the Fellowship are required to call on the president on Monday, October 6, between the hours of 3 and 5, or 8 and 9 P.M. The examination will commence on the following day.

It seems that the projected balloon voyage from New York to Europe is not now likely to take place. An attempt was made to inflate the balloon on the 10th, but it failed, owing to a high wind. The attempt was renewed on the 12th, but a rent appeared and the operation was abandoned. Mr. Wise, the aeronaut, had foreseen this result, owing to the imperfect manner in which the balloon was constructed; and indeed from what has been stated, it would seem Science may be congratulated that an enterprise in which newspaper advertising had so much to do, has been thus liberated from the responsibility of having to answer for a much more serious disaster, which, we repeat, need not be risked at all so far as Science is concerned.

Mr. George Smith has just discovered the fragments of an ancient Assyrian Canon, from the Babylonian copy of which the much-contested Canon of Berosus was unquestionably derived. The importance of this relic to chronologists can scarcely be over-estimated, and it will form the substance of a paper shortly to be read before the Society of Biblical Archæology by its fortunate discoverer.

A French translation of Grisebach's "Vegetation der Erde nach ihrer klimatischen Anordnung" is promised, with annotations, by M. P. de Tchihatcheff.

We understand that Messrs. Macmillan will publish, early in the approaching season, a splendid series of pictures by Mr. Joseph Wolf, illustrative of the "Life and Habits of Wild Animals." The illustrations have been in course of engraving by Messrs. Whymper during the last seven years, and, as they are the last series which will be drawn by Mr. Wolf, either upon wood or upon stone, they will have an especial claim to the attention of all those who are interested in Natural History. The pictures are accompanied by descriptive letterpress by Mr. D. G. Elliot, whose monograph of the pheasants was noticed by us some time ago.

The *Journal of Botany* states that Dr. Beccari, the Italian traveller and collector, when last heard of, was at the island of Wokam, off the south-west coast of New Guinea; he was to go on to Amboina, and had made large collections of plants and animals, which no doubt will include a number of novelties.

The *Revue Horticole* states that M. Planchon, the Professor of Botany at Montpellier, has been charged by the French Government with the duty of visiting America to study the ravages of the new vine disease, the *Pemphigus vitifoliæ*. No change of government seems to lessen the sense of importance of scientific investigation displayed by our neighbours across the Channel.

A tract of hematite iron ore has been discovered in Shropshire, and eleven hundred acres have been secured on behalf of certain Staffordshire ironmasters, who will work it as a company. Some specimens contain 57 per cent. of iron. The discovery is of great importance to the iron industry.

The additions to the Zoological Society's Gardens during the past week include two Indian Antelopes (*Antilope cervicapra*) from India, presented by Mr. G. E. Rogers; an Alligator (*Alligator mississippiensis*) from America, presented by Dr. Palin; a Cardinal Grosbeak (*Cardinalis virginianus*), a Red-shouldered Starling (*Agelaeus phoeniceus*), a Baltimore Hangnest (*Icterus baltimore*), from North America, presented by Mr. Samuel Stubbs; a Cuckoo (*Cuculus canorus*), British, presented by Dr. Williams; a Rattlesnake (*Crotalus durissus*) from North America, purchased; twelve White-faced Tree Ducks (*Dendrocygna autumnalis*) from Brazil; a Manx Shearwater (*Puffinus anglorum*), British, deposited.

MOLECULES*

AN atom is a body which cannot be cut in two. A molecule is the smallest possible portion of a particular substance. No one has ever seen or handled a single molecule. Molecular science, therefore, is one of those branches of study which deal with things invisible and imperceptible by our senses, and which cannot be subjected to direct experiment.

The mind of man has perplexed itself with many hard questions. Is space infinite, and if so in what sense? Is the material world infinite in extent, and are all places within that extent equally full of matter? Do atoms exist, or is matter infinitely divisible?

The discussion of questions of this kind has been going on ever since men began to reason, and to each of us, as soon as we obtain the use of our faculties, the same old questions arise as fresh as ever. They form as essential a part of the science of the nineteenth century of our era, as of that of the fifth century before it.

We do not know much about the science organisation of Thrace twenty-two centuries ago, or of the machinery then employed for diffusing an interest in physical research. There were men, however, in those days, who devoted their lives to the pursuit of knowledge with an ardour worthy of the most distinguished members of the British Association; and the lectures in which Democritus explained the atomic theory to his fellow-citizens of Abdera realised, not in golden opinions only, but in golden talents, a sum hardly equalled even in America.

To another very eminent philosopher, Anaxagoras, best known to the world as the teacher of Socrates, we are indebted for the most important service to the atomic theory, which, after its statement by Democritus, remained to be done. Anaxagoras, in fact, stated a theory which so exactly contradicts the atomic theory of Democritus that the truth or falsehood of the one theory implies the falsehood or truth of the other. The question of the existence or non-existence of atoms cannot be presented to us this evening with greater clearness than in the alternative theories of these two philosophers.

Take any portion of matter, say a drop of water, and observe its properties. Like every other portion of matter we have ever seen, it is divisible. Divide it in two, each portion appears to retain all the properties of the original drop, and among others that of being divisible. The parts are similar to the whole in every respect except in absolute size.

Now go on repeating the process of division till the separate portions of water are so small that we can no longer perceive or handle them. Still we have no doubt that the sub-division might be carried further, if our senses were more acute and our instruments more delicate. Thus far all are agreed, but now the question arises, Can this sub-division be repeated for ever?

According to Democritus and the atomic school, we must answer in the negative. After a certain number of sub-divisions, the drop would be divided into a number of parts each of which is incapable of further sub-division. We should thus, in imagination, arrive at the atom, which, as its name literally signifies, cannot be cut in two. This is the atomic doctrine of Democritus, Epicurus, and Lucretius, and, I may add, of your lecturer.

According to Anaxagoras, on the other hand, the parts into which the drop is divided, are in all respects similar to the whole drop, the mere size of a body counting for nothing as regards the nature of its substance. Hence if the whole drop is divisible, so are its parts down to the minutest sub-divisions, and that without end.

The essence of the doctrine of Anaxagoras is that the parts of a body are in all respects similar to the whole. It was therefore called the doctrine of Homoiomereia. Anaxagoras did not of course assert this of the parts of organised bodies such as men and animals, but he maintained that those inorganic substances which appear to us homogeneous are really so, and that the universal experience of mankind testifies that every material body, without exception, is divisible.

The doctrine of atoms and that of homogeneity are thus in direct contradiction.

But we must now go on to molecules. Molecule is a modern word. It does not occur in *Johnson's Dictionary*. The ideas it embodies are those belonging to modern chemistry.

A drop of water, to return to our former example, may be divided into a certain number, and no more, of portions similar to each other. Each of these the modern chemist calls a molecule of water. But it is by no means an atom, for it contains two different substances, oxygen and hydrogen, and by a certain process the molecule may be actually divided into two parts, one consisting of oxygen and the other of hydrogen. According to the received doctrine, in each molecule of water there are two molecules of hydrogen and one of oxygen. Whether these are or are not ultimate atoms I shall not attempt to decide.

We now see what a molecule is, as distinguished from an atom.

A molecule of a substance is a small body such that if, on the one hand, a number of similar molecules were assembled together they would form a mass of that substance, while on the other hand, if any portion of this molecule were removed, it would no longer be able, along with an assemblage of other molecules similarly treated, to make up a mass of the original substance.

Every substance, simple or compound, has its own molecule. If this molecule be divided, its parts are molecules of a different substance or substances from that of which the whole is a molecule. An atom, if there is such a thing, must be a molecule of an elementary substance. Since, therefore, every molecule is not an atom, but every atom is a molecule, I shall use the word molecule as the more general term.

I have no intention of taking up your time by expounding the doctrines of modern chemistry with respect to the molecules of different substances. It is not the special but the universal interest of molecular science which encourages me to address you. It is not because we happen to be chemists or physicists or specialists of any kind that we are attracted towards this centre of all material existence, but because we all belong to a race endowed with faculties which urge us on to search deep and ever deeper into the nature of things.

We find that now, as in the days of the earliest physical speculations, all physical researches appear to converge towards the same point, and every inquirer, as he looks forward into the dim region towards which the path of discovery is leading him, sees, each according to his sight, the vision of the same quest.

One may see the atom as a material point, invested and surrounded by potential forces. Another sees no garment of force, but only the bare and utter hardness of mere impenetrability.

But though many a speculator, as he has seen the vision recede before him into the innermost sanctuary of the inconceivably little, has had to confess that the quest was not for him, and though philosophers in every age have been exhorting each other to direct their minds to some more useful and attainable aim, each generation, from the earliest dawn of science to the present time, has contributed a due proportion of its ablest intellects to the quest of the ultimate atom.

Our business this evening is to describe some researches in molecular science, and in particular to place before you any definite information which has been obtained respecting the molecules themselves. The old atomic theory, as described by Lucretius and revived in modern times, asserts that the molecules of all bodies are in motion, even when the body itself appears to be at rest. These motions of molecules are in the case of solid bodies confined within so narrow a range that even with our best microscopes we cannot detect that they alter their places at all. In liquids and gases, however, the molecules are not confined within any definite limits, but work their way through the whole mass, even when that mass is not disturbed by any visible motion.

This process of diffusion, as it is called, which goes on in gases and liquids and even in some solids, can be subjected to experiment, and forms one of the most convincing proofs of the motion of molecules.

Now the recent progress of molecular science began with the study of the mechanical effect of the impact of these moving molecules when they strike against any solid body. Of course these flying molecules must beat against whatever is placed among them, and the constant succession of these strokes is, according to our theory, the sole cause of what is called the pressure of air and other gases.

This appears to have been first suspected by Daniel Bernoulli, but he had not the means which we now have of verifying the theory. The same theory was afterwards brought forward independently by Lesage, of Geneva, who, however, devoted most of his labour to the explanation of gravitation by the impact of atoms. Then Herapath, in his "Mathematical Physics,"

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[*] Lecture delivered before the British Association at Bradford, by Prof. Clerk-Maxwell, F.R.S.

published in 1847, made a much more extensive application of the theory to gases, and Dr. Joule, whose absence from our meeting we must all regret, calculated the actual velocity of the molecules of hydrogen.

The further development of the theory is generally supposed to have been begun with a paper by Krönig, which does not, however, so far as I can see, contain any improvement on what had gone before. It seems, however, to have drawn the attention of Prof. Clausius to the subject, and to him we owe a very large part of what has been since accomplished.

We all know that air or any other gas placed in a vessel presses against the sides of the vessel, and against the surface of any body placed within it. On the kinetic theory this pressure is entirely due to the molecules striking against these surfaces, and thereby communicating to them a series of impulses which follow each other in such rapid succession that they produce an effect which cannot be distinguished from that of a continuous pressure.

If the velocity of the molecules is given, and the number varied, then since each molecule, on an average, strikes the side of the vessel the same number of times, and with an impulse of the same magnitude, each will contribute an equal share to the whole pressure. The pressure in a vessel of given size is therefore proportional to the number of molecules in it, that is to the quantity of gas in it.

This is the complete dynamical explanation of the fact discovered by Robert Boyle, that the pressure of air is proportional to its density. It shows also that of different portions of gas forced into a vessel, each produces its own part of the pressure independently of the rest; and this, whether these portions be of the same gas or not.

Let us next suppose that the velocity of the molecules is increased. Each molecule will now strike the sides of the vessel a greater number of times in a second, but besides this, the impulse of each blow will be increased in the same proportion, so that the part of the pressure due to each molecule will vary as the *square* of the velocity. Now the increase of the square of velocity corresponds, in our theory, to a rise of temperature, and in this way we can explain the effect of warming the gas, and also the law discovered by Charles that the proportional expansion of all gases between given temperatures is the same.

The dynamical theory also tells us what will happen if molecules of different masses are allowed to knock about together. The greater masses will go slower than the smaller ones, so that, on an average, every molecule, great or small, will have the same energy of motion.

The proof of this dynamical theorem, in which I claim the priority, has recently been greatly developed and improved by Dr. Ludwig Boltzmann. The most important consequence which flows from it is that a cubic centimetre of every gas at standard temperature and pressure contains the same number of molecules. This is the dynamical explanation of Gay Lussac's law of the equivalent volumes of gases. But we must now descend to particulars, and calculate the actual velocity of a molecule of hydrogen.

A cubic centimetre of hydrogen, at the temperature of melting ice and at a pressure of one atmosphere, weighs 0·00008954 grammes. We have to find at what rate this small mass must move (whether altogether or in separate molecules makes no difference) so as to produce the observed pressure on the sides of the cubic centimetre. This is the calculation which was first made by Dr. Joule, and the result is 1,859 metres per second. This is what we are accustomed to call a great velocity. It is greater than any velocity obtained in artillery practice. The velocity of other gases is less, as you will see by the table, but in all cases it is very great as compared with that of bullets.

We have now to conceive the molecules of the air in this hall flying about in all directions, at a rate of about seventeen miles in a minute.

If all these molecules were flying in the same direction, they would constitute a wind blowing at the rate of seventeen miles a minute, and the only wind which approaches this velocity is that which proceeds from the mouth of a cannon. How, then, are you and I able to stand here? Only because the molecules happen to be flying in different directions, so that those which strike against our backs enable us to support the storm which is beating against our faces. Indeed, if this molecular bombardment were to cease, even for an instant, our veins would swell, our breath would leave us, and we should, literally, expire. But it is not only against us or against the walls of the room that the molecules are striking. Consider the immense number of them, and the fact that they are flying in every possible direction, and you will see that they cannot avoid striking each other. Every time that two molecules come into collision, the paths of both are changed, and they go off in new directions. Thus each molecule is continually getting its course altered, so that in spite of its great velocity it may be a long time before it reaches any great distance from the point at which it set out.

I have here a bottle containing ammonia. Ammonia is a gas which you can recognise by its smell. Its molecules have a velocity of six hundred metres per second, so that if their course had not been interrupted by striking against the molecules of air in the hall, everyone in the most distant gallery would have smelt ammonia before I was able to pronounce the name of the gas. But instead of this, each molecule of ammonia is so jostled about by the molecules of air, that it is sometimes going one way and sometimes another. It is like a hare which is always doubling, and though it goes a great pace, it makes very little progress. Nevertheless, the smell of ammonia is now beginning to be perceptible at some distance from the bottle. The gas does diffuse itself through the air, though the process is a slow one, and if we could close up every opening of this hall so as to make it air-tight, and leave everything to itself for some weeks, the ammonia would become uniformly mixed through every part of the air in the hall.

This property of gases, that they diffuse through each other, was first remarked by Priestley. Dalton showed that it takes place quite independently of any chemical action between the inter-diffusing gases. Graham, whose researches were especially directed towards those phenomena which seem to throw light on molecular motions, made a careful study of diffusion, and obtained the first results from which the rate of diffusion can be calculated.

Still more recently the rates of diffusion of gases into each other have been measured with great precision by Prof. Loschmidt of Vienna.

He placed the two gases in two similar vertical tubes, the lighter gas being placed above the heavier, so as to avoid the formation of currents. He then opened a sliding valve, so as to make the two tubes into one, and after leaving the gases to themselves for an hour or so, he shut the valve, and determined how much of each gas had diffused into the other.

As most gases are invisible, I shall exhibit gaseous diffusion to you by means of two gases, ammonia and hydrochloric acid, which, when they meet, form a solid product. The ammonia, being the lighter gas, is placed above the hydrochloric acid, with a stratum of air between, but you will soon see that the gases can diffuse through this stratum of air, and produce a cloud of white smoke when they meet. During the whole of this process no currents or any other visible motion can be detected. Every part of the vessel appears as calm as a jar of undisturbed air.

But, according to our theory, the same kind of motion is going on in calm air as in the inter-diffusing gases, the only difference being that we can trace the molecules from one place to another more easily when they are of a different nature from those through which they are diffusing.

If we wish to form a mental representation of what is going on among the molecules in calm air, we cannot do better than observe a swarm of bees, when every individual bee is flying furiously, first in one direction, and then in another, while the swarm, as a whole, either remains at rest, or sails slowly through the air.

In certain seasons, swarms of bees are apt to fly off to a great distance, and the owners, in order to identify their property when they find them on other people's ground, sometimes throw handfulls of flour at the swarm. Now let us suppose that the flour thrown at the flying swarm has whitened those bees only which happened to be in the lower half of the swarm, leaving those in the upper half free from flour.

If the bees still go on flying hither and thither in an irregular manner, the floury bees will be found in continually increasing proportions in the upper part of the swarm, till they have become equally diffused through every part of it. But the reason of this diffusion is not because the bees were marked with flour, but because they are flying about. The only effect of the marking is to enable us to identify certain bees.

We have no means of marking a select number of molecules of air, so as to trace them after they have become diffused among others, but we may communicate to them some property by which we may obtain evidence of their diffusion.

For instance, if a horizontal stratum of air is moving horizontally, molecules diffusing out of this stratum into those above and below will carry their horizontal motion with them, and so tend to communicate motion to the neighbouring strata, while molecules diffusing out of the neighbouring strata into the moving one will tend to bring it to rest. The action between the strata is somewhat like that of two rough surfaces, one of which slides over the other, rubbing on it. Friction is the name given to this action between solid bodies; in the case of fluids it is called internal friction or viscosity.

It is in fact only another kind of diffusion—a lateral diffusion of momentum, and its amount can be calculated from data derived from observations of the first kind of diffusion, that of matter. The comparative values of the viscosity of different gases were determined by Graham in his researches on the transpiration of gases through long narrow tubes, and their absolute values have been deduced from experiments on the oscillation of discs by Oscar Meyer and myself.

Another way of tracing the diffusion of molecules through calm air is to heat the upper stratum of the air in a vessel, and so observe the rate at which this heat is communicated to the lower strata. This, in fact, is a third kind of diffusion—that of energy, and the rate at which it must take place was calculated from data derived from experiments on viscosity before any direct experiments on the conduction of heat had been made. Prof. Stefan, of Vienna, has recently, by a very delicate method, succeeded in determining the conductivity of air, and he finds it, as he tells us, in striking agreement with the value predicted by the theory.

All these three kinds of diffusion—the diffusion of matter, of momentum, and of energy—are carried on by the motion of the molecules. The greater the velocity of the molecules and the farther they travel before their paths are altered by collision with other molecules, the more rapid will be the diffusion. Now we know already the velocity of the molecules, and therefore by experiments on diffusion we can determine how far, on an average, a molecule travels without striking another. Prof. Clausius, of Bonn, who first gave us precise ideas about the motion of agitation of molecules, calls this distance the mean path of a molecule. I have calculated, from Prof. Loschmidt's diffusion experiments, the mean path of the molecules of four well-known gases. The average distance travelled by a molecule between one collision and another is given in the table. It is a very small distance, quite imperceptible to us even with our best microscopes. Roughly speaking, it is about the tenth part of the length of a wave of light, which you know is a very small quantity. Of course the time spent on so short a path by such swift molecules must be very small. I have calculated the number of collisions which each must undergo in a second. They are given in the table and are reckoned by thousands of millions. No wonder that the travelling power of the swiftest molecule is but small, when its course is completely changed thousands of millions of times in a second.

The three kinds of diffusion also take place in liquids, but the relation between the rates at which they take place is not so simple as in the case of gases. The dynamical theory of liquids is not so well understood as that of gases, but the principal difference between a gas and a liquid seems to be that in a gas each molecule spends the greater part of its time in describing its free path, and is for a very small portion of its time engaged in encounters with other molecules, whereas in a liquid the molecule has hardly any free path, and is always in a state of close encounter with other molecules.

Hence in a liquid the diffusion of motion from one molecule to another takes place much more rapidly than the diffusion of the molecules themselves, for the same reason that it is more expeditious in a dense crowd to pass on a letter from hand to hand than to give it to a special messenger to work his way through the crowd. I have here a jar, the lower part of which contains a solution of copper sulphate, while the upper part contains pure water. It has been standing here since Friday, and you see how little progress the blue liquid has made in diffusing itself through the water above. The rate of diffusion of a solution of sugar has been carefully observed by Voit. Comparing his results with those of Loschmidt on gases, we find that about as much diffusion takes place in a second in gases as requires a day in liquids.

The rate of diffusion of momentum is also slower in liquids than in gases, but by no means in the same proportion. The same amount of motion takes about ten times as long to subside in water as in air, as you will see by what takes place when I stir these two jars, one containing water and the other air. There is still less difference between the rates at which a rise of temperature is propagated through a liquid and through a gas.

In solids the molecules are still in motion, but their motions are confined within very narrow limits. Hence the diffusion of matter does not take place in solid bodies, though that of motion and heat takes place very freely. Nevertheless, certain liquids can diffuse through colloid solids, such as jelly and gum, and hydrogen can make its way through iron and palladium.

We have no time to do more than mention that most wonderful molecular motion which is called electrolysis. Here is an electric current passing through acidulated water, and causing oxygen to appear at one electrode and hydrogen at the other. In the space between, the water is perfectly calm, and yet two opposite currents of oxygen and of hydrogen must be passing through it. The physical theory of this process has been studied by Clausius, who has given reasons for asserting that in ordinary water the molecules are not only moving, but every now and then striking each other with such violence that the oxygen and hydrogen of the molecules part company, and dance about through the crowd, seeking partners which have become dissociated in the same way. In ordinary water these exchanges produce, on the whole, no observable effect, but no sooner does the electromotive force begin to act than it exerts its guiding influence on the unattached molecules, and bends the course of each toward its proper electrode, till the moment when, meeting with an unappropriated molecule of the opposite kind, it enters again into a more or less permanent union with it till it is again dissociated by another shock. Electrolysis, therefore, is a kind of diffusion assisted by electromotive force.

Another branch of molecular science is that which relates to the exchange of molecules between a liquid and a gas. It includes the theory of evaporation and condensation, in which the gas in question is the vapour of the liquid, and also the theory of the absorption of a gas by a liquid of a different substance. The researches of Dr. Andrews on the relations between the liquid and the gaseous state have shown us that though the statements in our own elementary text-books may be so neatly expressed that they appear almost self-evident, their true interpretation may involve some principle so profound that, till the right man has laid hold of it, no one ever suspects that anything is left to be discovered.

These, then, are, some of the fields from which the data of molecular science are gathered. We may divide the ultimate results into three ranks, according to the completeness of our knowledge of them.

To the first rank belong the relative masses of the molecules of different gases, and their velocities in metres per second. These data are obtained from experiments on the pressure and density of gases, and are known to a high degree of precision.

In the second rank we must place the relative size of the molecules of different gases, the length of their mean paths, and the number of collisions in a second. These quantities are deduced from experiments on the three kinds of diffusion. Their received values must be regarded as rough approximations till the methods of experimenting are greatly improved.

There is another set of quantities which we must place in the third rank, because our knowledge of them is neither precise, as in the first rank, nor approximate, as in the second, but is only as yet of the nature of a probable conjecture. These are the absolute mass of a molecule, its absolute diameter, and the number of molecules in a cubic centimetre. We know the relative masses of different molecules with great accuracy, and we know their relative diameters approximately. From these we can deduce the relative densities of the molecules themselves. So far we are on firm ground.

The great resistance of liquids to compression makes it probable that their molecules must be at about the same distance from each other as that at which two molecules of the same substance in the gaseous form act on each other during an encounter. This conjecture has been put to the test by Lorenz Meyer, who has compared the densities of different liquids with the calculated relative densities of the molecules of their vapours, and has found a remarkable correspondence between them.

Now Loschmidt has deduced from the dynamical theory the following remarkable proportion:—As the volume of a gas is to the combined volume of all the molecules contained in it, so is the mean path of a molecule to one-eighth of the diameter of a molecule.

Assuming that the volume of the substance, when reduced to the liquid form, is not much greater than the combined volume of the molecules, we obtain from this proportion the diameter of a molecule. In this way Loschmidt, in 1865, made the first estimate of the diameter of a molecule. Independently of him and of each other, Mr. Stoney in 1868, and Sir W. Thomson in 1870, published results of a similar kind, those of Thomson being deduced not only in this way, but from considerations derived from the thickness of soap bubbles, and from the electric properties of metals.

According to the table, which I have calculated from Loschmidt's data, the size of the molecules of hydrogen is such that about two million of them in a row would occupy a millimetre, and a million million million million of them would weigh between four and five grammes.

In a cubic centimetre of any gas at standard pressure and temperature there are about nineteen million million million molecules. All these numbers of the third rank are, I need not tell you, to be regarded as at present conjectural. In order to warrant us in putting any confidence in numbers obtained in this way, we should have to compare together a greater number of independent data than we have as yet obtained, and to show that they lead to consistent results.

Thus far we have been considering molecular science as an inquiry into natural phenomena. But though the professed aim of all scientific work is to unravel the secrets of nature, it has another effect, not less valuable, on the mind of the worker. It leaves him in possession of methods which nothing but scientific work could have led him to invent, and it places him in a position from which many regions of nature, besides that which he has been studying, appear under a new aspect.

The study of molecules has developed a method of its own, and it has also opened up new views of nature.

When Lucretius wishes us to form a mental representation of the motion of atoms, he tells us to look at a sunbeam shining through a darkened room (the same instrument of research by which Dr. Tyndall makes visible to us the dust we breathe,) and to observe the motes which chase each other in all directions through it. This motion of the visible motes, he tells us, is but a result of the far more complicated motion of the invisible atoms which knock the motes about. In his dream of nature, as Tennyson tells us, he

"saw the flaring atom-streams
And torrents of her myriad universe,
Ruining along the illimitable inane,
Fly on to clash together again, and make
Another and another frame of things
For ever."

And it is no wonder that he should have attempted to burst the bonds of Fate by making his atoms deviate from their courses at quite uncertain times and places, thus attributing to them a kind of irrational free will, which on his materialistic theory is the only explanation of that power of voluntary action of which we ourselves are conscious.

As long as we have to deal with only two molecules, and have all the data given us, we can calculate the result of their encounter, but when we have to deal with millions of molecules, each of which has millions of encounters in a second, the complexity of the problem seems to shut out all hope of a legitimate solution.

The modern atomists have therefore adopted a method which is I believe new in the department of mathematical physics, though it has long been in use in the Section of Statistics. When the working members of Section F get hold of a Report of the Census, or any other document containing the numerical data of Economic and Social Science, they begin by distributing the whole population into groups, according to age, income-tax, education, religious belief, or criminal convictions. The number of individuals is far too great to allow of their tracing the history of each separately, so that, in order to reduce their labour within human limits, they concentrate their attention on a small number of artificial groups. The varying number of individuals in each group, and not the varying state of each individual, is the primary datum from which they work.

This, of course, is not the only method of studying human nature. We may observe the conduct of individual men and compare it with that conduct which their previous character and their present circumstances, according to the best existing theory, would lead us to expect. Those who practise this method endeavour to improve their knowledge of the elements of human nature, in much the same way as an astronomer corrects the elements of a planet by comparing its actual position with that deduced from the received elements. The study of human nature by parents and schoolmasters, by historians and statesmen, is therefore to be distinguished from that carried on by registrars and tabulators, and by those statesmen who put their faith in figures. The one may be called the historical, and the other the statistical method.

The equations of dynamics completely express the laws of the historical method as applied to matter, but the application of these equations implies a perfect knowledge of all the data. But the smallest portion of matter which we can subject to experiment consists of millions of molecules, not one of which ever becomes individually sensible to us. We cannot, therefore, ascertain the actual motion of any one of these molecules, so that we are obliged to abandon the strict historical method, and to adopt the statistical method of dealing with large groups of molecules.

The data of the statistical method as applied to molecular science are the sums of large numbers of molecular quantities. In studying the relations between quantities of this kind, we meet with a new kind of regularity, the regularity of averages, which we can depend upon quite sufficiently for all practical purposes, but which can make no claim to that character of absolute precision which belongs to the laws of abstract dynamics.

Thus molecular science teaches us that our experiments can never give us anything more than statistical information, and that no law deduced from them can pretend to absolute precision. But when we pass from the contemplation of our experiments to that of the molecules themselves, we leave the world of chance and change, and enter a region where everything is certain and immutable.

The molecules are conformed to a constant type with a precision which is not to be found in the sensible properties of the bodies which they constitute. In the first place the mass of each individual molecule, and all its other properties, are absolutely unalterable. In the second place the properties of all molecules of the same kind are absolutely identical.

Let us consider the properties of two kinds of molecules, those of oxygen and those of hydrogen.

We can procure specimens of oxygen from very different sources—from the air, from water, from rocks of every geological epoch. The history of these specimens has been very different, and if, during thousands of years, difference of circumstances could produce difference of properties, these specimens of oxygen would show it.

In like manner we may procure hydrogen from water, from coal, or, as Graham did, from meteoric iron. Take two litres of any specimen of hydrogen, it will combine with exactly one litre of any specimen of oxygen, and will form exactly two litres of the vapour of water.

Now if, during the whole previous history of either specimen, whether imprisoned in the rocks, flowing in the sea, or careering through unknown regions with the meteorites, any modification of the molecules had taken place, these relations would no longer be preserved.

But we have another and an entirely different method of comparing the properties of molecules. The molecule, though indestructible, is not a hard rigid body, but is capable of internal movements, and when these are excited it emits rays, the wave-length of which is a measure of the time of vibration of the molecule.

By means of the spectroscope the wave-lengths of different kinds of light may be compared to within one ten-thousandth part. In this way it has been ascertained, not only that molecules taken from every specimen of hydrogen in our laboratories have the same set of periods of vibration, but that light, having the same set of periods of vibration, is emitted from the sun and from the fixed stars.

We are thus assured that molecules of the same nature as those of our hydrogen exist in those distant regions, or at least did exist when the light by which we see them was emitted.

From a comparison of the dimensions of the buildings of the Egyptians with those of the Greeks, it appears that they have a common measure. Hence, even if no ancient author had recorded the fact that the two nations employed the same cubit as a standard of length, we might prove it from the buildings themselves. We should also be justified in asserting that at some time or other a material standard of length must have been carried from one country to the other, or that both countries had obtained their standards from a common source.

But in the heavens we discover by their light, and by their light alone, stars so distant from each other that no material thing can ever have passed from one to another, and yet this light, which is to us the sole evidence of the existence of these distant worlds, tells us also that each of them is built up of molecules of the same kinds as those which we find on earth. A molecule of hydrogen, for example, whether in Sirius or in Arcturus, executes its vibrations in precisely the same time.

Each molecule, therefore, throughout the universe, bears impressed on it the stamp of a metric system as distinctly as does the metre of the Archives at Paris, or the double royal cubit of the Temple of Karnac.

No theory of evolution can be formed to account for the similarity of molecules, for evolution necessarily implies continuous change, and the molecule is incapable of growth or decay, of generation or destruction.

None of the processes of Nature, since the time when Nature began, have produced the slightest difference in the properties of any molecule. We are therefore unable to ascribe either the existence of the molecules or the identity of their properties to the operation of any of the causes which we call natural.

On the other hand, the exact quality of each molecule to all others of the same kind gives it, as Sir John Herschel has well said, the essential character of a manufactured article, and precludes the idea of its being eternal and self existent.

Thus we have been led, along a strictly scientific path, very near to the point at which Science must stop. Not that Science is debarred from studying the internal mechanism of a molecule which she cannot take to pieces, any more than from investigating an organism which she cannot put together. But in tracing back the history of matter Science is arrested when she assures herself, on the one hand, that the molecule has been made, and on the other that it has not been made by any of the processes we call natural.

Science is incompetent to reason upon the creation of matter itself out of nothing. We have reached the utmost limit of our thinking faculties when we have admitted that because matter cannot be eternal and self-existent it must have been created.

It is only when we contemplate, not matter in itself, but the form in which it actually exists, that our mind finds something on which it can lay hold.

That matter, as such, should have certain fundamental properties—that it should exist in space and be capable of motion, that its motion should be persistent, and so on, are truths which may, for anything we know, be of the kind which metaphysicians call necessary. We may use our knowledge of such truths for purposes of deduction but we have no data for speculating as to their origin.

But that there should be exactly so much matter and no more in every molecule of hydrogen is a fact of a very different order. We have here a particular distribution of matter—a *collocation*—to use the expression of Dr. Chalmers, of things which we have no difficulty in imagining to have been arranged otherwise.

The form and dimensions of the orbits of the planets, for instance, are not determined by any law of nature, but depend upon a particular collocation of matter. The same is the case with respect to the size of the earth, from which the standard of what is called the metrical system has been derived. But these astronomical and terrestrial magnitudes are far inferior in scientific importance to that most fundamental of all standards which forms the base of the molecular system. Natural causes, as we know, are at work, which tend to modify, if they do not at length destroy, all the arrangements and dimensions of the earth and the whole solar system. But though in the course of ages catastrophes have occurred and may yet occur in the heavens, though ancient systems may be dissolved and new systems evolved out of their ruins, the molecules out of which these systems are built—the foundation stones of the material universe—remain unbroken and unworn.

They continue this day as they were created, perfect in number and measure and weight, and from the ineffaceable characters impressed on them we may learn that those aspirations after accuracy in measurement, truth in statement, and justice in action, which we reckon among our noblest attributes as men, are ours because they are essential constituents of the image of Him Who in the beginning created, not only the heaven and the earth, but the materials of which heaven and earth consist.

Table of Molecular Data.

		Hydrogen.	Oxygen	Carbonic oxide.	Carbonic acid.
Rank I.	Mass of molecule (hydrogen = 1)	1	16	14	22
	Velocity (of mean square), metres per second at 0° C.	1859	465	497	396
Rank II.	Mean path, tenth-metres.	965	560	482	379
	Collisions in a second, (millions)	17750	7646	9489	9720
Rank III.	Diameter, tenth-metre	5·8	7·6	8·3	9·3
	Mass, twenty-fifth-grammes.	46	736	644	1012

Table of Diffusion: $\frac{(\text{centimetre})^2}{\text{second}}$ measure.

	Calculated	Observed.	
H & O	0·7086	0·7214	Diffusion of matter observed by Loschmidt.
H & CO	0·6519	0·6422	
H & CO_2	0·5575	0·5558	
O & CO	0·1807	0·1802	
O & CO_2	0·1427	0·1409	
CO & CO_2	0·1386	0·1406	
H	1·2990	1·49	Diffusion of momentum Graham and Meyer.
O	0·1884	0·313	
CO	0·1748	0·212	
CO_2	0·1087	0·117	
Air		0·256	Diffusion of temperature observed by Stefan.
Copper		1·077	
Iron		0·183	

Cane sugar in water	0·00000365	Voit.
Diffusion in a day	0·3144	
Salt in water . .	0·00000116	Fick.

FUEL *

IN accepting the invitation of the Council of the British Association to deliver an address to the operative classes of this great industrial district, I felt that I was undertaking no easy task. Having to speak on behalf of the Association, and in the presence of many of its most distinguished members, I am bound to treat my subject scientifically, but I have to bear in mind at the same time that I am addressing myself to men unquestionably of good intelligence, but without that scientific training which has almost created a language of its own.

It is no consolation for me to think, that those who have taken a similar task upon themselves in former years, have admirably succeeded in divesting highly scientific subjects of the formalism in which they are habitually clothed. The very names of these men—Tyndall, Huxley, Miller, Lubbock, and Spottiswoode—are such as to preclude in me all idea of rivalry, but I hope to profit by their example, and to remember that truth must always be simple, and that it is only where knowledge is imperfect that scientific formulæ must take the place of plain statements.

The subject matter of my discourse is "Fuel;" a matter with which every one of us has become familiarised from his infancy, but which nevertheless is but little understood even by those who are most largely interested in its applications; it involves considerations of the highest *à priori* interest, both from a scientific and a practical point of view.

I purpose to arrange my subject under five principal heads:—

1. What is fuel?
2. Whence is fuel derived?
3. How should fuel be used?
4. The coal question of the day.
5. Wherein consists the fuel of the sun?

What is fuel?—Some of you may have already said within yourselves that it is but wasted time to enlarge upon such a theme, since all know that fuel is coal drawn from the earth from deposits, with which this country especially has been bountifully supplied; why disturb our plain understanding by scientific definitions which will neither reduce the cost of coal, nor make it last longer on our domestic hearth?

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* Lecture delivered before the British Association at Bradford, by Dr. Siemens.

Yet I must claim your patience for a little, lest, if we do not first agree upon the essential nature of fuel, we may afterwards be at variance in discussing its origin and its uses, the latter at any rate being of practical interest, and a subject worthy of your most attentive consideration.

Fuel, then, in the ordinary acceptation of the term, is carbonaceous matter, which may be in the solid, the liquid, or in the gaseous condition, and which, in combining with oxygen, gives rise to the phenomenon of heat. Commonly speaking, this development of heat is accompanied by flame, because the substance produced in combustion is gaseous. In burning coal, for instance, on a fire-grate, the oxygen of the atmosphere enters into combination with the solid carbon of the coal and produces carbonic acid—a gas which enters the atmosphere, of which it forms a necessary constituent, since without it the growth of trees and other plants would be impossible. But combustion is not necessarily accompanied by flame, or even by a display of intense heat. The metal magnesium burns with a great display of light and heat, but without flame, because the product of combustion is not a gas but a solid, viz. oxide of magnesia. Again, metallic iron, if in a finely divided state, ignites when exposed to the atmosphere, giving rise to the phenomena of heat and light without flame, because the result of combustion is iron oxide or rust; but the same iron, if presented to the atmosphere—more especially to a damp atmosphere—in a solid condition, does not ignite, but is nevertheless gradually converted into metallic oxide or rust as before.

Here, then, we have combination without the phenomena either of flame or light; but by careful experiment we should find that heat is nevertheless produced, and that the amount of heat so produced precisely equals that obtained more rapidly in exposing spongy iron to the action of oxygen. Only, in the latter case the heat is developed by slow degrees, and is dispersed as soon as produced; whereas in the former the rate of production exceeds the rate of dispersion, and heat, therefore, accumulates to the extent of raising the mass to redness. It is evident from these experiments that we have to widen our conception, and call fuel "any substance which is capable of entering into combination with another substance, and in so doing gives rise to the phenomenon of heat."

In thus defining fuel, it might appear at first sight that we should find upon our earth a great variety, and an inexhaustible supply of substances that might be ranged under this head; but a closer investigation will soon reveal the fact that its supply is, comparatively speaking, extremely limited.

In looking at the solid crust of the earth, we find it to be composed for the most part of siliceous, calcareous, and magnesian rock; the former, silica, consisting of the metal silicon combined with oxygen, and is therefore not fuel, but rather a burnt substance which has parted with its heat of combustion ages ago; the second limestone, being carbonate of lime, or the combination of two substances, viz., oxide of calcium and carbonic acid, both of which are essentially products of combustion, the one of the metal calcium and the other of carbon; and the third, magnesia, being the substance magnesium, which I have just burnt before you, and which, further combined with lime, constitutes dolomite rock, of which the Alps are mainly composed. All the commoner metals, such as iron, zinc, tin, alumina, sodium, &c., we find in nature in an oxidised or burnt condition; and the only metallic substances that have resisted the intense oxidising action that must have prevailed at one period of the earth's creation are the so-called precious metals, gold, platinum, iridium, and to some extent also silver and copper. But what about the oceans of water, which have occasionally been cited as representing a vast store of heat-producing power ready for our use when coal shall be exhausted? Not many months ago, indeed, on the occasion of a water-gas company being formed, statements to this effect could be seen in some of our leading papers. Nothing, however, could be more fallacious. When hydrogen burns, doubtless a great development of heat ensues, but water is already the result of this combustion (which took place upon the globe before the ocean was formed), and the separation of these two substances would take precisely the same amount of heat as was originally produced in the combustion. It will thus be seen that both the solid and fluid constituents of our earth, with the exception of coal, of naphtha (which is a mere modification of coal), and the precious metals, are products of combustion, and therefore the very reverse of fuel. Our earth may indeed be looked upon as "a ball of cinder, rolling eternally through space," but happily in company with another celestial body—the sun—whose glorious beams are the physical cause of everything that moves and lives, or that has the power within itself of imparting life, heat, or motion. The invigorating influence is made perceptible to our senses in the form of heat, but it is fair to ask, what is heat, that it should be capable of coming to us from the sun, and of being treasured up in our fuel deposits both below and on the surface of the earth?

If this inquiry had been put to me thirty years ago, I should have been much perplexed. By reference to books on Physical Science, I should have learnt that heat was a subtle fluid which, somehow or other, had taken up its residence in the fuel, and which, upon ignition of the latter, was sallying forth either to vanish or to abide elsewhere; but I should not have been able to associate the two ideas of combustion and development of heat by any intelligible principle in nature, or to suggest any process by which it could have been derived from the sun and petrified, or, as the empty phrase ran, rendered latent in the fuel.

It is by the labours of Meyer, Joule, Clausius, Ranken, and other modern physicists, that we are enabled to give to heat its true significance.

Heat, according to the "dynamical theory," is neither more nor less than motion amongst the particles of the substance heated, which motion, when once produced, may be changed in its direction and its nature, and thus be converted into mechanical effect, expressible in foot pounds, or horse power. By intensifying this motion among the particles, it is made evident to our visual organ by the emanation of light, which again is neither more nor less than vibratory motion imparted by the ignited substance to the medium separating us from the same. According to this theory, which constitutes one of the most important advances in science of the present century, heat, light, electricity, and chemical action are only different manifestations of "energy of matter," mutually convertible, but as indestructible as matter itself.

Energy exists in two forms, dynamic or "kinetic energy," or force manifesting itself to our senses as weight in motion, as sensible heat, or as an active electrical current; and "potential energy," or force in a dormant condition. In illustration of these two forms of energy, I will take the case of lifting a weight, say one pound one foot high. In lifting this weight "kinetic muscular energy" has to be exercised in overcoming the force of gravitation of the earth. The pound weight when supported at the higher level to which it has been raised, represents potential energy to the amount of one unit or foot pound. This potential energy may be utilised in imparting motion to mechanism during its descent, whereby a unit amount of "Work" is accomplished. A pound of carbon then, when raised through the space of one foot from the earth, represents, mechanically speaking, a unit quantity of energy, but the same pound of carbon being separated or lifted away from oxygen, to which it has a very powerful attraction, is capable of developing no less than 11,000,000 foot pounds or unit quantities of energy whenever the bar to their combination, namely excessive depression of temperature, is removed; in other words, the mechanical energy set free in the combustion of one pound of pure carbon is the same as would be required to raise 11,000,000 pounds weight one foot high, or as would sustain the work which we call a horse power during 5 hours 33 minutes. We thus arrive at once at the utmost limit of work which we can ever hope to accomplish by the combustion of one pound of carbonaceous matter, and we shall presently see how far we are still removed in our steam engine *practice from* this limit of perfection.[*]

The following illustrations will show the convertibility of the different forms of energy. If I let the weight of a hammer descend in rapid succession upon a piece of iron it becomes hot, and on beating a nail thus vigorously and skilfully for a minute it will be redhot. In this case the mechanical force developed in the arm by the combustion of carbonaceous muscular fibre is converted into heat. Again, in compressing the air in a fire syringe rapidly ignition of a piece of tinder is obtained. Again, in passing an electrical current through the platinum wire it is directly converted into heat, which is manifested by ignition of the wire, whereas the thermopile gives an illustration of the conversion of heat into electricity. The heat of combustion is the result of the chemical combination of two substances; but does it not follow from this that oxygen is a combustible as well as the carbonaceous substance which goes by the name of fuel? This is, unquestionably, the case, and if our atmosphere was composed of a carbonaceous gas we should have to conduct our oxygen through tubes and send it out through burners to supply us with light and heat, as will be seen by the experiment in which I burn a jet of atmospheric air in a transparent globe filled with common lighting gas; but we could not exist under such inverted conditions, and may safely strike out oxygen and analogous substances such as chlorine from the list of fuels.

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[*] In burning 1 lb. of carbon in the presence of free oxygen, carbonic acid is produced and 14 500 units of heat (1 lb. of water raised through 1° Fah.) are liberated. Each unit of heat is convertible (as proved by the deductions of Meyer and the actual measurements of Joule) into 774 units of force or mechanical energy; hence 1 lb. of carbon represents really 14,500 × 774 = 11,223,000 units of potential energy.

We now approach the second part of our inquiry—Whence is fuel derived?

The rays of the sun represent energy in the form of heat and light, which is communicated to our earth through the transparent medium which must necessarily fill the space between us and our great luminary. If these rays fall upon the growing plant, their effect disappears from direct recognition by our senses, inasmuch as the leaf does not become heated as it would if it was made of iron or dead wood, but we find a chemical result accomplished, viz., carbonic acid gas which has been absorbed by the leaf of the tree from the atmosphere, is there "dissociated," or separated into its elements carbon and oxygen, the oxygen being returned to the atmosphere, and the carbon retained to form the solid substance of the tree.

It is thus clearly shown that the sun has to impart 11,000,000 units of energy to the tree for the formation of one pound of carbon in the shape of woody fibre, and that these 11,000,000 units of energy will be simply resuscitated when the wood is burnt, or again combined with oxygen to form carbonic acid.

Fuel, then, is derived through solar energy acting on the surface of our earth.

But what about the stores of mineral fuel, of coal, which we find within its folds? How did they escape the general combustion which, as we have seen, has consumed all other elementary substances? The answer is a simple one. These deposits of mineral fuel are the results of primeval forests, formed in the manner of to-day through the agency of solar rays, and covered over with earthy matter in the many inundations and convulsions of the globe's surface, which must have followed the early solidification of its surface. Thus our deposits of coal may be looked upon as the accumulation of potential energy derived directly from the sun in former ages, or as George Stephenson, with a sagacity of mind in advance of the science of his day, answered, when asked what was the ultimate cause of motion of his locomotive engine, "that it went by the bottled-up rays of the sun."

It follows from these considerations that the amount of potential energy available for our use is confined to our deposits of coal, which, as appears from the exhaustive inquiries lately made by the Royal Coal Commission are still large indeed, but by no means inexhaustible, if we bear in mind that our requirement will be ever on the increase and that the getting of the coal will become from year to year more difficult as we descend to greater depth. To these stores must be reckoned lignite and peat, which, although not coal, are nevertheless the result of solar energy, attributable to a period of the earth's creation subsequent to the formation of the coal beds, but anterior to our own days.

In discussing the necessity of using our stores of fuel more economically, I have been met by the observation that we need not be anxious about leaving fuel for our descendants—that the human mind would surely invent some other source of power when coal should be exhausted, and that such a source would probably be discovered in electricity. I heard such a suggestion publicly made only a few weeks back at a meeting of the International Jury at Vienna, and could not refrain from calling attention to the fact that electricity is only another form of energy, that could no more be created by man than heat could, and involved the same recourse to our accumulated stores.

If our stores of coal were to ebb, we should have recourse, no doubt, to the force radiating from the sun from day to day; and it may be as well for us to consider, what is the extent of that force, and what our means of gathering and applying it. We have, then, in the first place, the accumulation of solar energy upon our earth's surface by the decomposition of carbonic acid in plants, a source which we know by experience suffices for the human requirements in thinly-populated countries, where industry has taken only a slight development. Wherever population accumulates, however, the wood of the forest no longer suffices even for domestic requirements, and mineral fuel has to be transported from great distances.

The sun's rays produce, however, other effects besides vegetation, and amongst these, evaporation is the most important as a source of available power. By the solar rays, an amount of heat is imparted to our earth that would evaporate yearly a lake of water fourteen feet deep. A considerable proportion of this heat is actually expended in evaporating sea water, producing steam or vapour, which falls back upon the entire surface of both land and sea in the form of rain. The portion which falls upon the elevated land flows back towards the sea in the form of rivers, and in its descent its weight may be utilised to give motion to machinery. Water power, therefore, is also the result of solar energy, and an elevated lake may indeed be looked upon as fuel, in the sense of its being a weight lifted above the sea level through its prior expansion into steam.

This source of power has also been largely resorted to, and might be utilised to a still greater extent in mountainous countries; but it naturally so happens that the great centres of industry are in the plains, where the means of transport are easy, and the total amount of available water-power in such districts is extremely limited.

Another result of solar energy are the winds, which have been utilised for the production of power. This source of power is, indeed, very great in the aggregate, but its application is attended with very great inconvenience. It is proverbial that there is nothing more uncertain than the wind, and when we were dependent upon windmills for the production of flour, it often happened that whole districts were without that necessary element to our daily existence. Ships also, relying upon the wind for their propulsion through the sea, are often becalmed for weeks, and so gradually give preference to steam-power on account of its greater certainty. It has been suggested of late years to utilise the heat of the sun by the accumulation of its rays into a focus by means of gigantic lenses, and to establish steam-boilers in such foci. This would be a most direct utilisation of solar energy, but it is a plan which would hardly recommend itself in this country, where the sun is but rarely seen, and which even in a country like Spain would hardly be productive of useful, practical results.

There is one more natural source of energy available for our uses, which is rather cosmical than solar, viz., the tidal wave. This might also be utilised to very considerable extent in an island country facing the Atlantic seas, like this, but its utilisation on a large scale is connected with great practical difficulty and expenditure, on account of the enormous area of tidal basin that would have to be constructed.

In passing in review these various sources of energy which are still available to us, after we have run through our accumulated capital of potential energy in the shape of coal, it will have struck you that none of them would at all supply the place of our willing and ever-ready slave, the steam-engine; nor would they be applicable to our purposes of locomotion, although means might possibly be invented of storing and carrying potential energy in other forms. But it is not force alone that we require, but heat for smelting our iron and other metals, and the accomplishment of other chemical purposes. We also need a large supply for our domestic purposes. It is true that with an abundant supply of mechanical force we could manufacture heat, and thus actually accomplish all our purposes of smelting, cooking, and heating, without the use of any combustible matter; but such conversion would be attended with so much difficulty and expenditure, that one cannot conceive human prosperity under such laborious and artificial conditions.

We come now to the question—How should fuel be used, and I propose to illustrate this by three examples which are typical of the three great branches of consumption.

a. The production of steam power.
b. The domestic hearth.
c. The metallurgical furnace.

I have represented on a diagram two steam cylinders of the same internal dimensions, the one being what is called a high-pressure steam cylinder, provided with the ordinary slide-valve for the admission and discharge of steam into the atmosphere, and the other so arranged as to work expansively (being provided with the Carless variable expansion gear) and working in connection with a condenser. I have also shown two diagrams of the steam pressures at each part of the stroke, assuming in both cases the same initial steam pressure of 60 lbs. per square inch above the atmospheric pressure, and the same load upon the engine. They show that in the latter case the same amount of work is accomplished by filling the cylinder roughly speaking up to one-third part of the length as in the other by filling it entirely. Here we have then an easy and feasible plan of saving two-thirds of the fuel used in working an ordinary high-pressure engine, and yet probably the greater number of the engines now actually at work are of the wasteful type. Nor are the indications of theory in this case (or in any other when properly interpreted) disproved by practice; on the contrary, an ordinary non-expansive non-condensing engine requires commonly a consumption of from 10 to 12 lbs. per horse-power per hour, whereas a good expansive and condensing engine accomplishes the same amount of work with 2 lbs. of coal per hour, the reason for the still greater economy being, that the cylinder of the good engine is properly protected by means of a steam-jacket and lagging against loss by condensation within the working cylinder, and that more care is generally bestowed upon the boiler and the parts of the engine, to ensure their proper working condition.

A striking illustration of what can be accomplished by way of accuracy in a short space of time was brought to light by the Institute of Mechanical Engineers, over which at present I have the honour to preside. In holding their annual general meeting in Liverpool in 1863, they instituted a careful inquiry into the consumption by the best engines in the Atlantic Steam Service, and the result showed that it fell in no case below 4½ lbs. per indicated horse power per hour. Last year they again assembled with the same object in view in Liverpool, and Mr. Bramwell produced a table showing that the average consumption by 17 good examples of compound expansive engines did not exceed 2¼ lbs. per indicated horse power per hour. Mr. E. A. Cowper has proved a consumption not exceeding 1¾ lbs. per indicated horse power per hour in a compound marine engine constructed with an intermediate superheating vessel, in accordance with his plans, nor are we likely to stop long at this point of comparative perfection, for in the early portion of my address I have endeavoured to prove that the theoretical perfection would only be attained if an indicated horse power was produced with $\frac{1}{5\cdot5}$ lbs. of pure carbon, or say ¼ lb. of ordinary steam coal.

Here then we have two distinct margins to work upon, the one up to the limit of say 2 lbs. per horse power per hour, which has been practically reached in some and may be reached in all cases, and the other up to the theoretical limit of ¼ lb. per horse power per hour which can never be absolutely reached, but which inventive power may and will enable us to approach!

Domestic Consumption—The wastefulness of the domestic hearth and kitchen fire is self-evident. Here only the heat radiated from the fire itself is utilised, and the combustion is generally extremely imperfect, because the iron back and excessive supply of cold airs, check combustion before it is half completed. We know that we can heat a room much more economically by means of a German stove, but to this it may be very properly objected that it is cheerless, because we do not see the fire or feel its drying effect on our damp clothing; it does not provide, moreover, in a sufficient degree for ventilation, and makes the room feel stuffy. These are, in my opinion, very potent objections, and economy would not be worth having if it could only be obtained at the expense of health and comfort. But there is at least one grate that combines an increased amount of comfort with reasonable economy, and which, although accessible to all, is as yet very little used. I refer to Captain Galton's "Ventilating Fireplace," of which you observe a diagram upon the wall. This fireplace does not differ in external appearance from an ordinary grate, except that it has a higher brick back, which is perforated at about mid-height to admit warmed air into the fire to burn a large proportion of the smoke which is usually sent up the chimney unburnt, for no better purpose than to poison the atmosphere we have to breathe.

The chief novelty and merit of Captain Galton's fireplace consists, however, in providing a chamber at the back of the grate, into which air passes directly from without, becomes moderately heated (to 84° Fah.), and, rising in a separate flue, is injected into the room under the ceiling with a force due to the heated ascending flue. A plenum of pressure is thus established within the room whereby indraughts through doors and windows are avoided, and the air is continually renewed by passing away through the fireplace chimney as usual. Thus the cheerfulness of an open fire, the comfort of a room filled with fresh but moderately warmed air, and great economy of fuel, are happily combined with unquestionable efficiency and simplicity; and yet the grate is little used, although it has been fully described in papers communicated by Captain Galton, and in an elaborate report made by General Morin, le Directeur du Conservatoire des Arts et Metiers of Paris, which has also appeared in the English language.

The slowness with which this unquestionable improvement finds practical application is due, in my opinion, to two circumstances,—the one is, that Captain Galton did not patent his improvement, which makes it nobody's business to force it into use, and the other may be found in the circumstance that houses are, to a great extent, built only to be sold and not to be lived in. A builder thinks it a good speculation to construct a score of houses after a cheap design, in order to sell them, if possible, before completion, and the purchaser immediately puts up the standard bill of "Desirable Residences to Let." You naturally would think that in taking such a house you had only to furnish it to your own mind, and be in the enjoyment of all reasonable creature comfort from the moment you enter the same. This fond hope is destined, however, to cruel disappointment; the first evening you turn on the gas, you find that although the pipes are there, the gas prefers to pass out by the joints into the room instead of by the burners; the water in like manner takes its road through the ceiling, bringing down with it a patch of plaster on to your carpet. But worst of all, the fire-grates (of a size irrespective, probably, of the size of the room), absolutely refuse to avail themselves of the chimney flues preferring to send the volumes of smoke into the room. Plumbers and chimney doctors are now put into requisition, pulling up floors, dirtying carpets, and putting up gaunt-looking chimney-pots; the grates themselves have to be altered again and again, until by slow degrees the house becomes habitable in a degree, although you now only become fully aware of innumerable drawbacks of the arrangements adopted. Nevertheless, the house has been an excellent one to sell, and the builder adopts the same pattern for another block or two in an increasing neighbourhood. Why should this builder adopt Captain Galton's fireplace? It will not cost him much, it is true, and it will save the tenant a great deal in his annual coal bill, not to speak of the comfort it would give him and his family; but nobody demands it of him, it would give him some trouble to arrange his details and subcontracts, which are all settled beforehand, and so he goes on building and selling houses in the usual routine way. Nor will this state of things be altered until the dwellers in houses will take the matter in hand, and absolutely refuse to put up with builders' ways, or, what is still better, get builders who will put up houses in their way. This is done to some extent by building societies, but there is as yet too much of the old leaven left in the trade, and the question itself too little understood.

Consumption in Smelting Operations.—We now come to the third branch of consumption, the smelting or metallurgical furnace, which consumes about 40,000,000 of the 120 millions of the fuel produced. Here also is great room for improvement, the actual fuel consumed in heating a ton of iron up to the welding point or of melting a ton of steel is more in excess of the theoretical quantity required for these purposes than is the case with regard to the production of steam power and to domestic consumption. Taking the specific heat of iron at ·114 and the welding heat at 2,700° F. it would require 2,700 × ·144—307 heat units to heat 1 lb. of iron. A pound of pure carbon developes 14,500 heat units, a pound of common coal 12,000, and therefore one ton of coal should bring 39 tons of iron up to the welding point. In an ordinary re-heating furnace a ton of coal heats only 1⅔ ton of iron, and therefore produces only $\frac{1}{23}$rd part of the maximum theoretical effect. In melting one ton of steel in pots 2½ tons of coke are consumed, and taking the melting point of steel at 3,600° F. the specific heat at ·119 it takes ·119 × 3,600 = 428 heat units to melt a pound of steel, and taking the heat producing power of common coke also at 12,000 units, one ton of coke ought to be able to melt 28 tons of steel. The Sheffield pot steel melting furnace therefore only utilises $\frac{1}{70}$th part of the theoretical heat developed in the combustion. Here therefore is a very wide margin for improvement, to which I have specially devoted my attention for many years, and not without the attainment of useful results. I have since the year

1846, or very shortly after the first announcement of the dynamical theory, devoted my attention to a realisation of some of the economic results which that theory rendered feasible. I fixed upon the regenerator as the appliance which, without being capable of reproducing heat when once really consumed, is extremely useful for temporarily storing such heat as cannot be immediately utilised in order to impart it to the fluid or other substance which is employed in continuation of the operation of heating or of generating force.

Without troubling you with an account of the gradual progress of these improvements, I will describe to you shortly the furnace which I now employ for melting steel. This consists of a furnace bed made of very refractory material, such as pure silica sand and silica or Dina's brick, under which four regenerators or chambers filled with checkerwork of brick are arranged in such a manner that a current of combustible gas passes upward through one of these regenerators, while a current of air passes upwards through the adjoining regenerator, in order to meet in combustion at the entrance into the furnace chamber. The products of combustion, instead of passing directly to the chimney as in an ordinary furnace, are directed downwards through the two other regenerators on their way towards the chimney, where they part with their heat to the checkerwork in such manner that the highest degree of heat is imparted to the upper layers, and that the gaseous products reach the chimney comparatively cool (about 300° F.) After going on in this way for half-an-hour, the currents are reversed by means of suitable reversing valves, and the cold air and combustible gas now enter the furnace chamber, after having taken up heat from the regenerator in the reverse order in which it was deposited, reaching the furnace therefore nearly at the temperature at which the gases of combustion left the same. A great reversion of temperature within the chamber is the result, and the two first-mentioned regenerators are heated to a higher degree than the latter. It is easy to conceive that in that way, heat may be accumulated within the chamber to an apparently unlimited extent, and with a minimum of chimney draught.

Practically the limit is reached at the point where the materials composing the chamber begin to melt. Whereas a theoretical limit also exists in the fact that combustion ceases at a point which has been laid by St. Clair Deville at 5000° Fah., and which has been called by him the point of dissociation. At this point hydrogen might be mixed with oxygen and yet the two would not combine, showing that combustion really only takes place between the units of temperature of about 500° and 4,500° Fah.

To return to the regenerative gas-furnace. It is evident that there must be economy where, within ordinary limits, any degree of heat can be obtained, while the products of combustion pass in the chimney only 300° hot. Practically a ton of steel is melted in this furnace with 12 cwt. of small coal consumed in the gas-producer, which latter may be placed at any reasonable distance from the furnace, and consists of a brick chamber containing several tons of fuel in a state of slow disintegration. In large works, a considerable number of these gas-producers are connected by tubes or flues with a number of furnaces. Collateral advantages in this system of heating, which is now extensively used in this and other countries, are that no smoke is produced, and that the works are not encumbered with solid fuel and ashes.

It is a favourite project of mine, which I have not had an opportunity yet of carrying practically into effect, to place these gas-producers at the bottom of coal-pits. A gas shaft would have to be provided to conduct the gas to the surface, the lifting of coal would be saved, and the gas in its ascent would accumulate such an amount of forward pressure that it might be conducted to a distance of several miles to the works or places or consumption. This plan, so far from being dangerous, would insure a perfect ventilation of the mine, and would enable us to utilise those waste deposits of small coal (amounting on the average to 20 per cent.) which are now left unutilised within the mine.

Another plan of the future which has occupied my attention is the supply of towns with heating gas for domestic and manufacturing purposes. In the year 1863 a company was formed, with the concurrence of the corporation of Birmingham, to provide such a supply in that town at the rate of 6*d.* per 1,000 cubic feet: but the Bill necessary for that purpose was thrown out in the Committee of the House of Lords because their Lordships thought that if this was as good a plan as it was represented to be, the existing gas companies would be sure to carry it into effect. I need hardly say that the existing companies have not carried it into effect, having been constituted for another object, and that the realisation of the plan itself has been indefinitely postponed.

Coal Question.—Having now passed in review the principal applications of fuel, with a view chiefly to draw the distinction between our actual consumption and the consumption that would result if our most approved practice was made general; and having, moreover, endeavoured to prove to you which are the ultimate limits of consumption which are absolutely fixed by theory, but which we shall never be able to realise completely, I will now apply my reasoning to the coal question of the day.

In looking into the "Report of the Select Committee appointed to Inquire into the Causes of the present Dearness of Coal," we find that in 1872 no less than 123,000,000 tons of coal were got up from the mines of England and Wales, notwithstanding famine prices and the colliers' strikes. In 1862 the total getting of coal amounted to only 83,500,000, showing a yearly average increase of consumption of 4,000,000 tons. If this progressive increase continues, our consumption will have reached, thirty years hence, the startling figure of 250,000,000 tons per annum, which would probably result in an increase of price very much in excess of limits yet reached. In estimating last year's increase of price, which has every appearance of being permanent, at 8*s.* per ton all round, and after deducting the 13,000,000 tons which were exported abroad, we find that the British consumer had to pay 44,000,000*l.* more than the market value of former years for his supply of coal—a sufficient sum, one would think, to make him look earnestly into the question of "waste of fuel," which, as I shall presently be able to show, is very great indeed. The Select Committee just quoted sums up its report by the following expression:—"The general conclusion to be drawn from the whole evidence is, that though the production of coal increased in 1872 in a smaller ratio than it had increased in the years immediately preceding, yet if an adequate supply of labour can be obtained, the increase of production will shortly keep pace with that of the last few years."

This is surely a very insufficient conclusion to be arrived at by a Select Parliamentary Committee after a long and expensive inquiry, and the worst of it is, that it stands in direct contradiction with the corrected table given in the same report, which shows that the progressive increase of production has been fully maintained during the last two years, having amounted to 5,826,000 for 1871, and 5,717,000 for 1872; whereas the average increase during the last ten years has only been 4,000,000 tons. It is to be hoped that Parliament will not rest satisfied with such a negative result, but will insist to know what can be done to re-establish a proper balance between demand and supply of coal in preventing its conversion into smoke or other equally hurtful or useless forms of energy

In taking the 105 million tons of coal consumed in this country last year for our basis, I estimate that, if we could make up our minds to consume our coal in a careful and judicious manner, according to our present lights, we should be able to reduce that consumption by 50 million tons. The realisation of such an economy would certainly involve very considerable expenditure of capital, and must be a work of time, but what I contend is that our progress in effecting economy ought to be accelerated in order to establish a balance between the present production and the ever-increasing demand for the effects of heat.

In looking through the statistical returns of the progressive increase of population, of steam power employed, and of production of iron and steel, &c., I find that our necessities increase at a rate of not less than 10 per cent. per annum, whereas our coal consumption increases only at the rate of 4 per cent., showing that the balance of 6 per cent. is met by what may be called our "intellectual progress." Now considering the enormous margin for improvement before us, I contend that we should not rest satisfied with this rate of intellectual progress, which involves an annual deficit of 4,000,000 tons to be met by increased coal consumption, but that we should bring our intellectual progress up to the rate of our industrial progress, by which means we should make the coal production nearly a constant quantity for several generations to come; by which time our successors may be expected to have effected another great step in advance towards the theoretical limit of effect, which, as we have seen, lays so far above any actual result which we have as yet attained to, that an annual consumption of 10 million tons would give more than the equivalent of the heat energy which we actually consume.

Solar Heat.—I have endeavoured to show, in the early part of this lecture, that all available energy upon the earth, excepting the tidal wave, is derived from the sun, and that the amount of heat radiated year by year, could be measured by the evaporation of a layer of water 14 ft. thick, spread over the entire surface, which again would be represented by the combustion of a layer of coal, covering our entire globe, 1 ft. in thickness. The amount of heat radiated away from the sun would be represented by the annual combustion of a thickness of coal 17 miles thick, covering its entire surface, and it has been a source of wonderment with natural philosophers how so prodigious an amount of heat could be given off year after year without any appreciable diminution of the sun's heat having become observable.

Recent researches with the spectroscope, chiefly by Norman Lockyer, have thrown much light upon this question. It is now clearly made out that the sun consists near the surface, if not throughout its mass, of gaseous elementary bodies, and in a great measure of hydrogen gas, which cannot combine with the oxygen present, owing to great elevation of temperature (due to the original great compression) which has been estimated at from 20,000° to 22,000° Fah This chemically inert and comparatively dark mass of the sun is surrounded by the photosphere where the gaseous constituents of the sun rush into combustion, owing to reduction of temperature in consequence of their expansion and of radiation of heat into space; this photosphere is surrounded in its turn by the chromosphere, consisting of the products of combustion, which, after being cooled down through further loss of heat by radiation, sink back, owing to their acquired density, towards the centre of the sun, where they become again intensely heated through compression and are dissociated or split up again into their elements at the expense of internal solar heat. Great convulsions are thus continually produced upon the solar surface, resulting frequently in explosive actions of extraordinary magnitude, when masses of living fire are projected a thousand miles or more upward, giving rise to the phenomena of sun-spots and of the corona which is visible during the total eclipses of the sun The sun may therefore be looked upon in the light of a gigantic gas-furnace, in which the same materials of combustion are used over and over again.

It would be impossible for me at this late hour to enter deeper upon speculations regarding the "regeneration of the sun's heat upon its surface," which question is replete with scientific and also practical interest, because Nature is our safest teacher, and in comprehending the great works of our Creator we shall learn how to utilise to the best advantage those stores of potential energy in the shape of coal which have providentially been placed at our disposal.

COALS AND COAL PLANTS[*]

PROF. WILLIAMSON said that his distinguished friend, their president, had spoken the truth to a certain extent; but at the same time there was in what he had said a slight measure of what a particular school would call the *suggestio falsi*. He believed that if a balance of account could be struck between them it would be found that he (the lecturer) was enormously the gainer from the fact that he enjoyed the same name as the president As far as he could arrange the balance, it was this—that their president was debtor one dinner which he (the lecturer) always contended his friend had got because he had received a card of invitation which did not belong to him—while, on the other hand, there was an item of credit to the extent of all the learning the president displayed at every meeting of the British Association, but for which, at least in the North of England, he (Prof. W. C Williamson) was usually credited. Under these circumstances he thought it would be seen that instead of his being the loser he was in reality an enormous gainer.

He remembered a distinguished friend of his, a member of the House of Commons, telling him that whenever an individual rose in that house to speak on a subject on which he was known to have written a book, the house speedily became emptied, because the members were alarmed at the idea of a speech from a man who had an inveterate hobby. He presumed, however, that he stood there that night simply because he had a hobby; but he would promise not to ride it too far or inflict it too long upon his audience. Furthermore when he remembered how short was the time since Prof. Huxley had addressed a Bradford audience on the subject of coal, he was somewhat appalled at his own boldness in having ventured to deal with a similar matter at the present moment. But luckily for him science did not stand still, and although so short a time had elapsed since Prof. Huxley had delivered the lecture referred to, there was much now to be said on the subject which could not have been said then. Still, with the magnificent address of Prof. Huxley within reach, it would not be necessary to detain the auditory long on the general theories which were now so widely accepted with reference to the origin of coal.

Prof. Phillips, in his address to the Geological Section on the previous morning, had reminded them how short a time it was—the period being within his own life-time—since the vegetable origin of coal was broadly and openly disputed. It would, however, be difficult now to find any one at all enlightened on the subject who would venture to dispute that the origin of coal was vegetable. In the same way another hypothesis—known by the title of the drift theory—had once been very generally accepted. Men who admitted the conclusion that coal had once been a mass of vegetable life differed as to the method by which that vegetable mass had found its way into its present position. The majority of the older geologists believed that coal had been conveyed into those positions by water—that large quantities of vegetable material had been brought down great rivers like the Mississippi or the Ganges, that these vegetable rafts, as they might be termed, had accumulated in the estuaries and the ocean, and that when they had become thoroughly water-logged, they had sunk to the bottom and formed accumulations of vegetable elements sufficient to constitute the existing coal-beds. Thanks to the labours of a series of indefatigable workers like the late Mr. Bowman, Mr. Binney, Sir Wm. Logan, and others, we now had a clearer and much more probable conception as to what coal originally was.

It must be understood that although the earth was popularly regarded as the type of everything that was stable and immovable, this was a very erroneous idea; for old mother earth was about one of the most fickle and inconstant of all the jades with which men had deal. She was never still. It happened that at the present day there were certain regions, such as the volcanic regions, which were always moving upwards, like the more aspiring of the youths of Bradford, while there were others, such as the coral regions, which were steadily going downward, like those less fortunate youths who did not succeed in the race of life. So it had been in the olden time. The coal beds appeared to have accumulated in the latter class of areas—the areas of depression—geographical areas in which the earth had a tendency to sink below the level of the ocean. Upon such areas mud and silt had accumulated until the deposit thus formed had reached the level of the water, and then came what would appear to have been highly necessary as a preliminary to the growth of the coal material, namely, a bed of blue mud. It was not known why that blue mud was there or whence it came, but it was as certain as that garden plants required favourable soils for their development, that whatever its cause the blue mud was the soil which seemed to have been preferred by the great majority of the plants constituting the forests of the carboniferous era. In it the minute spores or seeds of the vegetables which afterwards became coal, germinated and struck root, until eventually the muddy soil became converted into a magnificent and almost tropical forest. As the forest grew the spores fell from the trees, the half-dead leaves and decayed branches also dropped, and by-and-by the stems themselves gave way, and thus was accumulated an immense amount of vegetable matter. This, in the progress of time, sank below the water level, and more mud being deposited on the top of the coal, the new formation in turn underwent the same processes as its predecessors, until at length a new forest was formed to share the same fate as that which had gone before it. The process was repeated again and again, until at length we had an accumulation of materials, mixtures of the various substances he had spoken of, alternating with beds of coal, until we had a vertical thickness of rock varying from three, four, or five, to as much as eight or ten thousand feet.

But while these general truths were accepted with little or no reservation, there were one or two points contained in Prof. Huxley's lecture upon which he would venture for a moment to dwell. In that lecture he properly laid stress upon certain minute bodies that were found in the interior of coal.

[The lecturer here pointed to a diagram representing a vertical section of coal, and he also exhibited various pieces of coal, one of which he held in the position it occupied in the coal bed. Another diagram, he said, represented a quantity of black coaly matter arranged in layers, and embedded in this matter were some small bodies which had been flattened by the pressure of the coal, and by the superimposed beds between the coal.]

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[*] Abstract of Lecture delivered before the British Association, at Bradford, by Prof. W. C. Williamson, F.R.S.

Prof. Huxley spoke of these bodies under the name of sporangia, or spore cases. Now, he (Prof. Williamson) had come to the conclusion that they were all spores of two classes—the larger ones called macro-spores, and the smaller ones micro-spores. A large number of the plants, if not all, found in the coal-measures belonged to the cryptogamic plants, in which was found no trace of seeds or flowers. The reproductive bodies that took the place of seeds were little bud-like structures, to which the name of spores was given. In a certain class of those plants, the club-mosses, for instance, were two kinds of these spores. The sporangia of club mosses and similar plants never became detached from their parent stem. They burst and liberated multitudes of contained spores, which were objects like those so abundant in many coals. But these spores did not play so important a part in the formation of coal as Prof. Huxley supposed. On examining these objects it was found that each of the little rounded discs exhibited three ridges that radiated in a triangular manner from a common centre. These discs were originally masses of protoplasm, lodged within a mother-cell. By-and-by each of these masses broke up into three or four parts; and it was found that to accommodate one another in the interior of their circular chamber, they mutually pressed one another. To illustrate the mutual compression, Prof. Williamson produced a turnip, which he had cut into four parts, that corresponded exactly, he said, in their arrangement with the arrangement of the four spores in the interior of the mother cell.

Then Prof. Huxley held that coal consisted of two elements. Prof. Williamson, exhibiting again a piece of coal said the dirty blackening surface was a thin layer of little fragments of woody structures, vegetable tissues of various kinds, known by the name of mineral charcoal. These layers of mineral charcoal were exceedingly numerous. Prof. Huxley, recognising the abundance and significance of these little spore-like bodies, thought that mineral charcoal formed only a portion, and a limited portion, while the great bulk of black coaly matter was really a mass of carbon derived from chemically altered spores. He thought that on this point they would be obliged somewhat to differ from Prof. Huxley.

The bed which had been most widely quoted as containing most beautiful spores was found in the district of Bradford. If everything decayed, and Bradford was by an exceedingly improbable combination of circumstances to pass out of memory, it would be remembered in scientific history as the locality in which the "better bed" was found. The fragment he held in his hand was a fragment of the better bed. On examining it for a moment through a magnifying glass he saw that it was a solid mass of mineral charcoal, yet the microscope revealed in it no trace whatever of organic structure. Therefore, while Prof. Huxley divided coal into two elements—mineral charcoal and coal proper, including in the latter term altered spores—he would say that coal consisted of three elements—mineral charcoal, black coal derived from mineral charcoal, and spores.

This outline of the history of coal led them to the independent conclusion that two elements were mingled in coal; the vegetable *débris*, or broken up fragments of the plants of the carboniferous age were intermingled with the peculiar spores to which Prof. Huxley had so properly called attention. In proceeding to deal further with the plants of which coal was formed, the lecturer took occasion to acknowledge with thanks the loan of certain valuable specimens to illustrate his discourse from the Bradford Museum. One of these specimens was a most rare and valuable specimen which he would be glad to take away with him to Owens College, if he had the chance; but he was afraid the Bradford people were too Conservative to stand that.

After giving a number of botanical and other details with regard to the plants of which coal was formed, he said our knowledge of this subject resolved itself into two divisions, viz., that of the outward forms of plants and that of their inward organisation. These two lines of inquiry did not always run parallel, and the one great object of recent research had been to make them do so. Specimens throwing light on the subject had been found at Arran, Burntisland, Oldham, Halifax, Autun in France, and elsewhere, and upon these a host of observers had been and still were working. It had long been known that most, if not all, the coal plants belonged to two classes, known as the Cryptogamia, or flowerless plants, and the gymnospermous exogens, represented by the pines and firs. All recent inquiries added fresh strength to this conclusion. One of the most important of these groups was that of the Equiseta or horse tails, and which were represented in the coal by the Calamites. The long cylindrical stems, with their transverse joints and longitudinal grooves, were shown to be casts of mud or sand, occupying the hollows in the piths of the living plants. Each of these piths was surrounded by a thick zone of wood, which again was invested by an equally thick layer of bark. Specimens were shown in which, though the pith was only an inch in diameter, the wood and bark combined formed a cylinder 4 inches thick, giving a circumference of at least 27 inches to the living stem. But there exist examples of the pith casts alone, which are between 2 and 3 feet in diameter. It was evident, therefore, he concluded, that the Calamites became true forest trees, very different from their living representatives—the horse tails of our ponds and marshes.

After describing the organisation of these plants, the Professor proceeded to describe the Lycopods of the coal measures as represented by the Lepidodendra, Sigillariæ, and a host of other well-known plants. The living Lycopods, whether seen at home or in tropical forests, are dwarf herbaceous plants, but in the carboniferous age they became lofty forest trees, 100 feet high, and ten or twelve feet in circumference. To enable such lofty stems, with their dense mass of serial branches and foliage, to obtain nutrition, an organisation was given to them approaching more nearly to that of our living forest trees than to that of any recent cryptogams. A succession of woody layers was added to the exterior of those previously existing; so that as the plant rose into the air the stem became strengthened by these successive additions to the vascular tissue. As this process advanced it was accompanied by other changes, producing a large central pith, and two independent vascular rings immediately surrounding the pith, and the relations of these various parts to the roots, and leaves, as well as to the nutrition of the plants, was pointed out. The fruits of these Lycopods were then examined. The existence of two classes of spores corresponding in functions to the stamens and pistils of flowering plants, was dwelt upon, and one of these classes (the macrospores) was shown to be so similar to the small objects found in coal, as to leave no doubt that those objects were derived from the lepidodendroid and sigillarian trees which constituted the large portion of the forest vegetation.

Certain plants known as Asterophyllites were next examined. The ferns were also reviewed, and shown to be as remarkable for the absence of exogenous growth from their stems as the Calamites and Lycopods were for its conspicuous presence. The structure of some stems supposed to represent palms was shown to be that of a fern, there being no true evidence that palms existed in that age. The plants known as coniferous plants, allied to pines and firs, were described, and their peculiar fruits, so common at Peel, in Lancashire, were explained, and some plants of unknown affinities, but beautiful organisation, were referred to. The physiological differences between these extinct ferns, and other plants especially in their marvellous *quasi*-exogenous organisation, was pointed out, and the lecturer concluded by showing how unvarying must have been the green hue of the carboniferous forests, owing to the entire absence from them of all the gay colours of the flowering plants which form so conspicuous a feature in the modern landscape, especially in the temperate and colder regions. The antiquity of the mummy, he added, was as nothing compared with the countless ages that had rolled by since these plants lived, and yet they must not forget that every one of those plants, living in ages so incalculably remote, had a history, an individuality as distinct and definite as our own. They would probably be inclined to ask the question, When did all these things take place? Echo answered, When?

THE BRITISH ASSOCIATION

THE Bradford Meeting has been on the whole a good one; though there have been no salient discussions, the papers read have been all up to a good useful average. Mr. Ferrier's paper on the brain was a surprise to many, we believe, and the only approach to a genuine sensation was the appearance of Captain Markham, R.N., in the Geographical Section on Saturday, he having arrived only the previous day at Dundee in the *Arctic*, along with the *Polaris* men.

The private hospitality of the Bradfordians has been magnificent, but the hotel charges, every one admits, have been simply monstrous. We quite agree with the remarks made in the last number of the *Pharmaceutical Journal* on this subject, and do not think that hotel-keepers by so recklessly increasing their ordinary charges do themselves or their town any good. We hope that in future the authorities of towns visited by the British Association will devise some means of counteracting such proceedings, as they no doubt tend to diminish the number of visitors. The number of tickets of all classes issued this year is not much above 1,800, being several hundreds under that of last year; no doubt the relative attractions of Brighton and Bradford will partly account for this.

The *soirée* in St. George's Hall last Thursday was a great success; indeed all the arrangements for the meeting have been satisfactory. The public lectures, by Profs. W. C. Williamson, Clerk-Maxwell, and Dr. Siemens were well attended, but the proportion of the working-classes present at the lecture on Fuel, which was specially intended for their benefit, was very small. Indeed, many are of opinion that this lecture should be abolished, seeing that so few workpeople take advantage of it, and that a lecture should be given every night, or three or four times during the meeting, to working-men who are registered, as at the School of Mines, in order to secure that the right sort of people gain admission.

This year the Association gave another lesson to Government. Last year, it may be remembered, the question of the Tides was given up by the Association; this year they have done the same to the Rainfall question, as being a work which it is the interest of the nation to see done. We hope the nation will see that it is attended to in the proper quarter.

On Monday Prof. Smith proposed Dr. Tyndall as president of next year's meeting; and it was somewhat of a surprise to most present when the Mayor of Belfast patriotically proposed that Prof. Andrews of that city should preside over a meeting to be held in Ireland. Prof. Andrews had been first suggested by the Council, and his friends were consulted, but it was found that the state of his health rendered it unadvisable to press the honour upon him.

Belfast is the place of meeting next year, and Bristol, it has been settled, will be visited by the Association in 1875; there is a tacit understanding that Glasgow will be the rendezvous for 1876, the Lord Provost and a strong deputation being present on Monday to earnestly urge the claims of that important place.

The Report of the Council for the year 1872-3 was presented to the General Committee at Bradford, on Wednesday, 17th September. The Council have had under their consideration the three Resolutions which were referred to them by the General Committee at Brighton. The first Resolution was—"That the Council be requested to take such steps as they deem desirable to induce the Colonial Office to afford sufficient aid to the Observatory at Mauritius to enable an investigation of the cyclones in the Indian Ocean to be carried on there."

In accordance with this Resolution, a correspondence took place between Dr. Carpenter, the President of the Association, and the Right Honourable the Earl of Kimberley, Secretary of State for the Colonies.

In consequence of this correspondence, the Council requested the President to urge upon the Lords Commissioners of Her Majesty's Treasury the desirability of affording such pecuniary aid to the Mauritius Observatory as would enable the Director to continue his observations on the periodicity of the cyclones; and an intimation has been received from Her Majesty's Government that an inquiry into the condition, size, and cost of the establishment of the Mauritius is now being conducted by a Special Commission from England, pending which inquiry no increase of expenditure upon the Observatory can be sanctioned; but that when the results of this inquiry shall be made known, the Secretary of State for the Colonies will direct the attention of the Governor to the subject.

The second Resolution referred to the Botanical establishment at Kew, but happily the Council have not deemed it necessary to take any action upon this Resolution.

Third Resolution:—"That the Council be requested to take such steps as they may deem desirable to urge upon the Indian Government the preparation of a Photoheliograph and other instruments for solar observation, with the view of assisting in the observation of the Transit of Venus in 1874, and for the continuation of solar observations in India."

The Council communicated with his Grace the Duke of Argyll, the Secretary of State for India, upon the subject, with the result explained in the following letter:—

"India Office, February 28, 1873.

"Sir,—With reference to my letter of the 13th of December last, relative to an observation in India of the Transit of the planet Venus in December 1874, I am directed to state, for the information of the Council of the British Association for the Advancement of Science, that the Secretary of State for India in Council, having reconsidered this matter, and looking to the number of existing burdens on the revenues of India, and to the fact that the selection of any station in that country was not originally contemplated for 'eye-observations' of the transit, has determined to sanction only the expenditure (356*l.* 7*s.* 6*d.*) necessary for the purchase and packing of a Photoheliograph, and any further outlay that may be requisite for the adaptation of such instruments as may be now in India available for the purpose of the proposed observation.

"The Duke of Argyll in Council has been led to sanction thus much of the scheme proposed by Lieut. Colonel Tennant, in consequence of the recommendation submitted by the Astronomer Royal in favour of the use of photography for an observation of the transit at some place in Northern India.

"I am, Sir, Your obedient Servant,
(Signed) "Herman Merivale."

"William B. Carpenter, Esq., British Association."

A Committee was appointed at Exeter in 1869, on the Laws Regulating the Flow and Action of Water holding Solid Matter in Suspension, with authority to represent to the Government the desirability of undertaking Experiments bearing on the subject. The Committee presented a Memorial to the Indian Government, who have recently intimated their intention of advancing a sum of 2,000*l.* to enable Mr. Login to carry on experiments.

The Council have added the following list of names of gentlemen present at the last meeting of the Association to the list of Corresponding Members: M. C. Bergeron, Lausanne; Prof. E. Croullebois, Paris; Prof. G. Devalque, Liège; M. W. De Fonvielle, Paris; Prof. Paul Gervais, Paris; Prof. James Hall, Albany, New York; Mr. J. E. Hilgard, Coast Survey, Washington; M. George Lemoine, Paris; Prof. Victor von Richter, St. Petersburg; Prof. Carl Semper, Wurtzburg; Prof. A. Wurtz, Paris.

We now pass on at once to the Sectional work, delaying a reference to the Scientific grants made this year, and the concluding business till next week.

SECTION A.

OPENING ADDRESS BY THE PRESIDENT, PROF. HENRY J. S. SMITH, M.A., LL.D., F.R.S.

FOR several years past it has been the custom for the president of this section, as of the other sections of the Association, to open its proceedings with a brief address. I am not willing upon this occasion to deviate from the precedent set by my predecessors, although I feel that the task presents peculiar difficulties to one who is by profession a pure mathematician, and who, in other branches of science, can only aspire to be regarded as an amateur.

But, although I thus confess myself a specialist, and a specialist it may be said of a narrow kind, I shall not venture, in the few remarks which I now propose to make, to indulge my own speciality too far.

I am well aware that we are certain, in this section, to have a sufficient number of communications, which of necessity assume a special and even an abstruse character, and which, whatever pains may be taken to give them clearness, and however valuable may be the results to which they lead, are nevertheless extremely difficult to follow, not only for a popular audience, but even for men of science whose attention has not been specially, and recently, directed to the subject under discussion. I should think it, therefore, almost unfair to the section, if at the very commencement of its proceedings I were to attempt to direct its attention in any exclusive manner to the subject which, I confess, if I were left to myself, I should most naturally have chosen—the history of the advances that have been made during the last ten or twenty years in mathematical science. Instead, therefore, of adventuring myself on this difficult course, which, however, I strongly recommend to some successor of mine less scrupulous than myself, I propose, though at the risk of repeating what has been better said by others before me, to offer some general considerations which may have a more equal interest for all those who take part in the proceedings of this section, and which appear to me at the present time to be more than usually deserving of the notice of those who desire to promote the growth of the scientific spirit in this country. I intend, therefore, while confining myself as strictly as I can to the range of subjects belonging to this section, to point out one or two, among many, of the ways in which sectional meetings, such as ours, may contribute to the advancement of science.

We all know that Section A of the British Association is the section of mathematics and physics; and I dare say that many of us have often thought how astonishingly vast is the range of subjects which we slur over, rather than sum up, in this brief designation. We include the most abstract speculations of pure mathematics, and we come down to the most concrete of all phenomena—the most every-day of all experiences. I think I have heard in this section a discussion on spaces of five dimensions, and we know that one of our committees, a committee which is of long-standing, and which has done much useful work, reports to us annually on the Rainfall of the British Isles. Thus our wide range covers the mathematics of number and quantity in their most abstract forms, the mathematics of space, of time, of matter, of motion, and of force, the many sciences which we comprehend under the name of astronomy, the theories of sound, of light, heat, electricity; and besides the whole physics of our earth, sea, and atmosphere, the theory of earthquakes, the theory of tides, the theory of all the movements of the air, from the lightest ripple that affects the barometer up to a cyclone. As I have already said, it is impossible that communications on all these subjects should be interesting, or indeed intelligible, to all our members; and, notwithstanding the pains taken by the committee and by the secretaries to classify the communications offered to us, and to place upon the same days those of which the subjects are cognate to one another, we cannot doubt that the disparateness of the material which comes before us in this section is a source of serious inconvenience to many members of the Association. Occasionally, too, the pressure upon our time is very great, and we are obliged to hurry over the discussions on communications of great importance, the number of papers submitted to us being, of course, in a direct proportion to the number of the subjects included in our programme. It has again and again been proposed to remedy these admitted evils by dividing the section, or at least by resolving it into one or more sub-sections. But I confess that I am one of those who have never regretted that this proposal has not commended itself to the Association, or indeed to the section itself. I have always felt that by so sub-dividing ourselves we should run the risk of losing one or two great advantages which we at present possess; and I will briefly state what, in my judgment, these advantages are.

I do not wish to undervalue the use to a scientific man of listening to and taking part in discussions on subjects which lie wholly in the direction in which his own mind has been working. But I think, nevertheless, that most men who have attended a meeting of this Association, if asked what they have chiefly gained by it, would answer in the first place that they have had opportunities of forming or of renewing those acquaintances or intimacies with other scientific men which, to most men engaged in scientific pursuits, are an indispensable condition of successful work; and in the second place, that while they may have heard but little relating to their own immediate line of inquiry which they might not as easily have found in Journals or Transactions elsewhere, they have learned much which might otherwise have never come to their knowledge of what is going on in other directions of scientific inquiry, and that they have carried away many new conceptions, many fruitful germs of thought, caught perhaps from a discussion turning upon questions apparently very remote from their own pursuits. An object just perceptible on a distant horizon is sometimes better descried by a careless sideward glance than by straining the sight directly at it; and so capricious a gift is the inventive faculty of the human mind that the clue to the mystery hid beneath some complicated system of facts will sometimes elude the most patient and systematically conducted search, and yet will reveal itself all of a sudden upon some casual suggestion arising in connection with an apparently remote subject. I believe that the mixed character and wide range of our discussions has been most favourable to such happy accidents. But even apart from these, if the fusion in this section of so many various branches of human knowledge tends in some degree to keep before our minds the essential oneness of science, it does us a good service. There can be no question that the increasing specialisation of the sciences, which appears to be inevitable at the present time, does nevertheless constitute one great source of danger for the future progress of human knowledge. This specialisation is inevitable, because the further the boundaries of knowledge are extended in any direction, the more laborious and time-absorbing a process does it become to travel to the frontier; and thus the mind has neither time nor energy to spare for the purpose of acquainting itself with regions that lie far away from the track over which it is forced to travel. And yet the disadvantages of excessive specialisation are no less evident, because in natural philosophy, as indeed in all things on which the mind of man can be employed, a certain wideness of view is essential to the achievement of any great result, or to the discovery of anything really new. The twofold caution so often given by Lord Bacon against over-generalisation on the one hand, and against over-specialisation on the other, is still as deserving as ever of the attention of mankind. But in our time, when vague generalities and empty metaphysics have been beaten once, and we may hope for ever, out of the domain of exact science, there can be but little doubt on which side the danger of the natural philosopher at present lies. And perhaps in our section, as at present constituted, there is a freer and fresher air—we are, perhaps, a less inadequate representation of "that greater and common world" of which Lord Bacon speaks, than if we were subdivided into as many parts as we include—I will not say sciences—but groups of sciences. Perhaps there is something in the very diversity and multiplicity of the subjects which come before us which may serve to remind us of the complexity of the problems of science, of the diversity and multiplicity of nature.

On the other hand it is not, as it seems to me, difficult to assign the nature of the unity which underlies the diversity of our subjects, and which justifies, to a very great extent, the juxtaposition of them in our section. That unity consists not so much in the nature of the subjects themselves, as in the nature of the methods by which they are treated. A mathematician, at least—and it is as a mathematician I have the privilege of addressing you—may be excused for contending that the bond of union among the physical sciences is the mathematical spirit and the mathematical method which pervades them. As has been said with profound truth by one of my predecessors in this chair, our knowledge of nature, as it advances, continuously resolves differences of quality into differences of quantity. All exact reasoning—indeed all reasoning—about quantity is mathematical reasoning; and thus as our knowledge increases, that portion of it which becomes mathematical increases at a still more rapid rate. Of all the great subjects which belong to the province of this section, take that which at first sight is the least within the domain of mathematics—I mean meteorology. Yet the part which mathematics bears in meteorology increases every year, and seems destined to increase. Not only is the theory of the simplest instruments of meteorology essentially mathematical, but the discussion of the observations—upon which, be it remembered, depend the hopes which are already entertained with increasing confidence, of reducing the most variable and complex of all known phenomena to exact laws—is a problem which not only belongs wholly to mathematics, but which taxes to the utmost the resources of the mathematics which we now possess. So intimate is the union between mathematics and physics that probably by far the larger part of the accessions to our mathematical knowledge have been obtained by the efforts of mathematicians to solve the problems set to them by experiment, and to create "for each successive class of phenomena, a new calculus or a new geometry, as the case might be, which might prove not wholly inadequate to the subtlety of nature." Sometimes, indeed, the mathematician has been before the physicist, and it has happened that when some great and new question has occurred to the experimentalist or the observer, he has found in the armoury of the mathematician the weapons which he has needed ready made to his hand. But, much oftener, the questions proposed by the physicist have transcended the utmost powers of the mathematics of the time, and a fresh mathematical creation has been needed to supply the logical instrument requisite to interpret the new enigma. Perhaps I may be allowed to mention an example of each of these two ways in which mathematical and physical discovery have acted and re-acted on each other. I purposely choose examples which are well known and belong, the one to the oldest, the other to the latest times of scientific history.

The early Greek geometers, considerably before the time of Euclid, applied themselves to the study of the various curve lines, in which a conical figure may be cut by a plane—curve lines to which they gave the name, never since forgotten, of conic sections. It is difficult to imagine that any problem ever had more completely the character of a "problem of mere curiosity," than this problem of the conic sections must have had in those earlier times. Not a single natural phenomenon which in the state of science at that time could have been intelligently observed was likely to require for its explanation a knowledge of the nature of these curves. Still less can any application to the arts have seemed possible; a nation which did not even use the arch were not likely to use the ellipse in any work of construction. The difficulties of the inquiry, the pleasure of grappling with the unknown, the love of abstract truth, can alone have furnished the charm which attracted some of the most powerful minds in antiquity to this research. If Euclid and Apollonius had been told by any of their contemporaries that they were giving a wholly wrong direction to their energies, and that instead of dealing with the problems presented to them by nature were applying their minds to inquiries which not only were of no use, but which never could come to be of any use, I do not know what answer they could have given which might not now be given with equal, or even with greater justice, to the similar reproaches which it is not uncommon to address to those mathematicians of our own day who study quantics of n-indeterminates, curves of the nth order, and (it may be) spaces of n-dimensions. And not only so, but for pretty nearly two thousand years, the experience of mankind would have justified the objection: for there is no record that during that long period which intervened between the first invention of the conic sections and the time of Galileo and Kepler, the knowledge of these curves possessed by geometers was of the slightest use to natural science. And yet, when the fulness of time was come, these seeds of knowledge, that had waited so long, bore splendid fruit in the discoveries of Kepler. If we may use the great names of Kepler and Newton to signify stages in the progress of human discovery, it is not too much to say that without the treatises of the Greek geometers on the conic sections there could have been no Kepler, without Kepler no Newton, and without Newton no science in our modern sense of the term, or at least no such conception of nature as now lies at the basis of all our science, of nature as subject in its smallest as well as in its greatest phenomena, to exact quantitative relations, and to definite numerical laws.

This is an old story; but it has always seemed to me to convey a lesson, occasionally needed even in our own time, against a species of scientific utilitarianism which urges the scientific man to devote himself to the less abstract parts of science, as being more likely to bear immediate fruit in the augmentation of our knowledge of the world without. I admit, however, that the ultimate good fortune of the Greek geometers can hardly be expected by all the abstract speculations which, in the form of mathematical memoirs, crowd the Transactions of the learned societies; and I would venture to add that, on the part of the mathematician there is room for the exercise of good sense, and, I would almost say, of a kind of tact, in the selection of those branches of mathematical inquiry which are likely to be conducive to the advancement of his own or any other science:

I pass to my second example, of which I may treat very briefly. In the course of the present year a treatise on electricity has been published by Prof. Maxwell, giving a complete account of the mathematical theory of that science, as we owe it to the labours of a long series of distinguished men, beginning with Coulomb and ending with contemporaries of our own, including Prof. Maxwell himself. No mathematician can turn over the pages of these volumes without very speedily convincing himself that they contain the first outlines (and something more than the first outlines) of a theory which has already added largely to the methods and resources of pure mathematics, and which may one day render to that abstract science services no less than those which it owes to astronomy. For electricity now, like astronomy of old, has placed before the mathematician an entirely new set of questions, requiring the creation of entirely new methods for their solution, while the great practical importance of telegraphy has enabled the methods of electrical measurement to be rapidly perfected to an extent which renders their accuracy comparable to that of astronomical observations, and thus makes it possible to bring the most abstract deductions of theory at every moment to the test of fact. It must be considered fortunate for the mathematicians that such a vast field of research in the application of mathematics to physical inquiries should be thrown open to them, at the very time when the scientific interest in the old mathematical astronomy has for the moment flagged, and when the very name of physical astronomy, so long appropriated to the mathematical development of the theory of gravitation, appears likely to be handed over to that wonderful series of discoveries which have already taught us so much concerning the physical constitution of the heavenly bodies themselves

Having now stated, from the point of view of a mathematician, the reasons which appear to me to justify the existence of so composite an institution as Section A, and the advantages which that very compositeness sometimes brings to those who attend its meetings, I wish to refer very briefly to certain definite services which this section has rendered and may yet render to Science. The improvement and extension of scientific education is to many of us one of the most urgent questions of the day; and the British Association has already exerted itself more than once to press the question on the public attention. Perhaps the time has arrived when some further efforts of the same kind may be desirable. Without a rightly organised scientific education we cannot hope to maintain our supply of scientific men; since the increasing complexity and difficulty of science renders it more and more difficult for untaught men, by mere power of genius, to force their way to the front. Every improvement, therefore, which tends to render scientific knowledge more accessible to the learner, is a real step towards the advancement of science, because it tends to increase the number of well qualified workers in science.

For some years past this section has appointed a committee to aid in the improvement of geometrical teaching in this country. The report of this committee will be laid before the section in due course; and without anticipating any discussion that may arise on that report, I think I may say that it will show that we have advanced at least one step in the direction of an important and long-needed reform. The action of this section led to the formation of an Association for the improvement of geometrical teaching, and the members of that Association have now completed the first part of their work. They seem to me, and to other judges much more competent than myself, to have been guided by a sound judgment in the execution of their difficult task, and to have held, not unsuccessfully, a middle course between the views of the conservatives who would uphold the absolute monarchy of Euclid, or, more properly, of Euclid as edited by Simeon, and the radicals who would dethrone him altogether. One thing at least they have not forgotten, that geometry is nothing if it be not rigorous, and that the whole educational value of the study is lost, if strictness of demonstration be trifled with. The methods of Euclid are, by almost universal consent, unexceptionable in point of rigour. Of this perfect rigorousness his doctrine of parallels, and his doctrine of proportion, are perhaps the most striking examples. That Euclid's treatment of the doctrine of parallels is an example of perfect rigorousness, is an assertion which sounds almost paradoxical, but which I, nevertheless, believe to be true. Euclid has based his theory on an axiom (in the Greek text it is one of the postulates, but the difference for our purpose is immaterial) which, it may be safely said, no unprejudiced mind has ever accepted as self-evident. And this unaxiomatic axiom Euclid has chosen to state, without wrapping it up or disguising it,—not, for example, in the plausible form in which it has been stated by Playfair, but in its crudest shape, as if to warn his reader that a great assumption was being made. This perfect honesty of logic, this refusal to varnish over a weak point, has had its reward; for it is one of the triumphs of modern geometry to have shown that the eleventh axiom is so far from being an axiom, in the sense which we usually attach to the word, that we cannot at this moment be sure whether it is absolutely and rigorously true, or whether it is only a very close approximation to the truth. Two of those whose labours have thrown much light on this difficult theory are at present at this meeting—Prof. Cayley, and a distinguished German mathematician, Dr. Felix Klein; and I am sure of their adherence when I say that the sagacity and insight of the old geometer are only put in a clearer light, by the success which has attended the attempt to construct a system of geometry, consistent with itself, and not contradicted by experience, upon the assumption of the falsehood of Euclid's eleventh axiom.

Again, the doctrine of proportion, as laid down in the fifth book of Euclid, is, probably, still unsurpassed as a masterpiece of exact reasoning; although the cumbrousness of the forms of expression which were adopted in the old geometry has led to the total exclusion of this part of the elements from the ordinary course of geometrical education. A zealous defender of Euclid might add with truth that the gap thus created in the elementary teaching of mathematics has never been adequately supplied.

But after all has been said that can be said in praise of Euclid, the fact remains that the form in which the work is composed renders it unsuitable for the earlier stages of education. Euclid wrote for men; whereas his work has been used for children, and it is surely no disparagement to the great geometer to suppose that after more than 2,000 years the experience of generations of teachers can suggest changes which may make his Elements, I will not say more perfect as a piece of geometry, but more easy for very young minds to follow. The difficulty of a book or subject is indeed not in itself a fatal objection to its use in education, for to learn how to overcome difficulties is one great part of education: Geometry is hard, just as Greek is hard, and one reason why Geometry and Greek are such excellent educational subjects is precisely that they are hard. But in a world in which there is so much to learn, we must learn everything in the easiest way in which it can be learnt; and after we have smoothed the way to the utmost of our power, there is sure to be enough of difficulty left. I regard the question of some reform in the teaching of elementary geometry as so completely settled by a great concurrence of opinion on the part of the most competent judges, that I should hardly have thought it necessary to direct the attention of the section to it, if it were not for the following reasons:—

First, that the old system of geometrical instruction still remains (with but few exceptions) paramount in our schools, colleges, and universities, and must remain so until a very great consensus of opinion is obtained in favour of some one definite text-book. It appears to me, therefore, that the duty will eventually devolve upon this section of the British Association, of reporting on the attempts that have been made to frame an improved system of geometrical education; and if it should be found that these attempts have been at last successful, I think that the British Association should lend the whole weight of its authority to the proposed change. I am far from suggesting that any such decision should be made immediately. The work undertaken by the Association for the improvement of geometrical teaching is still far from complete; and even when it is complete it must be left to hold its own against the criticism of all comers before it can acquire such an amount of public confidence as would justify us in recommending its adoption by the great teaching and examining bodies of the country.

Secondly, I have thought it right to remind the section of the part it has taken with reference to the reform of geometrical teaching, because it appears to me that a task, at once of less difficulty and of more immediate importance, might now be undertaken by it with great advantage. There is at the present moment a very general agreement that a certain amount of natural science ought to be introduced into school education; and many schools of the country have already made most laudable efforts in this direction. As far as I can judge, there is further a general agreement that a good school course of natural science ought to include some part or parts of physics, of chemistry, and of biology; but I think it will be found that while the courses of chemistry given at our best schools are in the main identical, there is great diversity of opinion as to the parts of physics and of biology which should be selected as suitable for a school education, and a still greater diversity of opinion as to the methods which should be pursued in teaching them. Under these circumstances it is not surprising to find that the masters of those schools into which natural science has hardly yet found its way (and some of the largest and most important schools in the country are in this class), are doubtful as to the course which they should take; and from not knowing precisely what they should do, have not as yet made up their minds to do anything of importance. There can be no doubt that the masters of such schools would be glad on these points to be guided by the opinion of scientific men; and I cannot help thinking that this opinion would be more unanimous than is commonly supposed, and further, that no public body would be so likely to elicit an expression of it, as a Committee appointed by the British Association. I believe that if such an expression of the opinion of scientific men were once obtained, it would not only tend to give a right direction to the study of natural science in schools, but might also have the effect of inducing the public generally to take a higher and more truthful view of the objects which it is sought to attain by introducing natural science as an essential element into all courses of education. All knowledge of natural science that is imparted to a boy, is, or may be, useful to him in the business of his after life; but the claim of natural science to a place in education cannot be rested upon its practical usefulness only. The great object of education is to expand and to train the mental faculties, and it is because we believe that the study of natural science is eminently fitted to further these two objects, that we urge its introduction into school studies. Science expands the minds of the young, because it puts before them great and ennobling objects of contemplation; many of its truths are such as a child can understand, and yet such that, while in a measure he understands them, he is made to feel something of the greatness, something of the sublime regularity, and of the impenetrable mystery, of the world in which he is placed. But science also trains the growing faculties, for science proposes to itself truth as its only object, and it presents the most varied, and at the same time the most splendid examples, of the different mental processes which lead to the attainment of truth, and which make up what we call reasoning. In science, error is always possible, often close at hand; and the constant necessity for being on our guard against it is one important part of the education which science supplies. But in science, sophistry is impossible; science knows no love of paradox; science has no skill to make the worse appear the better reason; science visits with a not long deferred exposure all our fondness for preconceived opinions, all our partiality for views that we have ourselves maintained, and thus teaches the two best lessons that can well be taught—on the one hand the love of truth, and on the other, sobriety and watchfulness in the use of the understanding.

In accordance with these views I am disposed to insist very strongly on the importance of assigning to physics, that is to say to those subjects which we discuss in this section, a very prominent place in education. From the great sciences of observation, such as botany, or zoology, or geology, the young student learns to observe, or more simply, to use his eyes; he gets that education of the senses which is after all so important, and which a purely grammatical and literary education so wholly fails to give. From chemistry he learns, above all other things, the art of experimenting, and of experimenting for himself. But from physics, better as it seems to me than from any other part of science, he may learn to reason with consecutiveness and precision, from the data supplied by the immediate observation of natural phenomena. I hope we shall see the time when each successive portion of mathematical knowledge acquired by the pupil will be made immediately available for his instruction in physics; and when everything that he learns in the physical laboratory will be made the subject of mathematical reasoning and calculation. In some few schools I believe that this is already the case, and I think we may hope well for the future, both of mathematics and physics in this country, when the practice becomes universal. In one respect the time is favourable for such a revolution in the mode of teaching physical science. During the past few years a number of text-books have been made available to the learner, which far surpass anything that was at the disposal of former generations of pupils, and which are probably as completely satisfactory as the present state of science will admit. It is pleasant to record that these text-books are the work of distinguished men who have always taken a prominent part in the proceedings of this section. We have Deschanel's Physics, edited, or rather re-written, by Prof. Everett, a book remarkable alike for the clearness of its explanations and for the beauty of the engravings with which it is illustrated; and passing to works intended for students somewhat further advanced, we have the treatises of Prof. Balfour Stewart on Heat, of Prof. Clerk Maxwell on the Theory of Heat, of Prof Fleeming Jenkin on Electricity, and we expect a similar treatise on Light from another of our most distinguished members.

These works breathe the very spirit of the method which should guide both research and education in physics. They express the most profound and far-reaching generalisations of science in the simplest language, and yet with the utmost precision With the most sparing use of mathematical technicalities, they are a perfect storehouse of mathematical ideas and mathematical reasonings. An old French geometer used to say that a mathematical theory was never to be considered complete till you had made it so clear that you could explain it to the first man you met in the street. This is of course a brilliant exaggeration, but it is no exaggeration to say that the eminent writers to whom I have referred have given something of this clearness and completeness to such abstract mathematical theories as those of the electrical potential, the action of capillary forces, and the definition of absolute temperature. A great object will have been attained when an education in physical science on the basis laid down in these treatises has become generally accepted in our schools.

I do not wish to close this address without adverting, though only for one moment, to a question which occupies the minds of many of the friends of science at the present time, the question what should be the functions of the State in supporting, or in organising, scientific inquiry. I do not mean to touch on any of the difficulties which attend this question, or to express any opinion as to the controversies to which it has given rise. But I do not think it can be out of place for the President of this section to call your attention to the inequality with which, as between different branches of science, the aid of Government is afforded. National observatories for astronomical purposes are maintained by this, as by every civilised country. Large sums of money are yearly expended, and most rightly expended, by the Government for the maintenance of museums, and collections of mineralogy, botany, and zoology; at a very recent period an extensive chemical laboratory with abundant appliances for research as well as for instruction has been opened at South Kensington. But for the physical sciences—such sciences as those of heat, light, and electricity—nothing has been done; and I confess I do not think that any new principle would be introduced, or any great burden incurred, capable of causing alarm to the most sensitive Chancellor of the Exchequer, if it should be determined to establish, at the national cost, institutions for the prosecution of these branches of knowledge, so vitally important to the progress of science as a whole. Perhaps also, upon this general ground of fairness, even the pure mathematicians might prefer a modest claim to be assisted in the calculation and printing of a certain number of Tables, of which even the physical applications of their science are beginning to feel the pressing need.

One word further on this subject of State assistance to Science, and I have done. It is no doubt true that for a great, perhaps an increasing, number of purposes, Science requires the assistance of the State, but is it not nearer to truth to say that the State requires the assistance of Science? It is my conviction that if the true relations between Science and the State are not recognised, it is the State, rather than Science, that will be the great loser. Without Science the State may build a ship that cannot swim, and may waste a million or two on experiments, the futile result of which Science could have foreseen. But without the State, Science has done very well in the past, and may do very well in time to come. I am not sure that we should know more of pure mathematics, or of heat, of light, or electricity than we do at this moment if we had had the best help of the State all the time. There are, however, certain things which the State might do and ought to do for Science. It, or corporations created by it, ought to undertake the responsibility of carrying on those great systems of observation which, having a secular character, cannot be completed within the life-time of a single generation, and cannot therefore be safely left to individual energy. One other thing the State ought to do for Science. It ought to pay scientific men properly for the services which they render directly to the State, instead of relying, as at present, on their love for their work as a means of obtaining their services on lower terms. If anyone doubts the justice of this remark, I would ask him to compare the salaries of the officers in the British Museum with those which are paid in other departments of the Civil Service.

But what the State cannot do for Science is to create the scientific spirit, or to control it. The spirit of scientific discovery is essentially voluntary; voluntary, and even mutinous, it will remain: it will refuse to be bound with red tape, or ridden by officials, whether well-meaning or perverse. You cannot have an Established Church in Science, and, if you had, I am afraid there are many scientific men who would turn scientific nonconformists.

I venture upon these remarks because I cannot help feeling that the great desire which is now manifesting itself on the part of some scientific men to obtain for Science the powerful aid of the State may perhaps lead some of us to forget that it is self-reliance and self-help which have made Science what it is, and that these are qualities the place of which no Government help can ever supply.

Report of the Committee appointed to consider the possibility of improving the methods of instruction in Elementary Geometry.

Until recently the instruction in elementary geometry given in this country was exclusively based upon Simson's modification of the text of Euclid. Of late years, however, attempts have been made to introduce other text-books agreeing with the ancient *Elements* in general plan, but differing from it in some important details of treatment. And in particular, the Association for the Improvement of Geometrical Teaching, having considered the whole question with great labour and deliberation, is engaged in the construction of a Syllabus, part of which is already completed. The Committee had thus to consider, *first*, the question of the plurality of text-books; *secondly*, certain general principles on which deviation from the ancient standard has been recommended; and, *thirdly*, the Syllabus of the Geometrical Association.

1. *On the Plurality of Text-Books.*

It has already been found that the practical difficulty of examination stands in the way of allowing to the geometrical teacher complete freedom in the methods of demonstration, and in the order of the propositions. The difficulty of demonstrating a proposition depends upon the number of assumptions which it is allowable to start from; and this depends upon the order in which the subject has been presented. When different text-books have been used, it thus becomes virtually impossible to set the same paper to all the candidates. And in this country at present teaching is guided so largely by the requirements of examinations, that this circumstance opposes a serious barrier to individual attempts at improvement. On the other hand, the Committee think that no single text-book which has yet been produced is fit to succeed Euclid in the position of authority; and it does not seem probable that a good book could be written by the joint action of selected individuals. It therefore seems advisable that the requisite uniformity, and no more, should be obtained by the publication of an authorised Syllabus, indicating the order of the propositions, and in some cases the general character of the demonstrations, but leaving the choice of the text-book perfectly free to the teacher. And the Committee believe that the authorisation of such a Syllabus might properly come from the British Association.

2. *On some Principles of Improvement.*

The Committee recommend that the teaching of Practical Geometry should precede that of Theoretical Geometry, in order that the mind of the learner may first be familiarised with the facts of the science, and afterwards led to see their connection. With this end the instruction in practical geometry should be directed as much to the verification of theorems as to the solution of problems.

It has been proposed to introduce what are called redundant axioms, that is to say, assumptions whose truth is apparently obvious, but which are not independent of one another. Such, for example, as the two assumptions that two straight lines cannot enclose a space, and that a straight line is the shortest distance between any two of its points. It appears to the Committee that it is not advisable to introduce redundant axioms; but that all the assumptions made should be necessary for demonstration of the propositions, and independent of one another.

It appears that the Principle of Superposition might advantageously be employed with greater frequency in the demonstrations, and that an explicit recognition of it as an axiom of fundamental assumption should be made at the commencement,

The Committee think also that it would be advisable to introduce explicitly certain definitions and principles of general logic, in order that the processes of simple conversion may not be confounded with geometrical methods.

3. *The Syllabus of the Geometrical Association.*

The Association for the Improvement of Geometrical Teaching has issued (privately) a Syllabus covering the ground of the first four books of Euclid. The Committee are of opinion that the Syllabus is decidedly good, so far as it goes, but they do not wish to make a detailed report upon it in its present incomplete state. When it is finished, however, they will be prepared to report fully upon the merit of its several parts, to make such suggestions for revision as may appear necessary, and to discuss the advisability of giving to it the authority of the British Association. For this purpose the Committee request that they may be reappointed.

SECTION B.—CHEMISTRY

A report on *Essential Oils*, prepared by Dr. Wright and Dr. Gladstone, was read by the former.

On Black Deposits of Metals, by Dr. Gladstone, F.R.S.

If one metal be thrown down from solution by means of another metal, it does not always present itself of the same colour as it exhibits when in mass; in fact, most metals that are capable of being precipitated by substitution may be obtained in a black condition. The allied metals, platinum, palladium, and iridium, are generally if not always black when thus precipitated, and bismuth and antimony form black fringes and little else. Similar fringes are also formed by gold, but it also yields green, yellow, or lilac metal according to circumstances. Copper, when first precipitated on zinc, whether from a weak or a strong solution, is black; but in the latter case it becomes chocolate-coloured as it advances, or red if the action be more rapid. Lead, in like manner, is always deposited black in the first instance, though the growing crystals soon become of the well-known dull grey. Silver and thallium appear as little bushes of black metal on the decomposing plate, if the solution be very weak; otherwise they grow of their proper colour. Zinc and cadmium give a black coating, quickly passing into grey when their weak solutions are decomposed by magnesium. The general result may be stated thus: If a piece of metal be immersed in the solution of another metal which it can displace, the latter metal immediately makes its appearance at myriads of points in a condition that does not reflect light; but as the most favourably circumstanced crystals grow, they acquire the optical properties of the massive metal, the period at which the change takes place depending partly on the nature of the metal and partly on the rapidity of its growth. In the production of the black deposit of the copper-zinc couple lately employed by the author and Mr. Tribe to break up various compound bodies, there are several stages that may be noticed. At first an outgrowth of copper forms on the zinc; then, while this action is still proceeding, the couple itself acts upon the water or the sulphate of zinc in solution, the metallic zinc being oxidised, and hydrogen gas or black zinc being formed against the copper branches. This deposit of zinc was originally observed by Dr. Russell. The arrangement of the particles between the two metals in connection is supposed to be somewhat thus:—

$$Cu \mid Zn\,SO_4 \mid Zn\,SO_4 \mid H_2O \mid H_2O \mid Zn$$

which, by the conjoint power and chemical force, becomes—

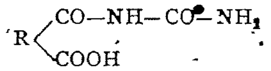
$$Cu \mid Zn \mid Zn\,SO_4 \mid H_2\,SO_4 \mid H_2O \mid Zn$$

If there is still copper sulphate in the solution, this deposited zinc may in its turn become coated with copper, but if it remains exposed to water it is sure to become oxidised. The black deposit often assumes a brownish colour when this is the case. The copper on which zinc has been deposited gives a brassy streak when rubbed in a mortar; but the presence of oxide tends to prevent the sticking together of the detached pieces of metal, and thus the formation of a streak on pressure. If, however, the oxide be removed by acetic acid, the clean ramifications of metal, whether black or otherwise, conglomerate of their own accord in a remarkable way, and little pressure is required to obtain a yellowish metallic streak; while if hydrochloric acid be used, the zinc itself also dissolves with effervescence, and the conglomerating pieces of metal, when rubbed, give a coppery streak.

The Secretary read a paper communicated by Mr. Tribe, *On an Improved Specific Gravity Bottle.* The apparatus was originally designed for taking the specific gravity of inflammable liquids, but, as the President explained, it might be used for any other class of liquids.

Mr. W. H. Pike read a paper on *Several Homologues of Oxaluric Acid.*

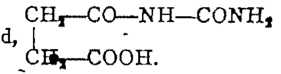
The anhydrides of dibasic acids combine with urea and sulphourea to form bodies which have the general formula



$$R\left<\begin{matrix}\text{CO—NH—CO—NH}_2\\ \text{COOH}\end{matrix}\right.$$

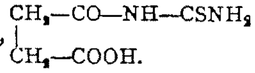
. The acids in this series which have been obtained are—

Succino-carbaminic acid,



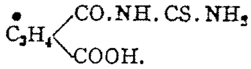
$$\begin{matrix}\text{CH}_2\text{—CO—NH—CONH}_2\\ |\\ \text{CH}_2\text{—COOH.}\end{matrix}$$

Succino-sulpho-carbaminic acid,



$$\begin{matrix}\text{CH}_2\text{—CO—NH—CSNH}_2\\ |\\ \text{CH}_2\text{—COOH.}\end{matrix}$$

Citracon sulpho-carbaminic acid,



$$C_3H_4\left<\begin{matrix}\text{CO.NH.CS.NH}_2\\ \text{COOH.}\end{matrix}\right.$$

Dr. Wright read a paper on *New Derivatives of Codeine and Morphine.*

It was a *résumé* of the results obtained in the previous year in continuation of those brought before the Association on former occasions. Morphine gave rise by treatment with sulphuric acid to polymerides precisely analogous to those obtained from codeine under similar conditions. Trimorphine and tetramorphine had been isolated, but di-morphine had not yet been formed. Derivatives from these bodies by the action of hydrochloric acid had been obtained and extended. By the action of hydrochloric acid on morphine a chlorinated product had been formed. By further treatment this formed apomorphine, a new body. Under the same circumstances codeine gave rise to a chlorinated base homologous with that from morphine. But further action gave rise not to the apomorphine, but to a somewhat similar body containing more of the elements of water. The action of zinc chlorides on morphine had also been examined; the final products were apomorphine and an isomeric base of the tetra series, intermediate substances being formed. The physiological properties of most of these new derivatives had been stated, and some connection made out in certain cases between the composition and the physiological action.

Friday, September 19

The report of the Committee for superintending the Monthly Reports of the Progress of Chemistry was read. The report bore testimony to the great good which the publication of the abstracts of chemical papers by the Chemical Society had already effected, and in the discussion which ensued it was stated that amongst the purposes to which the Association applied its funds, there was none which had proved more useful than this grant.

The report of the Committee on Siemens's Pyrometer was read by Prof. G. C. Foster, F.R.S.

The experiment of which the results were communicated to the Chemical Section of the Association in the Report presented last year, having shown that the exposure of the Pyrometer to a red heat caused an alteration of the Zero-point of the instrument, which was attributed by Prof. Williamson, in consequence of experiments on the behaviour of platinum heated in contact with silica in an atmosphere of carbonic oxide, to the chemical alteration of the platinum of the pyrometer-coil due to the joint action of the silica of the porcelain core on which the wire was wound, and of the reducing atmosphere existing inside the protecting iron tube. Mr. Siemens supplied the Committee with two pyrometers, in which, in order to guard against the cause of change above-mentioned, the platinum coil was incased in a platinum tube placed inside the outer iron tube. The experiments of the Committee during the past year have been directed to testing the efficacy of this modification of the instrument. Owing to circumstances, these experiments have not been as numerous or complete as they were intended to be, but, as far as they go, they indicate that the addition of the platinum tube does not result in any perceptible improvement, since the two pyrometers supplied to the Committee were found to be as much changed, after being heated to a good red heat, as the instrument experimented upon last year.

Independent testimony, however, of considerable weight as to the value of Siemens's pyrometer, as an instrument for industrial use, has been borne by Prof. Adolf Weinhold, of Chemnitz (*Programm des konigl. höheren Gewerbschule zu Chemnitz*, 1873), who after a careful, critical, and experimental review of various processes of pyrometry, arrives at the conclusion that this is the only ready-made pyrometer which can be recommended for use ("Von den fertig zu beziehenden Pyrometern ist nur das Siemens'sche brauchbar und empfehlenswerth," *loc. cit.* p. 42).

The Committee, therefore, consider that the further examination of Siemens's Pyrometer is a matter of sufficient importance to justify them in the recommendation that the Committee be re-appointed, and that the original grant of 30*l.*—no part of which has yet been expended—be renewed.

SECTION D.—Biology

DEPARTMENT OF ZOOLOGY AND BOTANY

Report of the Committee for the Foundation of Stations in different parts of the Globe.

The Committee reports that since the last meeting the Zoological Station at Naples has been completed, a photograph of which accompanies this report.

Both the mechanical and scientific arrangements inside require perhaps two more months to be finished, and though the cost of the whole has exceeded in no small degree the estimates, Dr Dohrn hopes nevertheless to balance them by finding new means of income for the establishment. He has succeeded in obtaining a subvention of 1,500*l.* from the German Empire, and his scheme of letting working-tables in the laboratories of the station has met with general approval. Two tables have been let to Prussia and to Italy, one to Bavaria, Baden, and the Universities of Strasburg and Cambridge. A letter from the Dutch Minister of the Interior informs Dr. Dohrn that Holland accepts the offer of one table for the stipulated annual payment of 75*l.* Applications have also been made to the Imperial Government of Russia, both on the part of Dr. Dohrn and by different Russian scientific authorities. A correspondence has taken place between Dr. Dohrn and Professors Lovén and Steenstrup about a possible participation of the Scandinavian kingdoms, but has as yet led to no definite result. The case with respect to Switzerland and Saxony has been similar, but hopes are entertained that these countries may join the others in their endeavour to support the Zoological Station, and afford every facility to their naturalists of profiting by this new and powerful instrument of investigation.

Dr. Dohrn thinks it desirable to explain once more the leading ideas that have induced him to request the assistance of all these Governments and Universities.

The Zoological Station has sprung up altogether in consequence of the desire to facilitate investigation in marine zoology, and to enable naturalists to pursue their studies in the most effective manner and with the greatest possible economy of money and energy. All those zoologists that have visited Naples during the last year—amongst whom have been Professors Gegenbaur, Claus, Oscar Schmidt, Pagenstacher—consider that this end will be fully attained by the organisation and arrangements made or intended in the station. They all agreed that it is in the highest degree desirable that nobody who cares at all for the progress of zoology should fail to join Dr. Dohrn's exertions in bringing about a universal participation in the expense of keeping up the new establishment; and thus it is due to Prof. Oscar Schmidt's influence that the Imperial Government at Berlin hired a table for the University of Strasburg, and to the initiation of Prof. Pagenstacher that the Grand Duchy of Baden has also taken one table, whilst Prof. Claus has promised his services to induce the Austrian Government to take a similar step.

As is, we believe, universally known, no money-speculation whatever is contemplated by the founder of the Naples Station, in so far as money-speculation means a high interest and the return of the capital invested into the pocket of the founder. Nevertheless every honest means will be used to procure as large an income as possible, for more than one reason. There is not only the necessity incumbent upon the establishment to repay some of the capital to those who have lent money to Dr. Dohrn in order that he might complete the building in its actual enlarged state, a task for which his own means would not have sufficed, in spite of the German Government's subvention. There is further reserve funds to be provided for the eventuality that the income of the aquarium might at any time not cover the outlay for the year's management. And last, not least, it is just the plan to have every year a certain sum to spend for scientific pursuits. If, for instance, Prof. Dubois-Reymond, as he has expressed to Dr. Dohrn his wish to do so, should proceed to Naples to carry on experiments on the electric torpedo, it needs would require not inconsiderable means to buy the necessary apparatus and physiological instruments, and to provide the famous physiologist every day with fresh materials to conduct his investigations on a scale large enough to yield a distinct result. Or to enable embryologists to carry on an investigation on comparative selection-embryology, it requires means to buy large quantities of female sharks and skates, which are by no means so cheap as a foreigner might think. And for conducting well and accurately faunistic researches, everybody in this section knows what an amount of money must be spent in dredging-expeditions; how much trouble, how much time and work is necessary to get at the animals and to determine their identity or non-identity with the known and described species. And this is one of the foremost duties which the Zoological Station will propose to itself, as it is too well known how great a confusion exists with regard to systematic and faunistic questions of the Mediterranean fauna. To bring this confusion to an end it will require more than one lustrum and more than 1,000*l.* There may perhaps have risen a prejudice among systematists against the new establishment as one which, in consequence of the partiality of its leader for Darwinian views, might dispense altogether with Systematics. Nothing could be more erroneous than such an opinion. The leader of the zoological station is as little opposed to systematics as the Darwinian theory itself. He is of opinion—and the reporter can state this on the most absolute authority—that zoological battles may be best won according to Count Moltke's principle, "to march separately and to fight conjunctively," thus leaving to systematists their own route as well as to anatomists, physiologists, and embryologists, on condition only that they will, when meeting the enemy—error and ignorance—fight together. And he desires the zoological station to become such a battle-field, where all the different zoological armies may meet and fight their common adversaries.

That such wars need much of the one element, which, according to Montecuculi, best secures victory—money, money, money, will be illustrated by two letters which Dr. Dohrn has received from Prof. Louis Agassiz, and which he has been authorised to publish.

The celebrated American naturalist writes, under the date "Museum of Comparative Zoology, Cambridge, Mass., June 10, 1873," the following:—

"It is a great pleasure and satisfaction to me, that I can tell you how, in consequence of the munificence of a wealthy New York merchant, it has become my duty to erect an establishment whose main object will be similar to that of your Naples station, only that teaching is to be united with it. The thing came thus to pass. During last winter I applied to our state authorities to secure more means for the museum in Cambridge (Mass.) Among the reasons, I alluded to the necessity of having greater means for trading purposes. I addressed my speech to our deputies, and it was afterwards reported in the newspapers. By chance the report fell into the hands of a rich and magnanimous tobacco-manufacturer, Mr. John Anderson, of New York. He sent, on the same day, a telegram asking me whether I would be at home on the following day for two friends, which I answered by 'yes.' The two gentlemen came, by order of Mr. Anderson, offering me a pretty little island in Buzzard Bay, for the purpose of erecting a zoological school. I accepted this offer, of course, but added, that without further pecuniary means it would be difficult to teach there. After two days, a sum of 50,000 dollars was handed over to me, and now I am erecting there a school for natural history, which at the same time will be a zoological station in the immediate neighbourhood of the gulf-stream, of the greatest assistance to our zoologists, especially as splendid dredging ground. This certainly must greatly promote zoological study in the United States. Already forty teachers of our Normal and high schools have applied for this summer's lessons; besides, I will be accompanied thereto by my private students. Some of my special colleagues are ready to assist me, so that I may hope to obtain already some results before winter's approach."

The next letter is dated "Penikese, Aug. 13, 1873," and contains some more information:—

"The school has been opened on July 8. Some of my friends have assisted me as teachers, several other naturalists are occupied with special studies. The bottom of the sea is very rich, the general situation quite excellent. The solitude which prevails is a great help for our teaching purposes. As students, forty teachers of our public schools are present, besides ten younger gentlemen, who prepare for a scientific career.

"The buildings are very well constructed and adapted to their uses. The two chief houses have a length of 120 feet, and a breadth of 25 feet each. In the lower story are the laboratories each with 28 windows; every student occupies one window, and has for himself one aquarium. In the upper story of each house are 28 bed-rooms, for every student one. The professors and naturalists are lodged in another house of the shape of a Greek cross. The dining-room is in a third house, which contains also the kitchen and the servants' rooms. Besides we have an ice-house, a cellar for alcohol, stables for domestic animals; about one hundred sheep are feeding in the pasture grounds of the island; some smaller hutches contain rabbits, guinea-pigs, &c.

"Next year physical, chemical, and physiological, laboratories will be constructed. . . .

"I believe I did not tell you before, that my son presented me on my birthday with 100,000 dollars for the enlargement of the Museum. I intend to apply this sum chiefly to the augmentation of the collections, hoping the State will pay for the enlargement of the buildings. . . ."

These letters prove that the name of this committee has not been ill-chosen, for though the American Zoological Station has not been founded by its direct intervention, there can be little doubt that the foundation of the Zoological Station of Naples has been the signal for a new and powerful movement to assist zoological research.

Of course the American Station has met with such extraordinary advantages, that a competition between it and Naples Station as regards means and favourable circumstances would be all but hopeless for the latter. Nevertheless it may prove the most powerful instrument in carrying out strictly the self-supporting principle, by earning money through the Aquarium, and by letting tables in the laboratory. And though any act of munificence to the Naples Station is exceedingly desirable, and would be heartily welcomed (as the moment has not yet arrived, where any scientific establishment in this world had at its disposal more money than it knew how to spend) the greatest stress will always be laid upon these two elements.

The reporter is further glad to state that the library of the Zoological Station has recently been augmented. A magnificent gift has been made by the Zoological Society of London, which presented a complete set of its illustrated proceedings. The Royal Academies of Copenhagen, Naples, and Berlin, have also granted their biological publications, and promised to continue to do so in future. The Jenckenberg Institute in Frankfort-on-the-Main, as well as the Zoological Gardens of that city, have sent all their transactions; as has the Smithsonian Institution in Washington, with respect to its biological publications. Well-founded hopes are entertained that in a short time many other academies and scientific societies will follow the example of the above-mentioned.

German publishers have continued to send their biological publications gratis to the library of the station, and great quantities of books, pamphlets, and separata from publications in periodicals, have been forwarded from all parts of the scientific world through the kindness of the authors.

From the side of the Zoological Station, though still in an embryonic state, considerable activity has been displayed with regard to furnishing continental zoologists with collections of well-preserved marine animals. Thus Prof. Wilhelm Muller in Jena, has been supplied with Amphioxus and Tunicata, Prof. Greeff of Marburg with large quantities of Echinodermata; mixed collections of every kind of animals have been sent to Prof. Oscar Schmidt, Strasburg, Prof. Claus, Vienna, to the Jenckenberg Museum at Frankfort, the Natural History Society at Offenbach, and many others.

Several German zoologists have already announced their intention to come during next winter and work in the station; a similar announcement is made through an Italian zoologist and through Prof. Foster. I am informed that two young English biologists will arrive at the station in January.

The committee hopes this report will convince the section, that the year between the present and the last meeting of the British Association has been one of steady and considerable progress for the Zoological Station at Naples. The committee refrains from making any further proposition to the section, but expresses its wish, that every influence may be used to secure to the station at Naples such assistance, as will serve to promote the eminent scientific ends for which it has been erected.

DEPARTMENT OF ANATOMY AND PHYSIOLOGY

Opening Address by the President, Professor Rutherford

In addressing you upon the subjects of anatomy and physiology, I would invite your attention to some of the features which characterise these departments of biology at this present time, and to some recent advances in physiology, the consideration of which you will find to be possessed of deep interest and importance.

State of Anatomy

Anatomy, dealing as it does merely with the structure of living things, is a far simpler subject than physiology, whose province it is to ascertain and explain their actions. It was not a difficult thing to handle such instruments as a knife and forceps, and with their aid to ascertain the coarser structure of the body. Accordingly, the naked eye anatomy of man has been fully investigated, and although the same cannot be said of that of many of the lower animals, it is nevertheless, as far as this kind of inquiry is concerned, a mere question of time as regards its completion. But minute or microscopic anatomy is in a different position. Requiring, as it does, the microscope for its pursuit, it could not make satisfactory progress until this instrument had been brought to some degree of perfection. Doubtless much advantage is still to be derived from improvements in the construction of this instrument; but probably most of the future advances in our knowledge of the structure of the tissues and organs of the body may be expected to result from the application of new methods of preparing the tissues for examination with such microscopes as we now have at our disposal. This expectation naturally arises from what has been accomplished in this direction during the last fifteen years. For example, what valuable information has been gained regarding the structure of such soft tissues as the brain and spinal cord by hardening them with such an agent as chromic acid, in order that these tissues may be cut into thin slices for microscopical study. How greatly has the employment of such pigments as carmine and the aniline dyes facilitated the microscopical recognition of certain elements of the tissues. What a deal we have learned regarding the structure of the capillaries, and the origin of lymphatics by the effect which nitrate of silver has of rendering distinctly visible the outlines of endothelial cells. What signal service chloride of gold has rendered in tracing the distribution of nerves by the property which it possesses of staining nerve fibrils, and thereby greatly facilitating their recognition amidst the textures. Moreover of what value osmic acid has been in enabling us to study the structure of the retina. In the hands of Lockhart Clarke, Beale, Recklinghausen, Cohnheim, Stultz, and others, these agents have furnished us with information of infinite value, and those who would advance microscopical anatomy may do so most rapidly by working in the directions indicated by these investigators. In human microscopical anatomy, indeed, there only remain for investigation things which are profoundly difficult, such as, for example, the structure of the brain, the peripheral terminations of nerves, the development of nerve tissue, and other subjects equally recondite But in the field of comparative anatomy there is far greater. scope for the histological investigator. He has only to avail himself of those reagents and methods which have recently proved so useful in the microscopical anatomy of the vertebrates; he has only to apply those more fully than has yet been done to the invertebrates, and he will scarcely fail to make discoveries. For the lover of microscopical research, there is, moreover, a wide field of inquiry in the study of comparative embryology; that is to say in the study of the development of the lower animals. Since it has become clear that a knowledge of the precise relations of living things one to another can only be arrived at by watching the changes through which they pass in the course of their development, research has been vigorously turned in this direction, and although an immense mass of facts has long since been accumulated regarding this question, Parker's brilliant researches on the development of the skull give an indication of the great things we may yet anticipate from this kind of research. Speaking of microscopical study before this audience, I cannot but remember that in this country more than in any other we have a number of learned gentlemen who, as amateurs eagerly pursue investigations in this department. I confess that I am always sorry to witness the enthusiastic perseverance with which they apply themselves to the prolonged study of markings upon diatoms, seeing that they might direct their efforts to subjects which would repay them for their labours far more gratefully. I would venture to suggest to such workers that it is now more than ever necessary to abandon all aims at haphazard discoveries, and to approach microscopy by the only legitimate method, of undergoing a thorough preliminary training in the various methods of microscopical investigation by competent teachers, of whom there are now plenty throughout the country.

State of Physiology

With regard to physiology, the present standpoint is not so high as in the case of anatomy. Physiology, resting as it does upon a tripod consisting of anatomy, physics or mechanics, and chemistry, is many-sided. The most minute anatomy, the most recondite physics, and the most complex chemistry, have all to be taken into account in the study of the physiology of living things; so that it is not surprising that it should, in its development, lag behind the comparatively elementary subject—anatomy. Until not so very long ago anatomy and physiology were in most of our medical schools taught by the same professor, who, although professing to teach both subjects, was generally more an anatomist than a physiologist. This arrangement gave to physiology a bias which was eminently anatomical, and this bias continued in many quarters, notwithstanding the separation of the physiological from the anatomical tuition. I am aware that there are still some distinguished anatomists who intermingle physiological with anatomical teaching. I am not questioning the usefulness of the practice when carried to a moderate extent. I wish merely to point out what appears to me to have been a result of the practice, and I believe that the result was to give to physiology an anatomical tendency. It was natural for the anatomist who dealt with visible structure to constantly refer to this in explaining physiological action or function. The physiologist with the anatomical tendency always tried to explain a difference in the action or function of a part by a difference in its evident structure, and when his microscope failed to show any structural difference between the cells which form saliva and those which produce pancreatic fluid, between the egg of a rabbit and that of a dog, he, baffled on the side of anatomy, was too ready to adopt the conclusion that inasmuch as the microscope reveals no difference in the structure there is really no structural difference between them, and that the only way in which the difference in action can be explained is by having recourse to the old hypothesis that the metamorphoses of matter, and the actions of force are in the living world regulated by a metaphysical entity termed a vital principle, and that dissimilar actions by similarly constructed parts are only to be explained by referring them to the operations of this principle. After alluding further to the hypothesis of the vital principle and its supposed actions, and after stating that he did not follow the teaching of those who still adhere to this doctrine, the lecturer said that, viewed from the physical side, there appears to be no reason for supposing that two particles of protoplasm, which possess a similar microscopic structure, must act in the same way; for the physicist knows that molecular structure and action are beyond the ken of the microscopist, and that within apparently homogeneous jelly-like particles of protoplasm there may be differences of molecular constitution and arrangement which determine widely different properties.

A great change is now taking place in physiological tuition in this country—a superabundance of physiological anatomy, and an almost entire absence of experiment, are no longer the characteristic features of our tuition. The study of physics, too much neglected, is happily now being more and more regarded as important in the preliminary training of the physiologist, as the study of anatomy and of chemistry; and I trust that the day is not far distant when in our medical schools the thorough education of our students in mathematics and physics will be insisted upon as absolutely essential elements in their preliminary education. Until this is done physiology will not advance in this country so rapidly as we could wish. I would not in this place have alluded to a question concerning medical education, but for the fact that the progress of physiology will always greatly depend upon the education of medical men, for only those who are conversant with physics and chemistry, and who, in addition, are acquainted with the phenomena of disease—that is to say, with abnormal physiological conditions—can handle physiology in all its branches. Physiology owes not a little to a study of pathology—that is, of abnormal physiological states. The study of a diseased condition has, on several occasions, given a clue to the discovery of the function of an organ. Nothing was known regarding the function of the spleen until the pathologist observed that an increase in the number of white corpuscles in the blood is commonly associated with an enlargement of this organ. Hence arose the now accepted doctrine that the spleen is concerned in the growth of blood corpuscles. The key to our knowledge of the functions of certain parts of the brain has also been supplied by a study of the diseased conditions of that organ. The very singular fact that the right side of the body is governed by the left, and not by the right side of the brain, was ascertained by observing that palsy of the right side of the body is associated with certain diseased conditions of the left side of the brain. That the corpus striatum is concerned in motion, while the optic thalamus is concerned in sensation; that intellectual operations are manifested specially through the cerebral hemispheres, are conclusions which were indicated by the study of diseased conditions. Moreover, by the pursuit of the same line of inquiry the key has been given to the discovery of many other facts regarding the brain functions. Some years ago M. Broca made the remarkable observation that, when a certain portion in the front part of the left side of the brain becomes disorganised by disease, the person loses the power of expressing his thoughts by words, either spoken or written. He can comprehend what is said to him, his organs of articulate speech are not paralysed, and he retains his power of writing, for he can copy words when told to do so, but when he is asked to give expression to his thoughts by speaking or by writing, or even to tell his name, he is helpless. With a palsy of a portion of his brain, he has lost his power of finding words—he has lost his memory for words; and mark you, although he loses his power of finding words, his intelligent perception of what passes around him and of what is said to him is not lost. It is true that this condition of aphasia, as it is termed, has been found to exist when various parts of the brain have been diseased; for example, it has been found to coexist with a diseased state of the posterior instead of the anterior part of the cerebrum. This fact renders it very difficult as yet to assign a precise locality to the faculty of speech. It is not, however, my intention to discuss this question, for my object is merely to show how the study of disease has given a clue to the physiologist. Broca's observation led to the thought that, after all, the dreams of the phrenologists would be realised, in so far as they supposed that the various mental operations are made manifest through certain definite territories of the brain.

It has until lately been supposed that the *convolutions of* the cerebrum are entirely concerned in purely intellectual operations, but this idea is now at an end. It is now evident, from recent researches, that in the cerebral convolutions—that is, in the part of the brain which was believed to minister to intellectual manifestations—there are nerve-centres for the production of voluntary muscular movements in various parts of the body. It has always been taught that the convolutions of the brain, unlike nerves in general, cannot be stimulated by means of electricity. This, although true as regards the brains of pigeons, fowls, and perhaps other birds, has been shown by Fritsch and Hitzig to be untrue as regards mammals. These observers removed the upper portion of the skull in the dog, and stimulated small portions of the exposed surface of the cerebrum by means of weak galvanic currents, and they found that when they stimulated certain definite portions of the surface of the convolutions in the anterior part of the cerebrum, movements are produced in certain definite groups of muscles on the opposite side of the body. By this new method of exploring the functions of the convolutions of the brain, these investigators showed that in certain cerebral convolutions, there are centres for the nerves presiding over the muscles of the neck, the extensor and adductor muscles of the forearm, for the flexor and rotator muscles of the arm, the muscles of the foot, and those of the face. They, moreover, removed the portion of the convolution on the left side of the cerebrum, which they had ascertained to be the centre for the movements of the right forelimb, and they found that after the injury thus inflicted, the animal had only an imperfect control over the movements of the part of the limb in question. Recently, Dr. Hughlings Jackson, from the observation of various diseased conditions in which peculiar movements occur in distinct groups of muscles, has adduced evidence in support of the conclusion that in the cerebral convolutions are localised the centres for the production of various muscular movements. Within the last few months these observations have been greatly extended by the elaborate experiments of my able colleague in King's College, Prof. Ferrier.

Adopting the method of Fritsch and Hitzig—but instead of using galvanic he has employed Faradic electricity, with which, strange to say, the investigators just mentioned obtained no very definite results—he has explored the brain in the fish, frog, dog, cat, rabbit, and guinea-pig, and lately in the monkey. The results of this investigation are of great importance. He has explored the convolutions of the cerebrum far more fully than the German experimenters, and has investigated the cerebellum, corpora quadrigemina, and several other portions of the brain not touched upon by them. There is, perhaps, no part of the brain whose function has been more obscure than the cerebellum. Dr. Ferrier has discovered that this ganglion is a great centre for the movements of the muscles of the eyeballs. He has also very carefully mapped out in the dog, cat, &c., the various centres in the convolutions of the cerebrum, which are concerned in the production of movements in the muscles of the eyelids, face, mouth, tongue, ear, neck, fore and hind feet, and tail. He confirms the doctrine that the corpus striatum is concerned in motion, while the optic thalamus is probably concerned in sensation, as are also the hippocampus major and its neighbouring convolutions. He has also found that in the case of the higher brain of the monkey there is what is not found in the dog or cat—to wit, a portion in the front part of the brain, whose stimulation produces no muscular movement. What may be the function of this part, whether or not it specially ministers to intellectual operations, remains to be seen. These researches of Fritsch, Hitzig, Jackson, and Ferrier, mark the commencement of a new era in our knowledge of brain function. Of all the studies in comparative physiology there will be none more interesting, and few so important, as those in which the various centres will be mapped out in the brains throughout the vertebrate series. A new, but this time a true, system of phrenology will be founded upon them; by this, however, I do not mean that it will be possible to tell a man's faculties by the configuration of his skull, but that the various mental faculties will be assigned to definite territories of the brain, as Gall and Spurzheim long ago maintained, although their geography of the brain was erroneous.

I have alluded to this subject, not only because it affords an illustration of the service which a study of diseased conditions has rendered to physiology, but also because these investigations constitute the most important work which has been accomplished in physiology for a very considerable time past.

Revival of Physiology in England

We may, I think, term this the renaissance period of English physiology. It seems strange that the country of Harvey, John Hunter, Charles Bell, Marshall Hall, and John Reid, should not always have been in the front rank as regards physiology. The neglect of physics must be admitted as a cause of this; it is also to be attributed to the, until a few years ago, almost entire absence of experimental teaching; but it would be unjust not to attribute it in great measure to the limited appliances possessed by our physiologists. It is to be remembered that physiology could not be successfully cultivated without proper laboratories, with a supply of expensive apparatus. Without endowments from public or private resources, how can such institutions be properly fitted up and maintained by men who can, for the most part, only turn to physiological research in moments snatched from the busy toil of a profession so laborious as that of medicine. In defiance of these difficulties we are now striving to hold our place in the physiological world. A new system of physiological tuition is rapidly extending over the country. In the London schools, in Edinburgh, Cambridge, Manchester, and elsewhere, earnest efforts are being made to give a thoroughly practical aspect to the tuition of our science, and notwithstanding the imperfect results which must necessarily ensue in the absence of suitable endowment, we can nevertheless point to the fact that the effect of these efforts has been to awaken a love for physiological research in the mind of many a student, and the results of this awakening are already apparent in the archives of Royal Societies, in the "Journal of Anatomy and Physiology," and elsewhere. But physiological research is most expensive and laborious, and it is, moreover, unremunerative. The labours of the physiologist are entirely philanthropic; all his researches do nothing but contribute to the increase of human happiness by the prevention of disease, and the amelioration of suffering; and I would venture to suggest to those who are possessed of wealth and of a desire to apply it for the benefit of society, that in view of the wholly unselfish and philanthropic character of physiological labours, they could not do better than follow the admirable example set by Miss Brackenbury in endowing a physiological laboratory in connection with Owens College, in Manchester. The endowment of a dozen such laboratories throughout the country would immensely aid in the development of physiological research amongst us.

We anticipate great benefit to the community not only from an advance of physiology, but from a diffusion of a knowledge of its leading facts amongst the people. This is now being carried out in our schools on a scale which is annually increasing. Thanks to the efforts of Huxley, the principles of physiology are now presented in a singularly palatable form to the minds of the young. The instruction communicated does not consist of technical terms and numbers, but in the elucidation of the principal events which happen within our bodies, together with an explanation of the treatment which they must receive in order to be maintained in health. Considering how much may be accomplished by these bodies of ours if they be properly attended to and rightly used, it seems to be a most desirable thing that the possessor of the body should know something about its mechanism, not only because such knowledge affords him much material for suggestive thought—not only because it is excellent mental training to endeavour to understand the why and the wherefore of the bodily actions, but also because he may greatly profit from a knowledge of the conditions of health. A thorough adoption of hygienic measures—in other words, of measures which are necessary to preserve individuals in the highest state of health—cannot be hoped for until a knowledge of fundamental physiological principles finds its way into every family. This country has taken the lead in the attempt to diffuse a sound knowledge of physiological facts and principles among the people, and we may fairly anticipate that this will contribute not a little to enable her to maintain her high rank amongst nations; for every step which is calculated to improve the physiological state of the individual must inevitably contribute to make the nation successful in the general struggle for existence.

DEPARTMENT OF ANTHROPOLOGY

OPENING ADDRESS BY THE PRESIDENT, JOHN BEDDOE, F.R.S.

The position of Anthropology in the British Association, as a permanent department of the Section of Biology, being now fully assured, and its relations to the allied and contributory sciences beginning to be well understood and acknowledged, I have not thought it necessary, in opening the business of the department, to follow the examples of my predecessors, Prof. Turner and Colonel Lane Fox. The former of these gentlemen, at our Edinburgh Meeting, devoted his opening address to the definition, history, and boundaries of our science; the latter, at Brighton, in the elaborate essay which many of you must have listened to, not only discussed its relations to other sciences, but gave an illustrative survey of a great portion of its field and of several of its problems.

But while, on the one hand, I feel myself incompetent to follow these precedents with success, on the other hand I am encouraged to take a different line by the consideration that if, as we are fond of saying in this department, "the proper study of mankind is man"—if, that is, anthropology ought to interest everybody, then assuredly the anthropology of Yorkshire ought to interest a Yorkshire audience.

Large as the county is, and sharply marked off into districts by striking diversities of geological structure, of climate and of surface, there is an approach to unity in its political and ethnological history which could scarcely have been looked for.

Nevertheless we must bear in mind the threefold division of the shire—not that into ridings, but that pointed out by nature. We have, first, the western third, the region of carboniferous limestone and millstone-grit, of narrow valleys and cold rainy moorlands; secondly, the great plain of York, the region, roughly speaking, of the Trias, monotonously fertile, and having no natural defence except its numerous rivers, which indeed have sometimes served rather as a gateway to the invader than as a bulwark against him; to this plain Holderness and the Vale of Pickering may be regarded as eastern adjuncts. Thirdly, we have the elevated region of the east, in the two very dissimilar divisions of the moorlands and the wolds; these are the most important parts of Yorkshire to the prehistoric archæologist; but to the modern ethnologist they are comparatively of little interest.

The relics of the palæolithic period, so abundant in the south of England, are, I believe, almost wholly wanting in Yorkshire, where archæology begins with the neolithic age, and owes its foundations to Canon Greenwell of Durham, Mr. Mortimer of Driffield, Mr. Atkinson of Danby, and their predecessors in the exploration of the barrows of Cleveland and the Wolds, whose results figure largely in the "Crania Britannica" of Davis and Thurnam,—themselves, by the way, both natives of the city of York.

The earliest inhabitants we can distinctly recognise were the builders of certain long barrows, such as that of Scamridge in Cleveland. There is still, I believe, some difference of opinion among the anthropologists of East Yorkshire (where, by the way, in the town of Hull, the science flourishes under the auspices of a local Anthropological Society)—still, I say, some difference of opinion as to whether the long-barrow folk were racially diverse from those who succeeded them and who buried their dead in round barrows. But Canon Greenwell at least adheres to Thurnam's doctrine, and holds that Yorkshire, or part of it, was occupied at the period in question, perhaps 3,000 years ago, by a people of moderate or rather short stature, with remarkably long and narrow heads, who were ignorant of metallurgy, who buried their dead under long ovoid barrows, with sanguinary rites, and who labour under strongly-founded suspicions of cannibalism.

Of the subsequent period, generally known as the bronze age, the remains in Yorkshire, as elsewhere, are vastly more plentiful. The Wolds especially, and the Cleveland hills, abound with round barrows, in which either burnt or unburnt bodies have been interred, accompanied sometimes with weapons or ornaments of bronze, and still more often with flint arrowheads. Where bones are found, the skull presents what Barnard Davis considers the typical British form; *i.e.* it is generally rather short and broad, of considerable capacity and development, with features harsh and bony. The bodily frame is usually tall and stalwart, the stature often exceeding 6 ft., as in the well-known instance of the noble savage of Gristhorpe, whose skeleton is preserved in the Scarborough Museum.

Though certain facts, such as the known use of iron in Britain before Cæsar's time, and its extreme rarity in these barrows, and some little difference in proportion between the skulls just described and the type most common among our modern British Kelts, do certainly leave room for doubt, I have little hesitation in referring these round barrows to the Brigantes and Parisii,* the known occupants of Yorkshire before the Roman conquest.

Both what I will term provisionally the pure long-barrow and the pure round-barrow types of cranium are represented among our modern countrymen. But the former is extremely rare, while the latter is not uncommon. It is probable enough that the older type may, in amalgamating with the newer and more powerful one, have bequeathed to the Kelts of our own time the rather elongated form which prevails among them. Whether this same older type was really Iberian is a point of great interest, not yet ripe for determination.

Another moot point is the extent to which the population of modern England is derived from the colonists introduced under the Roman occupation. It is my own impression that the extent, or rather the intensity of such colonisation has been over-estimated by my friend Mr. Thomas Wright and his disciples. I take it that, in this respect, the Roman occupation of Britain was somewhere between our own occupations of India and of South Africa, or perhaps still more nearly like that of Algeria by the French, who have their roads, villas, and military establishments, and even considerable communities in some of the towns, but who constitute but a very small percentage of the population, and whose traces would almost disappear in a few generations, could the communication with the mother-country be cut off.

If, however, any traces of the blood of the lordly Romans themselves, or of that more numerous and heterogeneous mass of people whom they introduced as legionaries, auxiliaries, or colonists, are yet recognisable anywhere in this county, it may probably be in the city of York, or in the neighbourhood of Catterick. The size and splendour of ancient Eburacum, its occupation at various times as a sort of military capital by the Emperor Severus and others, its continued existence through the Anglian and Anglo-Danish periods, and its subsequent comparative freedom from such great calamities* or vicissitudes as are apt to cause great and sudden changes of population, might almost induce us to expect to find such vestiges. If Greek and Gothic blood still assert themselves in the features and figures of the people of Arles, if Spanish characteristics are still recognisable in Bruges, why not Italian ones in York? It may be so; but I must confess that I have not seen them, or have failed to recognise them. Catterick, the site of ancient Cataractonium, I have not visited.

Of the Anglian conquest of Yorkshire we know very little, except that it was accomplished gradually by successive efforts, that the little district of Elmet, in the neighbourhood of Leeds, continued British for a while, and that Carnoban, which is almost certainly Craven, is spoken of by a Welsh writer as British after all the rest of the country had ceased to be so—a statement probable enough in itself, and apparently corroborated by the survival of a larger number of Keltic words in the dialect of Craven than in the speech of other parts of Yorkshire.

Certain regulations and expressions in the Northumbrian laws, among others the less value of a churl's life as compared with that of a thane, have been thought to indicate that the proportion of the British population that remained attached to the soil, under Anglian lords, was larger in the north than in some other parts of England. The premisses are, however, insufficient to support the conclusion; and, on the other hand, we are told positively by Bede that Ethelfrith Flesawr drove out the British inhabitants of extensive districts. The singular discoveries of Boyd Dawkins and his coadjutors in the Settle Cave, where elaborate ornaments and enamels of Romano-British type are found in conjunction with indications of a squalid and miserable mode of life long endured, attest clearly the calamities of the natives about that period (the early part of the seventh century), and show that even the remote dales of Craven, the least Anglian part of Yorkshire, afforded no secure refuge to the Britons of the plains, the unfortunate heirs of Roman civilisation and Roman weakness. The evidence yielded by local names does not differ much from that of the same kind in other parts of England. It proves that enow of Welshmen survived to transmit their names of the principal natural features (as Ouse, Derwent, Wharfe, Dun, Roseberry, Pen-y-gent), and of certain towns and villages (as York, Catterick, Beverley, and Ilkley), but not enow to hinder the speedy adoption of the new language, the re-naming of many settlements, and the formation of more new ones with Anglian names. The subsequent Danish invasion slightly complicated this matter; but I think it is safe to say that the changes in Yorkshire were more nearly universal than in counties like Devonshire, where we know that the descendants of the Welsh constitute the majority. If the names of the rivers Swale and Hull be really Teutonic, as Greta undoubtedly is, the fact is significant; for no stream of equal magnitude with the Swale, in the south of England, has lost its Keltic appellation.

We do not know much of the Anglian type, as distinguished from the Scandinavian one which ultimately overlaid it almost everywhere to a greater or less depth. The cranial form, if one may judge of it by the skulls found in the ancient cemetery of Lamel-Hill near York, was not remarkably fine, certainly not superior to the ancient British type as known to us, to which, moreover, it was rather inferior in capacity. There is some resemblance between these Lamel-Hill crania and the Belair or Burgundian type of Switzerland, while the Sion or Helvetian type of that country bears some likeness to our own Keltic form.

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* It has been conjectured that the Parisii were Frisians; but I think it very unlikely.

* Unless indeed York was the "municipal town" occupied by Cadwalla and besieged by his Anglian adversaries.

The group of tumuli called the Danes' Graves, lying near Driffield, and described by Canon Greenwell in the *Archæological Journal*, have yielded contents which are a puzzle for anthropologists. Their date is subsequent to the introduction of the use of iron. Their tenants were evidently not Christians; but they belonged to a settled population. The mode of interment resembles nothing Scandinavian; and the form of the crania is narrower than is usual, at least in modern times, in Norway and Denmark. It is hazardous to conjecture anything about them; but I should be more disposed to refer them to an early Anglian or Frisian settlement than to a Danish one.

We come now to the Danish invasions and conquest, which, as well as the Norman one that followed, was of more ethnological importance in Yorkshire than in most other parts of England. The political history of Deira, from the ninth century to the eleventh, the great number of Scandinavian local names (not greater, however, in Yorkshire than in Lincolnshire), and the peculiarities of the local dialect, indicate that Danes and Norwegians arrived and settled, from time to time, in considerable numbers. But in estimating these numbers we must make allowance for their energy and audacity, as well as for the very near kinship between the Danes and the Northumbrian Angles, which, though it did not prevent sanguinary struggles between them at first and great destruction of life, must have made amalgamation easy, and led the natives readily to adopt some of the characteristics of the invaders.

Whatever the Danish element in Yorkshire was, it was common to Lincolnshire and Nottinghamshire, and to the north-eastern part of Norfolk; and it was comparatively weak in Northumberland and even in Durham. In Yorkshire itself, it was irregularly distributed, the local names in *by*, *toft*, and *thwaite*, and the like being scattered in a more or less patchy manner, as may be seen on Mr. Taylor's map. They are very prevalent in Cleveland, as has been shown by Mr. Atkinson. Again, the long list of the landowners of the county under Edward the Confessor, given in Domesday Book, contains a mixture of Anglian with Scandinavian names, the latter not everywhere preponderating. Here, again, Cleveland comes out very Danish. I am inclined to believe that the Anglian population was, in the first fury of the invasion, to some extent pushed westwards into the hill-country of the West Riding, though even here distinctly Danish names, such as Sowerby, are quite common. Beverley and Holderness perhaps remained mainly Anglian.

The Norman conquest fell upon Yorkshire, and parts of Lancashire and Durham, with unexampled severity. It would seem that the statement of William of Malmesbury that the land lay waste for many years through the length of sixty miles, was hardly, if at all exaggerated. The thoroughness and the fatal effects of this frightful devastation were due, no doubt, partly to the character of William, who, having once conceived the design, carried it out with as much completeness and regularity as ferocity, and partly to the nature of the country, the most populous portion of which was level and devoid of natural fastnesses or refuge, but also, in some degree, to the fact that the Northumbrians had arrived at a stage of material civilisation at which such a mode of warfare would be much more formidable than while they were in a more barbarous condition, always prepared for fire and sword, and living, as it were, from hand to mouth. Long ages afterwards the Scots told Froissart's informants that they could afford to despise the incursions of the English, who could do them little harm beyond burning their houses, which they could soon build up again with sticks and turf; but the unhappy Northumbrians were already beyond that stage.

In all Yorkshire, including parts of Lancashire, Westmoreland, and Cumberland, Domesday numbers only about 500 freemen, and not 10,000 men altogether. This great destruction, or rather loss of population (for it was due in some measure to the free or forced emigration to Scotland of the vanquished), did not necessarily imply ethnological change. Let us examine the evidence of Domesday on this point. It agrees with that of William of Malmesbury, that the void created by devastation remained a void, either entirely or to a great extent. Whole parishes and districts are returned as "waste." In one instance 116 freemen (sockmanni) are recorded to have held land in King Edward's time, of whom not one remained; in another, of 108 sokemen only 7 remained. But foreigners *did* settle in the county to some extent, either as military retainers of the new Norman lords, as their tenants, or as burgesses in the city of York, where 145 francigenæ (Frenchmen) are recorded as inhabiting houses.

Of the number maintained by way of garrisons by the new nobility, one can form no estimate; but considering the impoverished and helpless condition of the surviving natives, such garrisons would probably not be large. But from the enumeration of mesne tenants, or middlemen, some inferences may perhaps be drawn. On six great estates, comprising the larger part of Eastern and Central Yorkshire, sixty-eight of these tenants are mentioned *by name*, besides 11 milites, or men-at-arms. Only 11 of the 68 bear names undoubtedly English; and none of them have large holdings, as is the case with some of those bearing Norman names. On the lands of Drogo de Bevrere, about Holderness, several of the new settlers were apparently Flemings.

The western part of the county, however, or the greater part of it, had been granted to two lords who pursued a more generous policy. Alan, count of Bretagne, the founder of Richmond, had twenty-three tenants, besides twelve milites, men-at-arms with very small holdings. Of the twenty-three, nine were Englishmen, in several instances holding as dependents the whole or part of what had been their own freeholds. The Breton ballads and traditions seem to favour the supposition that Count Alan's Breton followers mostly returned home; and Count Hersart de la Villemarquée, the well-known Breton archæologist, informed me that his ancestors returned to Bretagne from Yorkshire in the twelfth century. On the whole, I do not think it probable that the Breton colony was numerous enough to leave distinct and permanent vestiges; but if any such there are, they may be looked for in the modern inhabitants of Richmond and Gilling.

Ilbert de Lacy, again, had a great domain, including most part of the wapentakes of Morley, Agbrigg, Skyrack, and Staincross, extending, that is, far to the north and south of our present place of meeting. Bradford, by the way, was then hardly so important and wealthy as at the present day. A thane named Gamel had held it at the time of Edward the Confessor, when it was valued at 4*l*. yearly; but at the time of the survey it was waste, and worth nothing.

Sixty-seven mesne tenants under Ilbert de Lacy are mentioned, of whom no less than forty-one bore English names, and only twenty-six foreign ones. It is probable, therefore, that in this important part of the county the ethnological change wrought by the Conquest was not greater, if so great as in England generally, but that in the centre, east, and north-east it was of some moment, and that the Scandinavian element of population suffered and lost more than the Anglian.

It might be a matter of some interest to a minute ethnologist or antiquarian to trace out fully the local history after the Conquest from an ethnological point of view, investigating particularly the manner and source of the repeopling of the great plain of York.

After this had been completed, no further change of ethnological importance took place during several centuries. The Flemings and Frisians, who, in considerable numbers, settled at various times in Leeds, Halifax, and Wakefield, whether drawn hither by the course and opportunities of trade, or driven by the persecutions of Philip II. and the Roman Catholics, brought in no new element, and readily amalgamated with the kindred race they found here.

The more recent immigrations into the West Riding and Cleveland from all parts of Britain, and even from the Continent, have interest of other kinds. Vast as they have been, they have not yet obscured in any great degree the local types, physical or moral, which still predominate almost everywhere, though tending of course to assimilate themselves to those of the mixed population of England in general.

In describing these types I prefer to use the words of Prof. Phillips, who, in his "Rivers of Yorkshire," has drawn them in true and vivid colours. He speaks of three natural groups:—

"First. Tall, large-boned, muscular persons; visage long, angular; complexion fair or florid; eyes blue or grey; hair light, brown or reddish. Such persons in all parts of the county form a considerable part of the population. In the North Riding, from the eastern coast to the western mountains, they are plentiful.

Second. Person robust; visage oval, full and rounded; nose often slightly aquiline; complexion somewhat embrowned, florid; eyes brown or grey; hair brown or reddish. In the West Riding, especially in the elevated districts, very powerful men have these characters.

"Third. Person of lower stature and smaller proportions; visage short, rounded, complexion embrowned; eyes very dark, elongated; hair very dark. Individuals having these characters occur in the lower grounds of Yorkshire, as in the valley of the Aire below Leeds, in the vale of the Derwent, and the level regions south of York."

I have chosen to quote from Professor Phillips rather than to give descriptions of my own, both because his acquaintance with the facts is more extensive than mine, and because I desire to pay my small tribute to the genius and insight of the author of a work so unique and so admirable as his upon Yorkshire.

He ascribes the first and second of these types mainly to a Scandinavian, the last to a Romano-British, or possible Iberian origin; and appears to think that the first, the tall, fair, long-faced breed, resembles the Swedes, and that the second, the brown burly breed of the West Riding, is more Norwegian in character. He probably selects the Swedes as the purest or most typical of the Scandinavian nations. For my own part, I am disposed to treat the first as Norwegian more than Anglian, the second as Anglian rather than Norse, and Norse rather than British. The tall fair type engrosses most of the beauty of the north, having often an oval face, with a fine straight profile nearly approaching the Greek, as Knox and Barnard Davis, two close observers, have both remarked. And it is markworthy that it reappears in force almost everywhere in Britain where Norse blood abounds, *e.g.* in Shetland, Orkney, Caithness, in the upper class of the Hebrideans, in Cumberland, Westmoreland, and Lonsdale, about Lincoln (where Professor Phillips also noted it) and the Vale of Trent, and about the towns of Waterford and Wexford. The second type, on the other hand, much resembles a prevailing form in Staffordshire, a very Anglian county. A notable point about it is the frequency of eyes of a neutral, undecided tint, between light and dark, green, brown, and grey, the hair being comparatively light. The third is of more doubtful and of more manifold origin. Iberian, Britokeltic, Roman, Breton, Frenchman, may all, or any of them, have contributed to its prevalence. I am inclined to think, though on rather slender grounds, that it is common in some of the districts depopulated by the Conqueror. Professor Phillips speaks of its smaller proportions, but it includes many robust men. It is probably far from well representing the Brigantian type, which seems to me to have influenced the other types, but rarely to crop out all purely.

The breadth of the head is, on the average, somewhat greater in Yorkshire than in other parts of Britain; so we are informed by the hatters. In this the natives of Yorkshire agree with those of Denmark and Norway, who have rather broader heads than those of Sweden and of Friesland.

I have already spoken of the colours of the eyes and hair The latter is, on the whole, lighter in Yorkshire than in most parts of England; but dull rather than bright shades prevail. In the east, at Whitby, Bridlington, and Beverley, in Teesdale and Middle Airedale, light hair is particularly abundant; in Craven, as might have been expected, it is less so. Other parts of the county are not so well known to me, and in this matter I have to trust to my own observations.

As to the stature and bulk of the people, however, I have much and accurate information, through the kindness of numerous observers, some of them of repute as naturalists. These are Mr. Atkinson of Danby, Mr. Tudor of Kirkdale, Dr. Wright of Melton, Dr. Christy of the North Riding Asylum, Drs. Kelburne King and Casson of Hull, Mr. Ellerton of Middlesborough, Mr. Wood of Richmond, Mr. Kaye of Bentham, Mr. Edy of Grassington, Dr. Paley of Ripon, Dr. Ingham of Haworth, Messrs. Armitage of Farnley, Dr. Wood of Kirkby Overblow, Dr. Aveling and Mr. Short of Sheffield, Mr. Miller, late of Wakefield Prison, and a clergyman on the Wolds, whom the prejudices or fears of his parishioners will not allow me to name. "A Yorkshireman," complained this last gentleman, "is a difficult animal to catch and weigh and measure;" but a very large number of them have been subjected to these processes by my obliging correspondents. The general result is that in the rural districts they are remarkably tall and stalwart, though not, except in parts of the west, so heavy as their apparent size would indicate—but that in the towns, and especially in Sheffield, they are rapidly degenerating; and I conclude from the Haworth report that the same is the case in the manufacturing villages. In many of the rural districts the average ranges between 5 ft. 8 in. and 5 ft. 9 in., and about Richmond and on the Bentham Fells is considerably higher: while at Sheffield and even at Haworth, it may hardly reach 5 feet 6 inches. The causes of this great degeneration are manifold: some of them may easily be traced; but either the will or the power to remedy the evil is wanting.

Of the moral and intellectual endowments of Yorkshiremen, it may perhaps appear presumptuous or invidious to speak; but the subject is too interesting to be passed by in silence, and I will endeavour to treat it without either "extenuating, or setting down aught in malice." In few parts of Britain does there exist a more clearly marked moral type. To that of the Irish it has hardly any affinity; but the Scotchman and the Southern Englishman alike recognise the differences which distinguish the Yorkshire character from their own, but are not so apt to appreciate the numerous respective points of resemblance. The character is essentially Teutonic, including the shrewdness, the truthfulness without candour, the perseverance, energy, and industry of the Scotch, but little of their frugality, or of the theological instinct common to the Welsh and Scotch, or of the imaginative genius, or the more brilliant qualities which sometimes light up the Scottish character.

The sound judgment, the spirit of fair-play, the love of comfort, order, and cleanliness, and the fondness for heavy feeding are shared with the Saxon Englishman; but some of them are still more strongly marked in the Yorkshireman, as is also the bluff independence—a very fine quality when it does not degenerate into selfish rudeness. The aptitude for music was remarked by Giraldus Cambrensis seven centuries ago; and the taste for horseflesh seems to have descended from the old Norsemen, though it may have been fostered by local circumstances. The mind like the body, is generally very vigorous and energetic, and extremely well adapted to commercial and industrial pursuits, as well as the cultivation of the exact sciences; but a certain defect in imaginative power must, I think, be admitted, and is probably one reason, though obviously not the only one, why Yorkshire, until quite modern times, was generally behindhand in politics and religion, and why the number of her sons who, since Caedmon, have attained to high eminence in literature is not above the average of England.

DIARY

WEDNESDAY, OCTOBER 1.

ROYAL MICROSCOPICAL SOCIETY, at 8.—A description of some new species of Diatomaceæ: F. Kitton.—On an Organism found in fresh pond water: Dr Maddox.

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THURSDAY, OCTOBER 2, 1873.

ON MEDICAL STUDIES

AS at the present time so many students have just assembled at the medical schools in London and the provinces to commence or continue their medical education, we think that notwithstanding the advice so freely given them in all directions by their friends, and especially by those who deliver the introductory addresses at the different hospitals, there are some few points to which their attention cannot be too frequently directed.

First, with regard to the range of subjects which is required by the higher examining boards, such as the University of London, in the earlier stages of the medical curriculum. There cannot be the least doubt, though several who have not participated in its advantages are fond of expressing an opinion to the contrary, that the wider and more extended the field of study that can be grasped by a student at the outset, the more chance he has of ultimate success; and he who has no higher object in view than that of passing the least difficult of the necessary examinations which give him a licence to practise, must ultimately find himself far behind in the race. In surgery, no doubt, there are a few who, without much scientific knowledge, have attained great eminence as operators, on account of their manual dexterity; but this position ought not to be the aspiration of the commencing student, as the reputation is generally of short duration, and is not much higher than that of a man who has rowed in a winning University boat-race.

One great argument in favour of a liberal medical education is that the mental capacities of the young men who commence it are very different; and if those who are the most gifted have but little chance of acquiring a knowledge of the facts and theories of Science as they stand at the time at which they study, they are placed in a position of disadvantage for future research, and find it always difficult to make up for lost time. When all have to start on the extended course, which includes a knowledge of physics, botany, pure physiology, and chemistry, those who have the capacity for higher work in Science obtain an opportunity of developing their tendencies, and are often led to give up their original design of being medical practitioners, to become specialists in their favourite subjects, and an honour to Natural and Medical Science. This means of selecting the best men for scientific work would be a sufficient result in itself to justify the primary education of all medical students in the pure sciences that relate indirectly to medicine; for it must be remembered by those who hold the contrary opinion, that it is to its scientific supporters that the medical profession owes most of its dignity. If we look at the names of those who stand highest in the profession at the present day, it is readily seen that nearly all have their reputation based on a thorough scientific foundation. The lowering of the scientific standard would, therefore, undoubtedly lower the status of the profession amongst society at large, and it will be generally acknowledged that such a result is anything but desirable.

The recent thorough working out of the cause of the outbreak of typhoid fever in the west end of London this summer, shows how satisfactory are the results which follow the employment of a rigorous scientific method of observation. How long it would have remained undiscovered that the impurities in the milk-supply of a locality are the not unfrequent cause of an outbreak of typhoid fever it is impossible to say, if the subject had not been entered upon and carried through in a manner which does great credit to those who detected its origin, as Dr. Ballard had done on a former occasion in Islington.

A second point worthy of attention is the social position of the medical student. That he generally does not compare favourably with the undergraduates of Oxford and Cambridge is certain; but why this is the case does not seem to be so definitely settled. One of the great reasons is that the medical education does not include anything but the mental training: and although the medical student is like the average University undergraduate so far as age, preliminary education, and object of life are concerned, nevertheless after a curriculum of three years or more, the latter has made more progress as a social individual. The different natures of their studies cannot be proved to have anything to do with the difference in the results, and nearly all may be traced to the systems in which each participates. The University undergraduate is subject to two independent influences for good. A fixed code of University and College rules restrains him in many directions, as with regard to his conduct and the allotment of his time; at the same time that a much more stringent, but not written code, the result of his necessarily intimate relations with a large number of companions of his own age, regulates the details of his actions continually, the infringement of which code removes him from his most pleasurable source of enjoyment during leisure hours. Most medical students miss both of these. The absence of a Proctorial system and College rules makes him free to his heart's content; and the comparative smallness, as a rule, of the clique to which he belongs, helps to encourage rather than remove objectionable individual peculiarities, which would not be tolerated in general society. It is excessive freedom which is the bane of the young medical student, and the introduction of any system which provided a reasonable amount of restraint during the medical education would undoubtedly improve the social status of its undergraduates. Attempts have been made, but on too small a scale to be really successful. If the leading schools could be persuaded to invest money in building suitable apartments for their pupils, and spend part of the profits which must necessarily accrue to them, in giving scholarships, open only to those who resided in such buildings, a system might be developed which, after some time, from the convincing evidence it would give of its advantages, would cause all to participate in it.

Until there is much more co-operative feeling among the different schools in London, it is difficult to conceive how this or any other really marked improvement can be effected. Whilst things stand as they do, we are convinced that, in the long run, those will enjoy the most profitable studentship, and afterwards find themselves in the most advantageous position, who put themselves under reasonable restraint, and endeavour to extend their circle of acquaintance beyond the few sympathising "chums," who generally have but little influence for good, at the same time that they often, by an unconscious process of approval and persuasion, help to exaggerate bad qualities and develop worse.

LYELL'S "ANTIQUITY OF MAN"

The Geological Evidences of the Antiquity of Man, with an Outline of Glacial and Post Tertiary Geology, and Remarks on the Origin of Species, with special reference to Man's First Appearance on the Earth. By Sir Charles Lyell, Bart., M.A., F.R.S. Fourth Edition Revised. Illustrated with Woodcuts. (London: John Murray, 1873.)

SINCE the first volume of "The Principles of Geology" appeared—now more than forty-three years ago—Sir Charles Lyell has put forth an uninterrupted series of new works or new editions, and we have now arrived at the 11th edition of the "Principles," the 7th of the "Elements of Geology," and the 4th of the "Antiquity of Man." A most striking feature of these works is, that they give the fullest and most accurate scientific details, and the most philosophical discussion of principles and results, without for a single page ceasing to be interesting to any well educated and thoughtful man. Perhaps no author has attained in so perfect a degree the art of making science popular without ever attempting to popularise it, or has produced a series of works which are equally acceptable to the experienced geologist and to the general reader.

The present edition of the well-known "Antiquity of Man" will fully sustain the author's high reputation, since it is not a mere corrected reprint of former editions, but, in several important respects, a new work, embodying all the most recent discoveries and researches on the various subjects of which it treats, while several discussions of temporary or personal interest have been omitted. Almost every chapter contains either important new facts or new results derived from a more careful study of old ones; while some are almost wholly rewritten, as, for example, chap. xii., in which the most recent researches on the climate of the Crag period is very fully given; and it would need a very acute critic to discover in these any lack of that lucidity of arrangement and vigour of thought which have always distinguished Sir Charles Lyell's writings.

The most striking additional facts bearing directly on the Antiquity of Man are so well known and have been so often before the public, that it is unnecessary to enumerate them here; but it may be advisable to remark briefly upon a theoretical point of some importance on which the author's views seem open to question; and there are also a few matters connected with the general subject which seem worthy of attention.

Although Professor Gastaldi, of Turin, after a careful study of the Italian Alps, has adopted Professor Ramsay's view of the excavation of alpine lake basins by ice, Sir Charles Lyell is still strongly opposed to that view. He maintains that they have been produced by changes of level in valleys, producing depressions which have been preserved during the glacial epoch by being filled with ice, while at all other times they were either soon filled by *débris*, or their lower barriers were cut down as fast as they were formed. He thus accounts for the fact that lakes only occur in any abundance in glaciated districts. He further maintains that the erosive power of glaciers, as indicated by the muddy torrent that always issues from them has been overrated, because "the flour of rock" thus produced is due, not solely to the wearing down of the floor of the valley, but, "to a considerable extent," to the grinding up of the stones which fall upon the glacier and are engulphed in its crevasses.

There are doubtless many difficulties in Prof. Ramsay's theory, and much remains to be done to verify it, but it does seem to cover a larger portion of the facts than that now opposed to it. There is no evidence before us to show how much of the glacier mud is respectively due to the two sources above referred to, but the enormous bulk of many of the old moraines, where they have not been destroyed by subsequent denudation, seems amply sufficient to account for the *débris* which falls upon a glacier; while the wide extent of glaciated surfaces, and the manner in which the very hardest upturned strata are often planed off or *moutonnées*, is equally convincing proof that large masses of rock have been ground down by glaciers. The evidence of this is very remarkable also, in the case of the Loess, a deposit which covers an enormous extent of country, and in some parts of the valley of the Rhine reaches a thickness of near 1,000 feet, and which Sir Charles Lyell himself considers to be undoubtedly glacial mud. It is difficult to conceive how such an enormous amount of mud could have been formed except by a grinding power capable of producing most of the effects imputed to it by Prof. Ramsay. It is considered to be one of the most powerful arguments against the ice-erosion theory that no lakes exist in certain valleys which were undoubtedly filled with enormous glaciers; but the answer to this is, that a lake will only be produced when the erosion is considerably greater at one part of the valley than at another, and this inequality may be caused either by unequal hardness of the subjacent rocks or by the piling up of the ice to a greater thickness in certain spots by the convergence of several branch glaciers, as must have been notably the case over the site of Lago Maggiore, which received the icy streams descending from near 100 miles of the loftiest Alps. It must also be remembered, that at such points of convergnce the rate of motion of the glacier will be much more rapid than elsewhere, in order to discharge the accumulated ice-streams; and we shall thus have a double cause of increased grinding in such positions. A difficulty of a somewhat similar nature, and which cannot be so easily overcome, besets the unequal-subsidence theory, which can hardly be made to account for the thousands and tens of thousands of lakes so thickly scattered over the lowlands of Northern Europe and America.

It is somewhat remarkable that notwithstanding the numerous researches in post-tertiary caves and gravels in all parts of Europe, no human remains have been discovered which can be proved to be older than those found by Dr. Schmerling more than forty years ago in the caverns near Liége. After many years' labour this gentleman, a skilful anatomist and palæontologist, published, in 1833, a detailed account of his researches, copiously illustrated. It is curious to see, from Sir Charles Lyell's account of this work, how completely its author anticipated all the more important results of modern cave exploration, and how thoroughly he had worked out that doctrine of the antiquity of man which the great majority of geologists so long attempted to put down. Such wholly independent researches as those of Schmerling in Belgium, McEnery in Devonshire, and Boucher de Perthes in France, made by careful and conscientious observers, and all converging to the demonstration of one fact, were for many long years laughed at or ignored, solely because they clashed with preconceived opinions. When this occurred with the students of a science which had already fought and won many hard battles against popular and theological prejudice, and whose whole course of study should have taught them how to interpret the evidence adduced, we are bound to deal tenderly with the less unjustifiable prejudices of those who have had no such training.

Notwithstanding the lesson these long-ignored facts should have taught them, some geologists still exhibit a strange fear or hesitation in facing the whole results of modern inquiries on the subject. How is it that, whenever any estimate is made of the lapse of time (expressed in years) since any human remains or works of art were deposited, the lowest possible estimate is almost always chosen? One would think that, having once got beyond the traditional six thousand years, the period of man's past existence would be a matter of purely scientific inquiry, to be arrived at by careful estimates in a variety of ways. But how can we possibly arrive at the truth by always taking the lowest estimate? we might just as reasonably always take the highest. Is there any merit in arriving at a false result so that the figures are small? Is it really the "safe" side so to calculate that we shall almost certainly be wrong? Astronomers do not think those observations most likely to be correct which give the smallest distances and sizes of the heavenly bodies and it would be more dignified and more scientific if geologists, whenever any data exist on which to found a calculation, should insist on taking the mean result of various impartial estimates as that most likely to be the true one. From this point of view it may be interesting to give a summary of the more important attempts which have yet been made to determine the antiquity of human remains or works of art.

From observations at the delta of the Tinière and on the lakes of Neufchatel and Bienne, the bronze age in Europe has been determined with approximate accuracy to have been from 3,000 to 4,000 years ago, and the stone age of the Swiss Lake dwellings at from 5,000 to 7,000 years and an indefinite anterior period. The burnt brick found 60 ft. deep in the Nile alluvium indicates an antiquity of about 20,000 years, taking, from a calculation by Mr. Horner, the estimate of 3½ in. per century as the rate of deposit of the mud. Another fragment found at 72 ft. deep is estimated by M. Rosière to be 30,000 years old. Some human bones found in a lacustrine formation in Florida have been considered by Agassiz, after a careful examination of the locality, to be at least 10,000 years old. A human skeleton found at a depth of 16 ft. below four buried forests superposed upon each other, has been calculated by Dr. Dowler to have an antiquity of 50,000 years.

These latter estimates may be very uncertain, but we have no reason to think them improbable, from what we know of the great changes of physical geography that have undoubtedly taken place since man existed. Kent's Cavern at Torquay furnishes a good example of these, since the whole drainage of the surrounding country must have been very different when the great thickness of cave earth was deposited by floods rushing through the cavern which is now situated in an isolated hill. We have here indications of an immense antiquity from various sources. The upper stalagmitic floor itself marks a vast lapse of time, since it divides the relics of the last two or three thousand years from a deposit full of the bones of extinct mammalia, many of which, like the reindeer, mammoth, and glutton, indicate an arctic climate. It has been remarked that the varying thicknesses of the stalagmitic floor, from 16 in. to 5 ft. and upwards, closely correspond to the present amount of drip in various parts of the cave, so that the cave itself with its various fissures and crevices does not appear to have been materially altered since the stalagmite was deposited. It is true that the drip may once have been greater, but it may also have been less, and we do not know that a more copious drip would necessarily produce a more rapid deposit of stalagmite. But names cut into this stalagmite more than two centuries ago are still legible, showing that, in a spot where the drip is now very copious, and where the stalagmite is 12 ft. thick, not more than about one-eighth of an inch, or say one-hundredth of a foot, has been deposited in that length of time (British Association Report, 1869, p. 196). This gives a foot in 20,000 years, or 5 ft. in 100,000 years; and there is no reason whatever to consider this to be too high an estimate to account for the triple change of organic remains, of climate, and of physical geography. But below this again there is another and much older layer of stalagmite, generally broken up and imbedded in the cave earth. This older stalagmite is very thick and is much more crystalline than the upper one, so that it was probably formed at a slower rate. Yet below this again, in a solid breccia, very different from the cave earth, undoubted works of art have been found. A fair estimate will therefore give us, say, 100,000 years for the upper stalagmite, and about 250,000 for the deeper layer of much greater thickness, and of more crystalline texture. But between these we have a deposit of cave-earth which implies a different set of physical conditions and an alteration in the geography of the surrounding country. We have no means of measuring the period during which this continued to be formed, but it was probably very great; and there was certainly some great change in physical conditions during the deposit of the lower stalagmite, because the fauna of the county underwent a striking change in the interval. If we add 150,000 years for this period, we arrive at the sum of half a million as representing the years that have probably elapsed since flints of human workmanship were buried in the lowest deposits of Kent's Cavern. It may be objected that such an estimate is so loose and untrustworthy as to be altogether valueless; but it may be maintained, on the other hand, that such estimates, if sufficiently multiplied, are of great value, since they help us to form a definite idea of what kind of periods we are dealing with, and furnish us with a series of hypotheses to be corrected or supported by further observation, and will at last enable us to arrive at the antiquity of man within certain probable limits of error. Without laying stress on any portion of the above very rude estimate, it may, I think, be averred that it is not palpably too high, but is just as likely to be too low; and this last supposition will be rendered more probable when we consider the vast lapse of time implied by the position of some of the recently discovered palæolithic weapons.

The flint tools found in the gravel at Bournemouth, in the Isle of Wight, and near Salisbury, at elevations of from 80 to 100 feet above the present valleys, imply, according to the best observers, that the whole series of surrounding river valleys have been excavated since they were deposited, and that the system of drainage and position of the coast-line have been very greatly altered. The hippopotamus of the Gower Caves implies changes equally great, since the peninsula of Gower now contains only small streams, and could not possibly have had a large river without very important changes in its relations to the adjacent country. The position of the flint weapons in the valley of the Somme, at Hoxne in Suffolk, and in many other places, all combine in indicating that very important changes in physical geography have taken place since they were deposited. We can hardly suppose that in all these different localities the changes were abnormally rapid, especially as in no case do records of the historic period indicate that any remnant of the process was then going on; and from what we do know of the rate of such changes, and their intermittent nature, we are entitled to affirm that the most extreme estimates yet made of the antiquity of the men who fashioned and used the palæolithic implements is quite as likely to be under as over the truth.

There is as yet no clear evidence that man lived in Northern Europe before the glacial epoch, and even if he did so the action of the ice sheet would probably have obliterated all records of his existence. Every evolutionist, however, now believes that he must have existed far back in the tertiary period, and that the proof of it will be found, if at all, in some of the warmer regions of the old world. Here is surely a problem of grand and absorbing interest awaiting solution at our hands. Geologists are not usually wanting in energy or enterprise, and they number in their ranks many wealthy men. It is to be hoped that they will soon energetically attack the problem; and no more promising field of research offers itself than the limestone caves of Borneo, which can be explored with perfect safety, and at a moderate expense. We can hardly now expect any great additions to our knowledge respecting the antiquity of man in Northern and Central Europe, and must go to warmer regions if we wish for new discoveries and startling revelations.

A. R. WALLACE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Fellowship at Magdalen College

I THINK the notice in NATURE of Sept. 25 respecting the election about to take place to a Natural Science Fellowship at Magdalen College requires some comment. The amount of academic preferment which falls to the share of science in Oxford is so small, that it might reasonably be demanded that what there is should be thrown open to as many candidates as possible. When, therefore, it was announced that the Fellowship would be given for proficiency in Biology, it might have been inferred that the electors had this object in view. Biology is held, elsewhere than in Oxford, to be the science which treats of the laws governing organization and vital activity; in other words, structure and function in *all* forms of life, whether vegetable or animal. It was not, perhaps, an unreasonable inference, therefore, to draw from the terms of the notice, that it was the intention of the College to make Biology in its widest sense the foundation of the examination, and to allow individual candidates to exhibit, in addition, such detailed knowledge as they might possess of Zoology, Botany, or even Palæontology. This would not have attributed to Biology a wider meaning than, for example, Mr. Herbert Spencer or the Science and Art Department attach to it. Thinking it desirable, however, to get some official information upon the subject, I wrote to the President, who, after some delay, replied that, in his opinion, as preference would be given to Biology, it would be useless to offer Botany as a special subject. This is not more reasonable than it would be to say, that because Physics was to be the subject of an examination it would be useless to offer Electricity or Heat as a special subject. But the terms of the President's reply were rather ambiguous, and I therefore made some further inquiries. I learnt, as the result, that the College considered it impossible to compare the merits of a candidate who stood on the Zoological, with one who stood on the Botanical, side of the general subject.

I think myself the difficulty is not one which should have been found insuperable; but, assuming that the College had sufficient grounds for a different opinion, then I think the electors should not have offered their Fellowship for Biology, when what they really had in view appears to be a detailed knowledge of the Zoological preparations in the University Museum.

W. T. THISELTON DYER

The Sphygmograph

THERE appears in NATURE, vol. viii. p. 330, a notice of a thesis for the M.D. Cantab. on the subject of Bright's disease, in which reference is especially made to some sphygmographic observations therein contained. It is apperently from the pen of Mr. Garrod, who is himself the author of interesting and important researches with the sphygmograph and cardiograph. While agreeing with a part of my explanation of the normal pulse tracing, as regards the points in which it differs from the view commonly received, he takes exception to the account which I have given of the tidal or first secondary wave. It may be well to say in reply a few words upon the point at issue, since the reference to it in the thesis was very brief and incidental, and I should not wish it to be taken as a full account of my views as to the mechanism of the pulse.

The explanation of Mr. Garrod himself is that the tidal wave is an instantaneous wave due to the closure of the aortic valves. This theory was first proposed by M. Marey to account for the tidal wave in many of its forms; but, so far as I know, it has not been adopted by any writer on the subject in England with the exception of Mr. Garrod. There is this difference, however, between them, that while M. Marey holds that the dicrotic wave has nothing to do with the aortic valves, but is a reflection from the periphery, Mr. Garrod considers that it is the wave of expansion from the closure of the aortic valves, which becomes separated from the instantaneous wave as it recedes from the heart. Thus the faculty of originating two waves of different velocity, which by most writers is attributed to the first impulse of the heart, combined with the closure of the mitral valve, is by Mr. Garrod denied to that event, but ascribed to the closure of the aortic valves. Now I believe it to be mechanically impossible for any wave to be propagated with a velocity different from that of the wave of expansion, except the purely vibratory wave of sound, and Mr. Garrod appears himself to hold that a mere vibration produces no elevation in the tracing. The question, however, may easily be determined experimentally. If there appear in the tracing two waves which are travelling with different velocities, their relative position will vary at different distances from the heart. Let, therefore, anyone who wishes to settle the question for himself take tracings of a good many markedly tricrotic pulses, say from the femoral and also from the dorsalis pedis arteries. According to the view of Dr. Burdon Sanderson and most other writers, the interval between the primary and tidal waves ought to be more than doubled in the dorsalis pedis; according to the view of Mr. Garrod, on the contrary, that between the tidal and dicrotic waves. It will be found that there is no such considerable and constant variation as would be required by either theory, although the tidal wave does not maintain its relative position so closely as does the dicrotic wave. The kind of pulse best of all suited for this experiment is fortunately[*] rather scarce; it is that of a young person who has a granular kidney, but is free from dropsy.

The theory of Mr. Garrod may appear at first sight suitable to one of the forms of healthy pulse, in which the tidal wave appears as a slight elevation preceding the dicrotic wave, but I do not think that it will be accepted by anyone who has watched its variation in a large number of diseased pulses, and has seen it pass through every gradation, from a separate and distinct wave to a mere convexity in the descending curve, which may commence immediately from the top of the primary upstroke. In the pulse of rigid arteries this latter form is often taken when the heart is quiet, but when it acts more vigorously the tidal wave becomes separated, owing to the development of the so-called "percussion element," which is really the effect of acquired velocity in the sphygmograph. The case which should afford the most crucial test is perhaps that very rare one in which the aortic orifice is closely obstructed, and scarcely any valves remain to produce a wave by their closure. The tidal wave should then, according to Mr. Garrod's theory, be at least greatly diminished, but, in point of fact, it is then more greatly developed than under any other circumstances whatever. Evidence to the same effect may be derived from the use of an artificial heart with experimental elastic tubes, for it is found that, under suitable conditions, the tidal wave may be greatly prolonged by a protracted contraction of the heart. This was first shown by Mr. Mahomed in the *Medical Times*, and although I believe his theory to be erroneous as to the relation between the primary and tidal waves, yet, with regard to the practical associations of the tidal wave, my experiments have led me to conclusions which are quite in agreement with his, namely, that three things contribute to the development of the tidal wave—increase of pressure, diminution of elasticity, and prolongation of the heart's contraction.

Mr. Garrod argues that the tidal wave cannot have anything to do with the inertia of the long lever, because it is shown in the reflecting sphygmoscope, in which that is absent. I do not, however, consider that the result is due solely, and possibly not even chiefly, to the inertia of the lever, but to that of the instrument altogether, and inertia is possessed likewise by the sphygmoscope. Moreover, since the latter does not record its indications, it would be difficult to ascertain whether the tidal wave shown by it corresponds precisely to that of the sphygmographic tracing. Another instrument has also been called a sphygmoscope, in which the motion of the pulse is shown by the variation of a gas flame. In this there appears indeed the counterpart of the tidal wave, but not in the form of a single wave; instead of this a series of small waves is shown. These may appear only as a slight quivering motion, and are evidently due to the oscillation of the elastic diaphragm upon which the pressure of the pulse is received.

Mr. Garrod maintains his own theory especially on the ground of observations with his cardio-sphygmograph, showing the commencement of the tidal wave in the radial pulse to be synchronous with the closure of the aortic valves. But the determination of the moment of that closure depends on the correctness of his interpretation of the minor elevations in the cardiographic tracing. These are numerous, and his interpretation of them all is most ingenious, but to accept it requires an implicit faith that the instrument itself has no part in producing any of the minor features of the curve. Now, that curve was drawn by a lever, moving on a pivot, and balanced between two springs, which would seem a contrivance peculiarly liable to oscillate. When therefore it is further found that in cardiac tracings published by other observers, or those obtained by applying the sphygmograph directly to the heart, there is no close correspondence either in the number or the position of the elevations, the conclusion can hardly be resisted that some of them are due to such oscillation. My own opinion is that neither in the cardiographic nor in the radial pulse tracing can the point corresponding to the end of systole be precisely determined.

The whole subject is one which it is difficult even to state intelligibly without a constant reference to diagrams of tracings, and therefore, for a fuller account of my views as to the theory of the pulse, particularly in reference to the complete explanation of the dicrotic wave, I must refer to a paper to be published in the next number of the *Journal of Anatomy and Physiology*.

While I consider that the construction of the sphygmograph has some influence on the tracing produced, yet I believe that, by a fortunate chance, the result is more practically useful than if the pulse-wave were recorded with perfect accuracy, for I think that slight differences in it, which would then perhaps escape notice, are, as it were, magnified and made manifest to the eye.

I may say in conclusion that I do not quite agree in the view that we must wait for the practical application of the sphygmograph until physiologists are agreed about the theory of the pulse, for, according to present appearances, that consummation is distant indeed. There is, however, among sphygmographers an agreement about practical inferences which is almost as notable as the confusion which prevails as to mechanical causes. It is possible therefore for a person to use the sphygmograph for diagnosis and prognosis, who does not even attempt to understand the cause of the waves seen in its tracings. But it must be allowed that the settling of the mechanical question is much to be desired, and that, without it, the sphygmograph cannot afford that service, which otherwise it would be capable of doing, to the solving of all general physiological problems relating to the vascular system. And, from a practical point of view, these may perhaps be regarded as among the most important in physiology, for it is probably through the agency of the vascular system that many of the greatest effects of remedies are produced.

A. L. GALABIN

On the Origin of Nerve-Force

IN a paper on this subject, by Mr. A. H. Garrod, in NATURE, vol. viii. p. 265, the author states that in cold-blooded animals, nerve-force must be generated by the difference between their own temperatures and that of the medium by which they are surrounded. Now, to take the case of a frog as a common example of a "so-called" cold-blooded animal. A few days ago, when the thermometer was standing at 71°, I took the temperature of two frogs, one was 69°, and the other 67°; the difference between their temperature and that of the surrounding air was practically *nil*. Now, on a day of this sort of temperature, it would seem that the pervious integument of the frog is continually exhaling moisture, and that in consequence the temperature falls, and would continue to fall below that of the surrounding air, were it not that it was raised by the heat generated "by the destruction of tissue that is continually going on within the body of the animal;" so between these two contending forces a state of equilibrium results, and the temperature of the animal and the surrounding air are the same. But, if this be true, it follows that the whole of the heat from the animal is used up in keeping up its temperature, and therefore none can be spared for conversion into nerve-force. Therefore, a frog at rest on a summer's day ought to have no nervous energy. Now, suppose our frog takes to leaping vigorously, he will develop a certain amount of heat, and then he ought to have a great deal of nerve-force; but it is not found that an active frog is more "nervous" than a quiescent one.

Again, the nervous irritability of a frog, though perhaps not acting with the instantaneous energy with which it acts in a mammal, still persists far longer than in other vertebrates, and will continue much longer after the somatic death of the animal, when it is quite clear that the temperature of the body and the surrounding medium will be the same. Now in this case the nerves may be so irritated as to lose all irritability, and yet, after a period of rest, this irritability will be regained, clearly, to my mind, showing that nervous energy must be generated after the death of the animal, when all differences of temperature have ceased.

Finally, it must be admitted, without the aid of any hypothesis, that the difference between the temperature of a frog and the surrounding air is, at any time, very slight; and yet this animal possesses what we call an extremely "persistent" form of nerve-force.

R. LYDEKKER

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[*] [We have omitted the prefix un- from this word; we hope Mr. Galabin will forgive us.—Ed.]

On the Polarisation of Light in the Rainbow

As I do not remember seeing any notice, in books on light and colour, about the polarisation of light in the rainbow, I think it my duty to relate the following facts, although I can scarcely think the appearance has been unobserved till now.

Three times I have tested the rainbow-light this summer, and each time I found it wholly polarised. On the first occasion, while looking at the rainbow, I thought I would examine it with a tourmaline, which I kept in my pocket. I looked at the bow, through the tourmaline, and saw the bow; but on rotating the tourmaline the bow alternately disappeared and reappeared at every quarter turn: while the light from a stack of chimneys which stood within the bow remained apparently unchanged. From this I inferred that the light of the rainbow was wholly polarised, while the other light in its neighbourhood was not so.

I have observed the vanishing and reappearing of the light of the rainbow on rotating the tourmaline on two occasions since that. I have waited for these additional occasions to make sure of the fact, as I was called away from the first observation; and when I could go back the rainbow had vanished.

The date of the second and third times are August 28 and September 4, 1873.

Leicester, Sept. 5 GEO. FINLAY

[The polarisation of the light of the rainbow was observed by Biot in 1811, and by Brewster in 1812. (See "Brewster's Optics," art. 185.) With respect to rainbows by reflection, there are two kinds—(1) that observed by X. Z. Y., in which the light comes to the eye from the water. This is not thought worthy of special mention by Brewster. (2) That in which the light of the sun reflected from water strikes the shower and forms a bow not concentric with the common bow. (See "Brewster's Optics," art. 186.) It is very easy to see that these two kinds of bow form parts of the same cone whose axis is at the same altitude as the sun, but in the opposite azimuth.—J. C. M.]

Autumnal Typhoid Epidemics

THERE appear to be two types of these,—first, the malignant and dangerous, which breaks out in isolated spots and is usually traceable either directly or indirectly to some sins of sewerage; and a second or milder form, which extends over far larger areas, is much more general, and apparently unconnected with sewage exhalations or liquid contaminations. Some observations I have lately made suggest an explanation of the origin of this latter form. We have had just a moist and rather warm summer, followed by an unusually wet autumn. Turnips, swedes, beets, mangolds, cabbages, potatoes, peas, &c., put forth luxurious foliage, and much of this, especially the lower leaves of turnips, swedes, and cabbages, have been rotted by the recent rains—so much so, that many a country lane that should have exhaled sweet balmy odours has been the abode of most unromantic stink. This is especially the case in the flat market garden areas that lie by the side of the Thames, and in these the most especially where cabbages are cultivated. I have no doubt that the partridge shooters of 1873, who have largely availed themselves of the cover of turnip-fields, will confirm my observation of their offensive odour.

Modern agriculture is, in England, chiefly developing and extending in the direction of root crops for cattle feeding, and the foliage of these is very liable to offensive decomposition under the conditions above named. When the autumn is hot and dry, their outer leaves, and also those of kitchen vegetables, drop off and return to the soil in a dry, crisp, and inodorous condition.

That the moist decomposition of such vegetable matter should supply nourishment to disease germs analogous to those which are fed by sewage, and that the exhalations of the decomposing vegetables should spread them after the manner of sewage exhalations, is obviously probable.

If I am right, the widely extended and milder forms of autumn epidemics should be most prevalent in such years as the present, and should prevail more especially in market-garden and cattle-feeding districts.

So far as my own means of observation extend, this appears to be the case, but as these are too limited to justify any positive conclusion, I throw out the above as a merely suggestive explanation, demanding further confirmation, which some of the readers of NATURE may be able to supply.

Woodside, Sept. 8 W. MATTIEU WILLIAMS

Venomous Caterpillars

OBSERVING a letter in NATURE respecting venomous caterpillars, I venture to offer a few remarks from personal experience.

The rough hairy caterpillars have a bad reputation everywhere. As a boy, the nurses told me if one got tight round my finger, *it* (and of course I understood the finger) would have to be cut off. In Switzerland they are regarded by the common people as poisonous, though, as far as I know, without foundation.

In Portugal there is a most remarkable gregarious species, known as the "procession caterpillar," from the great numbers that may be seen advancing in a body. This kind has undoubtedly the power of causing very considerable irritation to a tender skin. A specimen once crawled up the arm of my little girl, then one year old, leaving the skin-surface red and inflamed along its track; and there was a tradition at Lisbon of a child that had fallen into a mass of these larvæ, and subsequently died from the consequent inflammation.

In Brazil there is a species in the neighbourhood of Rio that, with regard to the formidable nature of its external clothing, is a veritable porcupine. It corresponds remarkably with the description of the Burmese specimen, both in size and colour. The hairs, in a state of repose, are, however, but slightly erect, and it is only when irritated or alarmed that it raises them in hostile guise. There can be no question as to the stinging properties of these hairs, to which my wife, among others, can bear testimony; but as our experimental ardour did not induce us to grasp the creature, the consequences were never serious. The largest hairs must be nearly an inch long, and the points of all have a lighter appearance, as though singed. It was interesting to watch their elevation by the animal on the approach of the finger, as though by some electric attraction. The stinging sensation is analogous to that caused by a nettle. I am inclined to think that in this case the cause was likewise analogous. It is, however, possible that the hairs are brittle, or armed with articulated branches.

With reference to the power of detaching hairs possessed by some caterpillars, a remarkable instance came under my notice in Tiguca (Brazil). It was observed in the larva of a beautiful black and white butterfly with conspicuous yellow tail. The determining principle of its existence appeared to be rather economy than defence. Consequently the hairs with which its body was covered were utilised in the construction of its cocoon. For this purpose it was first clearly necessary to shed them; after which they were dexterously crossed and recrossed over the creature's body ensconced under the shadow of some convenient leaf. In this process, if thread was used at all, it was with the greatest economy.

As it was evident that such hairs must be well adapted to their purpose, I examined them under a good microscope, when I found them armed with short barbs on all sides, especially towards the extremities. The spines were tolerably thick, giving under the lens much the appearance of a sprig of juniper.

Berne, Switzerland C. EDEN

IN reference to the article on venomous caterpillars in NATURE of the 14th inst., I beg to offer you, if the subject is not closed, my own very unpleasant experience.

On the 19th of June last, as I was sitting in my drawing-room near an open window, looking on the garden, I suddenly felt an itching sensation in my throat and arm, and on examining my dress I found a large brown long-haired caterpillar. In a few moments my skin, on the parts affected, was covered with a strong eruption attended with intense heat. Thinking it impossible that the insect could have produced this inflammation, I sent for a doctor. After examining the skin he assured me he could see no other cause, and that the eruption resulted from the hairs of the caterpillar remaining in the skin.

He ordered me some simple applications, telling me that a few hours would bring relief. In this he was totally mistaken. The inflammation increased to the extent of producing general fever; I passed a sleepless night, and the next day it continued unabated. After that it very gradually subsided, but the traces of the eruption were visible ten days afterwards.

The insect could not, I imagine, have bitten me, as I felt nothing at the moment.

I have frequently been bitten by tropical insects, but in no one instance have I suffered so severely, or been so disfigured. The sensation reminded me somewhat of the prickly heat, only it was infinitely more intolerable.

There was no predisposing cause, as I was at the time in good health, and had no tendency to fever, although the temperature was remarkably high for the month of June.

I have not seen a similar accident during my fifteen years' residence in France, but I presume they are not unfrequent here, or there would be no reason for the vulgar French expression "Mauvaise comme une chenille." A. GILLANDER

7, Rue St. Claire, Passy, Paris

The Glacial Period

PROF. TYNDALL has several times called attention to a point in regard to the height of the snow-line, which seems to be steadily overlooked by those who speculate on the causes of the great prevalence of snow during the glacial epoch. It is of course well known that the height of the snow-line at any place is determined mainly by two things, viz., the depth of annual snow-fall, and the temperature of the place. If the amount of snow falling over the whole earth is to be increased, the evaporation must also be increased. ("Heat as a Mode of Motion," pp. 206–7. New York, 1866.) This would also raise the temperature, but the snow-line might nevertheless descend. We have a case of exactly this kind in the Himalayas. On the warm southern side of these mountains the snow-line is, nevertheless, 3,000 ft. lower than on the northern side, where the temperature is very much colder. This is evidently due to a difference in the amount of annual precipitation. Assume that the sun was at one time much warmer than now, and that since then it has been steadily cooling; and I believe you have the key to the solution of the questions asked by J. H. Rohrs, as well as to such questions as the widespread occurrence of tropical vegetation during the past ages.

Iowa City, U.S. FRANK E. NIPHER

RECENT RESEARCHES ON THE LOCALISATION OF THE CEREBRAL FUNCTIONS

THE fifth part of Dr. Brown-Séquard's new "Archives of Scientific and Practical Medicine" contains an excellent report by Dr. Neftel, "on some of the recent researches in neuropathology" embracing a digest of several important modern methods, recently introduced, for the purpose of analysing the functions of the different parts of the cerebral hemispheres, together with a succinct account of the results arrived at by their employment. An abstract of this report forms the substance of the present notice.

The researches of Longet, Magendie, Matteucci, and others have led to the assumption by most physiologists, that the cerebral hemispheres, especially their cortical substance, are destitute of sensibility, being the seats of origin of higher mental phenomena only. The experiments from which these conclusions were arrived at, consisted in the irritation of the hemispheres in living animals by mechanical, chemical, and electrical means; and in none were they succeeded by muscular contractions. As if to put the question beyond a doubt, Flourens removed the entire hemispheres without disturbing the muscular mechanism.

But the tendency of modern observation is in a different direction; the new researches have been made independently by several investigators, with entirely different methods, nevertheless the results are the same, contrary to that of the earlier workers; the evidence going to prove that the cortical substance of the cerebral hemispheres is in close relation with certain muscular groups, forming the "psychomotor centres" of Gudden.

Fritsch and Hitzig commenced these researches, the latter having observed that galvanic excitation of the hemispheres in the living man produced contraction of the eye-muscles. This aberrant result suggested further experiments. They irritated the cerebral hemispheres in a dog with an extremely weak current, and found that movements of certain groups of muscles followed the excitation of definite spots on the anterior convex portion of the brain, always upon the side opposite to that which was acted on; whilst the same excitation of portions of the hemispheres situated more posteriorly, produced no effect. Thus they found the centre for the extensor and adductor muscles of the anterior extremity at the external end of the pre-frontal convolution; and somewhat behind it the centre for the flexor and rotating muscles of the same extremity. The irritation of these centres by metallic closing of a very weak galvanic current produces a single contraction, whilst the interrupted current produces tonic and gradually disappearing contractions of these muscles, followed by epileptiform movements. The anode has much more influence in producing these results than the cathode, so much so, that with a current of minimal intensity contractions can only be produced by the anode.

When Fritz and Hitzig removed in dogs the centre for the anterior extremity, this latter did not become entirely paralysed, the animal could use it, but imperfectly, and seemed quite unconscious of the condition of the limb, which could be placed into any position without attracting its attention.

Nothnagel employs a new method for the determination of the functions of the brain. His observations are made mostly on rabbits. An incision is made in the scalp, the skull is perforated with a needle. Through the canal thus formed in the bone a very small drop of a concentrated solution of chromic acid is injected by means of a hypodermic syringe with a very slender nozzle. The scalp wound is then united by suture, and the animal does not seem to be affected, except with regard to the functional derangement incidental to the lesion. Generally they survive the operation two or three weeks, and die from causes which Nothnagel cannot explain, no constitutional symptoms being developed. However, when the chromic acid is injected into the lateral cerebral ventricles death is the immediate result. On post-mortem examination, where the chromic acid was injected a minute circumscribed place appears, of a green colour, resistant and hard.

In methods employed previous to this, many causes acted to impair the value of the results arrived at; there was considerable hæmorrhage, refrigeration of the brain surface, and modification of the intra-cranial pressure, in addition to which the animal died very shortly. These are obviated by the new means just described; many fresh facts have therefore been brought to light. In one of his experiments Nothnagel made a chromic acid lesion on the surface of the cerebral cortex, which penetrated very slightly into its substance, in a spot corresponding exactly to the outer end of the post-frontal convolution. The animal appeared healthy, but it was found on careful observation that it had lost the muscular sense in the anterior extremity, on the opposite side to the cerebral lesion, it being possible to put and retain for some time, the affected paw in strained positions. This condition passed off before death, which seems to indicate that the terminal station or the real centre for the muscular sense exists elsewhere, and that after a time other ways to it become developed.

Nothnagel found, further, a circumscribed locality in the cerebral cortex, the lesion of which produces a partial and transient hemiplegia of the opposite extremity. This spot is in front of that for the muscular sense, and deeper than it. In no other portions of the cerebral cortex, except those above mentioned, have the chromic acid lesions been followed by paralytic symptoms.

Gudden has introduced another method by which the function of the different parts of the cerebrum may be studied. He finds that newly-born animals, as rabbits, will undergo a very great amount of mutilation without interfering seriously with the nutritive functions, so that portions of the brain may be removed, and the animal will grow to full size, with no peculiarities excepting those resulting from the absence of the parts removed. The slight sensibility, rapid coagulation of the blood, and the quick growth, are all in favour of operations. The following are the results of his experiments on the cerebral hemispheres :—"Very convincing facts are obtained by removing the cerebral hemispheres in new-born animals, and allowing them to grow up. The result is idiotismus. There is also reason to locate the organic conditions of voluntary movements in the cortical substance of the brain, but there is no reason to accept the corpus striatum as a motor ganglion. The hemiplegion following the destruction of the nucleus lenticularis can be satisfactorily explained by the rupture of fibres passing through the internal capsule. But admitting the cerebral cortex as the organ for voluntary movements, there is no necessity to have another motor ganglion. Indeed, Gudden's experiments on new-born rabbits, by removing portions of the hemispheres, have demonstrated that the organ of voluntary motion is located in the frontal part of the cerebral cortex."

Dr. Ferrier, whose results are referred to in another column, is working in a similar field of observation, with the view of elucidating the relations between certain convolution centres, and definite sets of muscles at the periphery.

FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE second meeting of the French Association for the Advancement of Science was held at Lyons from the 21st to the 28th of August, under the Presidency of Prof. Quatrefages. This Association bids fair to become as popular in France as the British Association in this country. The work done in the sections which I visited, those of Anthropology and Geology, was, to say the very least, as valuable as that done by our own sections. Among the papers brought before the former, the pleistocene station of Solutré excited considerable interest, and was subsequently visited by the section. The site has been used by man for habitation and burial, as late as the Merovingian times, in which it was a cemetery, and the strata are to a considerable extent *remanié*. The association of remains on that spot of varying age, Palæolithic, Neolithic, and Frankish, seems to throw a doubt on the precise date of the human skeletons, buried at full length, and generally believed to be of the same age as the associated implements of reindeer, and bones of mammoth. Dr. Gosse also read a paper on the reindeer-cave of Veyriers, Switzerland, and exhibited carved implements of reindeer antler, usually called "batons de commandement," which are of the same form as the arrow-straighteners of the Eskimos. Here, as in the caves of Belgium explored by M. Dupont, they presented but one perforation. The debates were very animated, and drew out many valuable remarks from the eminent anthropologist, Dr. Paul Broca.

In the Geological section, papers were contributed by the Comte de Saporta, MM. Dumortier, Beboux, and others, and in the debates Prof. Carl Vogt of Geneva took a prominent part. MM. Falsan and Chantre exhibited and described an elaborate map of the glacial phenomena of the middle basin of the Rhone, drawn on a large scale. They traced the glaciers of the Alps, and of the Jura, as far to the west as the Saône, and as far to the south as Valence; and they proved that there were two epochs of glaciation, the one during which the area in question was covered by a great ice-sheet, conveying Alpine blocks over the Jura into the valley of the Saône and middle basin of the Rhone, and the other during which the glaciers were isolated, and local moraines accumulated in the river valleys. These two periods correspond with those which have been noted in Great Britain and Ireland, by Prof. Ramsay, Hull, and others. The map presented a combination of artistic skill, with careful work in the field, which is very rarely met with.

In the evening three popular lectures were given to the public, one of which, by M. Janssen, on the Constitution of the Sun, was admirably illustrated.

The times of meeting of the sections differ from ours, the programme of the day being, first, a morning sitting from 8.30, or 9 to 11 A.M.,—*déjeuner*; and, an afternoon sitting from 3 to 5 P.M.—then dinner; and sometimes an evening sitting commencing at eight, when there were no lectures going on. The sections were 15 in number, and comprised Agriculture and Medicine, as well as those represented in the British Association. There were excursions down the Rhone, and to Geneva; a grand *fête* given by one of the merchants, and a magnificent entertainment given by the City of Lyons in the Town Hall.

In writing this short notice the extreme courtesy and consideration of the French Association to the strangers should not be omitted. Their hospitality to the only English guest present was too great to flow from any personal motive, and evidently was intended as a mark of respect to the British Association. W. B. D.

THE METEOROLOGICAL CONGRESS AT VIENNA

THE Meteorological Congress which met at Vienna during the past month worked very hard amid many difficulties, and we believe will have good results. The Congress sat from Sept. 2 to Sept. 16. The protocols and appendices are in the press, and will appear officially in French and German, while Mr. R. H. Scott has undertaken an English translation, which will appear as soon as possible. The following is a list of the delegates from the various countries:—Antonio Aguilar, Spain; H. Buys-Ballot, Netherlands; Carl Bruhns, Germany, Alexander Buchan, Great Britain and Ireland; J. D. Campbell, China; Giov. Cantoni, Italy; Aristide Coumbary, Turkey; v. Czelechowsky, Austria; F. Doergens, Germany; Prof. Ebermayer, Bavaria; Fradesso da Silveira, Portugal; M. Gloesener, Belgium; Julius Hann, Austria; Hoffmeyer, Denmark; Carl Jelinek, Austria; Josef Lorenz, Austria; Heinrich Mohn, Norway; Robert Muller, Austrian-Hungary; Albert Myer, United States; Georg Neumayer, Germany; E. Plantamour, Switzerland; Ernst Quetelet, Belgium; R. Rubenson, Sweden; Guido Schenzl, Hungary; Julius Schmidt, Greece; H. Schoder, Germany; Robert H. Scott, Great Britain and Ireland; Carl Sohncke, Germany; H. Wild, Russia; F. Winnecke, Germany; A. Zamara, Austria. The following is the programme of subjects discussed —

I. *Instruments.*—1. What is the construction of the barometer most suitable for stations of the second order? Is the use of aneroids at such stations advisable? 2. What model of exposure of thermometers for the observation of air temperature is the best and most suitable for general adoption? 3. What is the best construction for maximum and minimum thermometers? 4. What instruments should be used for determining intensity of radiation, and in what way can the comparison of the results obtained be secured? 5. What is the best apparatus for observing earth temperatures? At what depths ought they to be made, in order that the desired agreement may be attained? 6. What instruments should be used for ascertaining the state of moisture of the atmosphere? Does the psychrometer suffice for this purpose? Can the hair hygrometer be made applicable, and with what limitations? 7. In what way can an agreement in the signs for the directions of the wind be attained? Is the deduction of the mean direction of the wind according to Lambert's formula desirable? Is it desirable or not to include very light winds (force 0) in constructing wind roses for the direction of the wind? 8. What scale is to be used for the force of wind where it has to be estimated without the aid of an instrument? 9. Is the introduction of simple counting instruments for ascertaining the rate of the wind desirable? What units should be fixed upon as a basis for observing the rate of the wind? 10. What is the most suitable form, size, and position for rain-gauges? At what time of day should the measurement of rainfall be made. 11. Should days of rain and snow-fall be separated from each other, or be counted as the same? 12. Is it desirable in recording the amount of hail to separate the falls of sleet (*graupel*) from those of hail proper? 13. In reckoning thunderstorms, are the storms only to be recorded, or the days in which they occurred? How is sheet-lightning to be regarded? 14. What apparatus is to be recommended for measuring evaporation? What is the most suitable exposure for the vaporimeter? 15. How should the amount of cloud be estimated and recorded? Is it desirable to introduce for clouds, hydrometeors, and for other extraordinary phenomena, a nomenclature which shall be independent of local language, and therefore universally intelligible? 16. Moreover, should other elements which are reckoned meteorological, *e.g.* atmospheric electricity, ozone, &c., be included in the circle of normal observations, and what are the most suitable instruments for observing them. 17. For meteorological measurements should the same units of measure (units of length, degree, time, &c.), be introduced into all countries? or is it sufficient to establish fixed rules for the reduction of the measurements used in different countries?

II. *Taking and calculation of the observations.*—18. Could corresponding times of observation be established at all meteorological stations. 19. According to what rules, periods of time, &c., are the mean values of the various meteorological observations to be calculated? Is it expedient to begin the meteorological year with the month of January, or with the month of December? 20. In what way, and for what periods of time are the normal values of the several meteorological elements to be deduced?

III. *Weather telegrams.*—21. Does the interchange of weather telegrams appear so useful that a wider circulation and more complete organisation should be given to it?

IV. *Maritime Meteorology.*—22. In what way would maritime meteorology be best introduced into the system of general meteorology?

V. *Organisation.*—23. Is it desirable that in each country one or more central stations for the superintendence, collection, and publication of meteorological observations, should be established? 24. In reference to the verification of instruments and the inspection of meteorological stations, can any adequate general rules be laid down? And is it advisable to introduce general instructions for taking and calculating meteorological observations? 25. In what way can the agreement of the standard instruments of the various central establishments be best secured?

VI. *Publication of Observations.*—26. Is it desirable and practicable to publish the meteorological observations of a limited number of stations in each country in a uniform manner and within a reasonably short time after the observations have been made? 27. How is the interchange of meteorological publications of various institutions and countries to be organised most simply, speedily, and certainly?

VII. *The Carrying Out of the Decisions of the Congress.*—28. What measures should be adopted for the accomplishment of the decisions and purposes of the Meteorological Congress? For this purpose, is the establishment of a permanent committee and the arrangement of further meteorological Congresses necessary?

BIRMINGHAM NATURAL HISTORY AND MICROSCOPICAL SOCIETY

ABOUT twenty members of this society, including several ladies, proceeded to Teignmouth in the beginning of September, in fulfilment of the proposed marine excursion, and took up their quarters according to agreement at the Queen's Hotel. The yacht *Ruby* had been chartered for the occasion, and proved a most seaworthy and serviceable craft. Dredging operations commenced on Monday, Sept. 1, and were continued daily throughout the week, in depths varying from 5½ to 20 fathoms. The atmospheric, surface, and bottom temperatures were taken at each sounding, the maximum and minimum results being as follows:—

Atmospheric temperature,	Maximum			66°	Minimum	64°
Surface	"	"	"	61°	"	58½°
Bottom	"	"	"	60½°	"	58°

The averages were: atmospheric, 65¼°, surface, 59½°, bottom, 58¾°. A Miller-Casella thermometer was used. On the whole the results of the dredging were very satisfactory. The weather was fine, but cloudy, with occasional rain, and sometimes a little too calm for the work. About 30 hauls of the dredge were made, and specimens of many of the marine invertebrate animals in the neighbourhood secured. The tangles attached to the bag of the dredge sometimes came up literally swarming with echinoderms. By far the most noteworthy capture was *Comatula rosacea*, the feather-star, two individuals of which were taken in the larval pedunculate condition attached near the base of a frond of *Laminaria*, which was torn off by the dredge.* The specimens measured about one-third of an inch each in length. Five young Comatulæ in a free condition, the largest about an inch across, were also taken. A subsequent haul on the following day brought up from the same locality three adults. The members of the Society had the unusually rare opportunity of seeing under the microscope the young feather-stars in the living state. They were but little thicker than sewing-silk, of graceful, erect, lily-like form, and very lively, bending and waving on the peduncle; the arms vigorously contracting in an inward direction. Drawings of the larval Comatulæ in the living state were made to scale by Mr. Wills, with the camera lucida, and the specimens mounted by him for exhibition to the Society. A full description will be communicated to the Society in a report of the excursion. During the evenings the members had the opportunity of examining under the microscope the pedicellariæ of the star-fishes and sea-urchins, and the whip and bird's head processes of certain of the polyzoa, also the structure of Botryllus and other tunicates, the larval forms of crustacea, &c; objects always interesting, but specially so to a society carrying on its work in an inland neighbourhood far removed from the sea. In the course of the week very enjoyable excursions were made by some of the members down the River Dart to Berry-Pomeroy Castle, Lustleigh, Becky Falls, Moreton Hampstead, Chagford, Exeter, Torquay, &c. On the whole, the excursion has proved a most successful experiment, quite fulfilling the expectations of those who projected it, and it is to be hoped may be succeeded by others in a wider field. The members received much kind attention from the Rev. R. Cresswell, Mr. W. G. Ormerod, Rev. R. C. Douglas, Mr. Adams, and other gentlemen. Most of the party returned to Birmingham by train on Monday, having had a most delightful excursion.—The members of the society who remained in Devonshire after the marine excursion had a great treat on the following Friday, when they were escorted through the famous cavern by W. Pengelly, F.R.S., who courteously explained to them the mode of conducting the explorations, the contents of the flora, and their relation to geological time. Mr. Pengelly also showed them at his own house the collection of bones, teeth, &c., of man, and the extinct bear, hyæna, dog, and other animals, and the flint implements of earlier and later manufacture found therewith in the cavern.

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* They were taken in the vicinity of Torbay on Thursday, Sept 3, at a depth of 12 fathoms on a limestone bottom, the bottom temperature registering 59°.

THE COMMON FROG

I.

WHAT is a Frog? At first, almost all persons will think, on meeting with this question, that they can answer it readily and easily. Second thoughts, however, will show to most that such is by no means the case.

Indeed many a man of education and culture will find himself entirely at a loss, if suddenly called upon for a reply to what is in fact a problem by no means easy of solution.

"The Frog is a small saltatory Reptile" will probably be the reply of the majority. But *is* it a Reptile? At any rate it begins life (in its Tadpole stage) like *a Fish!*

By the great Cuvier, however, as by very many naturalists since, it has been regarded as a Reptile and classed with Lizards, Crocodiles, and Serpents; and yet it may be a question whether the murine affinity connubially assigned to it in the Nursery tale, be not the lesser error of the two.

If the Frog was only known by certain fossil remains it would be considered one of the most anomalous of animals.

Many persons are accustomed to make much of the distinctive peculiarities of the human frame. In fact, however, Man's bodily structure is far less exceptional in the animal series, is far less peculiar and isolated than that which is common to Frogs and Toads.

The number and nature of both the closer and the more remote allies of the Frog; its distribution both as to space and as to time; its relationships whether of analogy or affinity[*] to very different animals; its bony frame-work; its muscles and nerves; its brain and sense-organs; its respiratory and excretory structures; its various changes from the egg to maturity, together with peculiarities of habit in allied forms; are all matters which will well repay a little attentive consideration.

Indeed it is probable that no other existing animal is more replete with scientific interest of the highest kind, than is the Frog.

About it are gathered biological[†] questions which bear upon the origin of species, and upon the course and mode of organic development, as well as other speculative problems to which answers are as yet far to seek.

If it is a fact that all the various species of animals have arisen through ordinary generation one from another by a process of development, the life history of the Frog may with reason be expected to have some bearing upon such a process, since every Frog begins its free existence with the organisation of a Fish, and after undergoing a remarkable "Metamorphosis," attains the condition of an air-breathing quadruped, capable of easy and rapid terrestrial locomotion.

There is a matter with respect to which the zoologist can hardly avoid regarding the botanist with envy. The creatures sought after by the latter may be rare or inhabitants of stations difficult of access, but at any rate they are incapable of flight or concealment, and specimens of some kind or other generally present themselves in plenty.

On the other hand not only does the townsman of a thickly-peopled land like our own, often meet with fewer animals in his country walks than he anticipated, but the explorer of tropical lands and virgin forests has frequently to endure disappointment from the contrast between the richness of a known local fauna and the little to be actually seen of the animal population of the place.

Frogs and Toads, however, are often enough seen both at home and abroad, and when perceived generally fall a far more ready prey to the collector than do the swift-running Lizards and small Beasts which are the commonest ground-animals met with besides. The group is also rich in species as well as in individuals, and it is spread over the far greater part of the habitable globe. Nevertheless Frogs and Toads have few admirers even amongst professed zoologists, and meet with no little neglect.

While the term "Ornithologist"[‡] is familiar to everyone, and the title "Erpetologist"[§] is so to all naturalists; the name "Batrachologist"[‖] has not yet been conferred on or assumed by any one worker in Science.

Economically, Frogs are of little esteem in England save occasionally for bait and as the staple food of certain rare and interesting animals preserved in our menageries. Our American cousins indeed have given one more evidence of their French sympathies by the introduction of the Frog into their *cuisine*, and, as suits that land of the longest rivers and the largest lakes, it is no less a creature than the gigantic Bull-frog which figures in the *menu* of Transatlantic gourmets.

If zoologists and economists have neglected the Frog, the same assertion can by no means be made with respect to physiologists.

The Frog is the never-failing resource for the physiological experimenter. It would be long indeed to tell the sufferings of much-enduring frogs in the cause of Science! What Frogs can do without their heads? What their legs can do without their bodies? What their arms can do without either head or trunk? What is the effect of the removal of their brains? How they can manage without their eyes and without their ears? What effects result from all kinds of local irritations, from chokings, from poisonings, from mutilations the most varied? These are the questions again and again addressed to the little animal which perhaps more than any other deserves the title of "the Martyr of Science."

To return to our question at starting, "What is a Frog?"

To answer this, it will in the first place be well to make a certain preliminary acquaintance with the frog absolutely.

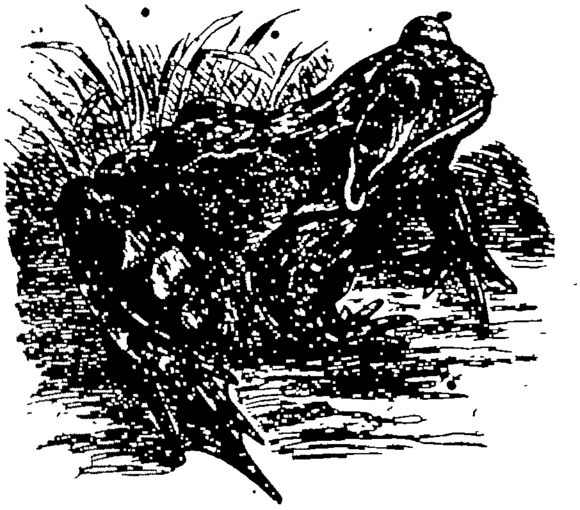

FIG. 1.—The Common Frog, *Rana temporaria*.

Secondly, to study those creatures which are most like it, and are, therefore, as we shall directly see, its "class fellows," living and fossil.

Thirdly, to investigate its anatomy so far as to be able to institute fruitful comparisons between its organisation and that of all other creatures belonging to the same great primary group of animals to which it pertains.

Fourthly, to sum up the results in a series of successively wider and wider comparisons, and by the light thence derived to answer as fully as the present state of Science allows the question first asked.

We shall then be able to answer that question, because we shall have ascertained how various parts of this creature form one organic whole as a system of mutually related structures; and how this whole and its parts are related to the entire series of animal existences from the monad up to man. Then, and then only shall we be able to say what a frog is.

In the first place it is necessary to acquire a general notion of the way in which animals are distinguished and segregated into groups, as well as the general system of arrangement of those groups and the mode of bestowing names which has been adopted by zoologists in common with botanists.

When we have acquired an adequate general notion of zoological classification we shall be able to see with what creatures the Frog is now admitted to be, in various degrees, allied.

The whole mass of animals of all kinds from man down to the lowest animalcula) is spoken of by the fanciful term *king-*

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[*] Analogous relationship refers to the uses to which parts are put. Relationship of affinity refers either to such a relationship as that of kindred or to an idea. affinity reposing on similarities of structure.

[†] Biological questions are questions referring to living beings. "Biology" being the science which treats of all living things, including both plants and animals.

[‡] Ὄρνιθος, a bird, and λόγος, a discourse.

[§] Ἑρπετόν, a reptile, and λόγος.

[‖] Βάτραχος, a frog, and λόγος.

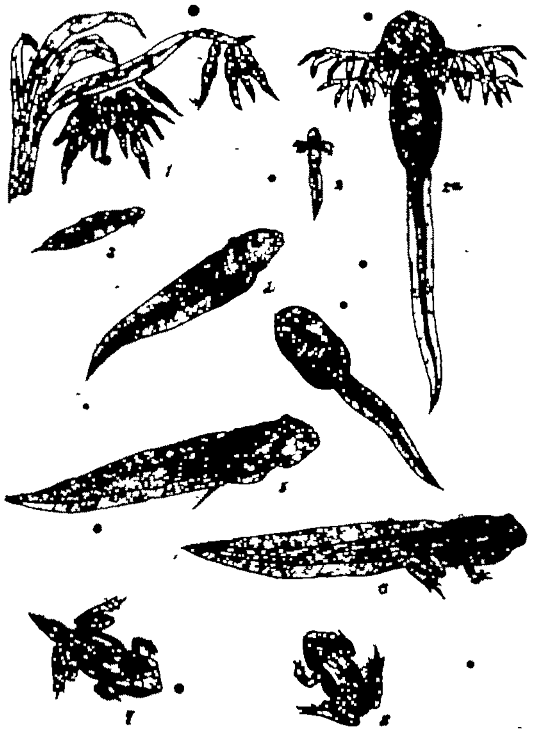
dom. Thus we have the animal kingdom in contrast with and in distinction from the vegetable and mineral kingdoms.

This great whole, the animal kingdom, is subdivided into seven great groups or *sub-kingdoms*, to one or other of which every animal known to us belongs.

Each of these sub-kingdoms (like every more subordinate zoological group) is characterised and defined by certain points of structure possessed by the animals which compose it and which serve to distinguish them.

Thus, if we take up an earthworm we see that its body is composed of a series of similar segments or rings placed one behind the other, and we know that it belongs to that great sub-kingdom of ringed animals termed *Annulosa*.

If we examine a thousand-legs or a wood-louse we see that here again the body is evidently composed of a series of rings or segments, to most of which jointed legs are attached. A successive survey of a lobster, a scorpion, a bee, a beetle, or a butterfly will reveal to us that all these creatures, however different in other respects, all belong to the same ringed type, *i.e.*, that they are all members of the sub-kingdom *Annulosa*, which contains all such animals, all insects, together with spiders, earthworms, and leeches.



G. 2 —Tadpoles in different stages of development, from those just hatched (1) till the adult form is attained (8).

Another great sub-kingdom called *Mollusca* contains all snails, slugs, cuttle-fishes, and creatures of the oyster and scallop class. Such animals have not the body composed of a series of similar segments, but are united by characters less obvious indeed, but as distinctive.

A third sub-kingdom called *Molluscoida* is made up of the sea-squirts, or Ascidians (sometimes called Tunicates) and lamp-shells, together with minute animals living in water in compound aggregations, like the *Flustra* (or Sea-mat) so common on our coasts, the surface of which is pitted with minute depressions, in each of which a minute animal had in life its abode—as doves in a dove-cot, if we imagine each fastened in its cell by natural growth.

A fourth sub-kingdom, *Annuloida*, is composed of such animals as star-fishes and sea-urchins, together with internal parasites (tape-worms, &c.) and their allies.

The fifth sub-kingdom is named *Cœlenterata*, and contains all sea-anemones, jelly-fishes, Portuguese men-of-war, polyps, and coral animals, these being the little creatures which have formed the atolls (or coral islands) of southern seas, and the vast reefs which stretched for so many hundred miles on the earth's surface.

The sixth sub-kingdom, *Protozoa*, comprises the Sponges, the Infusoria, and all the lower forms of animal life.

Now the whole of these six sub-kingdoms may be contrasted with the last and seventh, which bears the name *Vertebrata*, from which they all differ in several important particulars, and therefore they are often spoken of by the common and convenient term *Invertebrata*.

When we examine a fish (such as a sole, a herring, or a mackerel), one of the first things likely to be noticed by us on dividing it, is a solid structure—the backbone—extending from the head to the tail, and coated externally by the flesh.

This backbone is soon seen to be made up of a number of pieces jointed together. Each piece is called in natural history a *vertebra*, and every animal in which such a structure is found, is called, on that account, a Vertebrate animal.

Now every kind of beast and reptile agrees with these fishes in the possession of the vertebrate backbone, as well as in a variety of other important characters, which constitute the definition of the sub-kingdom *Vertebrata*.

Thus in the development of the egg of every Vertebrata (such *e.g.* as in that of the fowl), the first indication of the future animal, is the appearance on part of its surface of a minute longitudinal furrow called the *primitive groove*. Next the margins of this groove ascend to meet together above, thus enclosing a canal, the lining of which becomes thickened and transformed into no less important a structure than the brain and spinal marrow.

Concomitantly with the development of this canal, there is found, immediately beneath it, a little gelatinous rod enclosed in a membraneous envelope, and called the *notochord*, or *chorda dorsalis*. It is this structure which is subsequently developed and becomes the backbone.

Another singular condition is invariably presented in the development of every vertebrate, whether the structures formed are transitory or permanent.

This condition is the appearance of a certain series of openings formed at the side of the neck, and which, in fishes, remain permanent as the gill openings. These openings are termed *visceral clefts*, and lead from the exterior into the throat. The solid pillars (or intervals) between the clefts are called *visceral arches*, and in creatures (*e.g.* fishes) which develop gills upon them, *branchial arches*.

In all vertebrates again (unlike insects or spiders) there are never more than four limbs, and these are supported by bones, or cartilages, which are clothed externally with flesh, and are not moved by muscles placed *within* the hard parts, as is the case with lobsters, insects, and all their allies.

The heart in all vertebrates, consists of at least two distinct cavities, and sends forth blood into a system of arteries, thence it is brought back again to the heart by other vessels termed the veins. On its way back to the heart, however, some of the veins carry blood to be redistributed in the liver, forming what is called the *portal* circulation.

In all the points above enumerated, the Frog (as we shall shortly see) fully agrees with beasts, birds, reptiles, and fishes, and thus shows that it differs from the immense majority of animals—the *Invertebrata*—and pertains unmistakeably to the seventh sub-kingdom of animals—the *Vertebrata*.

Now every sub-kingdom of animals is further divided into a greater or lesser number of subordinate (though still large) groups, termed *classes*. Each class is again subdivided into a certain number of smaller and more subordinate groups, each of which is termed an *order*. Each order is made up of *families*, each family being of course, smaller, and more subordinate than an order. Every family consists again of still more subordinate groups, each of which is termed a *genus*. And every genus comprises one or more *species*.

In zoology, every animal bears a name composed of two words. The first of these is a substantive, and denotes the genus to which any given animal belongs. The second word is an adjective—or a word used in an adjective sense—and denotes which species of the genus that given animal is. Thus the Chimpanzee is called *Troglodytes niger*, it is the species *Niger* of the genus *Troglodytes*, which genus contains also another species, namely, the Gorilla.

St. George Mivart

(*To be continued.*)

NOTES

IT would be well if our men of science were to be found more frequently distributing prizes and taking an interest in the schools in which, thanks to the wisdom and energy of Mr. Cole, so many thousands of our people are learning science. In this Prof. Williamson has just set a good example by distributing the prizes at the Keightley School of Science and Art on Thursday last. Prof. Williamson, at the end of his speech, remarked that "We in this country give a peculiar position to Science in relation to material affairs. If we find a coal-seam we look upon it as wasteful not to work it and make the most of it, but what he said was, that to leave the clear heads and true hearts of our countrymen left useless was a greater waste, because he believed that they were infinitely more valuable than any coal-seam that ever was discovered."

AN anonymous donor has placed a large sum in the hands of the Committee of the Birmingham and Midland Institute, for the foundation of a Lectureship on the Laws of Health, and also for a prize fund in connection with the class. Dr. Corfield has been offered the post for this year, has accepted it, and will deliver an inaugural lecture in the Town Hall, Birmingham, on Thursday, October 9, at 8 P.M., on "Sanitary Progress." The course will begin on Tuesday, October 14, at 8 P.M., and be continued on succeeding Tuesdays until some time in April. It is intended more especially for the working classes, and both men and women will be admitted.

THE programme of the Birmingham and Midland Institute for Session 1873–4 is a very full one, and, to judge from what is set down, is well organised in its departments, and doing a thoroughly good educational work among all classes of the populous and important district in the midst of which it is established. At a merely nominal fee it places valuable scientific instruction within the reach of the poorest artisan.

SIR SAMUEL and Lady Baker left Alexandria for London on Tuesday.

WE would draw our readers' attention to a letter from Professor Thiselton Dyer, in this week's number, on the Oxford Fellowships in Science about to be competed for. We hope that, at any rate, the matter of Research will be taken into consideration.

NEXT year's meeting of the American Association for the Advancement of Science will be held at Hartford, Conn., and the officers elect are:—President, Dr. John L. Le Conte, of Philadelphia; Vice-President, Prof. C. S. Lyman, of New Haven; Gen. Sec., Dr. A. C. Hamlin, of Bangor; Treasurer, Mr. W. S. Vaux, of Philadelphia.

THE Italian Association for the Advancement of Science meets on the 20th inst.

THE business of the Social Science Congress opened at Norwich yesterday, with a meeting of the Council, after which there was a special service in the Cathedral; and in the evening the inaugural address was delivered by the President. To-day the exhibition of sanitary and educational apparatus and appliances at the Drill Hall, kindly lent for the occasion, will be opened with an address by the High Sheriff of Norwich. The address of the President of the Council, Mr. G. W. Hastings, will follow, after which the departments will meet in their respective rooms, and in the evening a *soirée* will be given by the local Executive Committee in St. Andrew's Hall. On Friday morning Mr. Joseph Brown, Q.C., will deliver his address as president of the Department of Jurisprudence and Amendment of the Law; and after the meetings of the various departments for the reading and discussion of papers, a working men's meeting in St. Andrew's Hall, at which the Mayor will preside, will conclude the business of the day. On Saturday an address on education will be delivered by Prof. W. B. Hodgson, LL.D., and after the rising of the departments the President of the Congress will distribute the certificates and prizes to the successful candidates at the last Cambridge middle-class examination. The address of Capt. Douglas Galton, C.B., F.R.S., president of the Health Department, will be given on Monday morning. The departments will meet as usual in their respective rooms, and in the evening a grand concert will be given in St. Andrew's Hall. Mr. Thomas Brassey, M.P., will deliver his address on Economy and Trade on Tuesday, and after the business of the departments a *soirée* will be given in St. Andrew's Hall by the Mayor, and the concluding meeting, preceded by a meeting of the Council, will be held on the Wednesday. In connection with the Congress there will be a conference on female education, and in the Exhibition short addresses will be delivered daily in the afternoon on the subject of the articles exhibited in the various classes. Excursions to various places, it is understood, are being arranged.

THE *Diana*, screw steamer, in which Mr. B. L. Smith left Dundee in May last on a voyage of discovery to the Polar Seas, by the Spitzbergen route, arrived in Dundee on Saturday last. The *Daily News* sums up the voyage of the *Diana* as follows:—A succession of gales was experienced—the weather on almost all occasions when the ship was in the open sea being such that, although she was provided with complete apparatus for sounding, deep-sea temperatures, &c., not nearly what was intended has been accomplished. Owing to the unfavourable nature of the ice, little in the way of exploration has been possible. The time had, however, been very fully occupied in dredging, trawling, photographing, surveying, and making as complete and perfect collections as circumstances permitted of the flora of Spitzbergen. Specimens of rare birds have been secured, and collections made, probably the first of any value. The collections of marine plants and animals are likely to prove especially interesting, and it has been discovered, among other things, that some parts of those seas hitherto reported as almost destitute of fish, abound in cod of excellent quality. In the way of geology everything possible was done in the parts unexplored by the Swedes, and numerous specimens of fossils have been brought back from the hitherto unvisited parts of the coast of the north-east land. From the appearance of open water seen in this expedition beyond Cape Platen, and also reported by the Swedes as existing—ascertained during their sleigh journey—it seems to be by no means certain that the route farther northwards which the *Diana* on leaving England hoped to reach does not exist, and the question still remains open, were it possible to reach this early in the season, whether a means of reaching a higher latitude to the north-east of Spitzbergen is not available. Mr. Smith has ascertained that the North Cape is situated on an island separated by a sound from the main land, and to this extent a knotty point has been determined. The expedition never got beyond 81°, while Mr. Smith in his expedition of 1871 got to 81·24°. He states that the *Diana* behaved admirably, but he did not realise his anticipations which would be achieved by the substitution of steam for sailing power.

WITH reference to our announcement of the forthcoming work by Mr. Boyd Dawkins on Cave Hunting,—the new line of inquiry which has added so much to our knowledge of ancient man,—we may now state the work will comprise the physical history of caves and their relation to the general physical geography of the district, as well as the history of their contents; and will treat of the men who have inhabited the caves of France, Spain, and Britain, during the historic, pre-historic, and pleistocene ages. The subject bristles with problems ethnological, archæological, and geographical, and demands a careful criticism that will sift the certain from the uncertain. The evidence will be given from which it may be concluded that the Eskimos lived as far to the south as the Pyrenees in the palæolithic age, and that the Basque or Iberic population ranged as far north as the British Isles.

THE "Astronomical Observations taken during the years 1870—72, at the private observatory of Mr. Joseph Gurney Barclay, Leyton, Essex," by Mr. C. G. Talmage, contains well-arranged tables of double star observations, followed by copious notes on the observations, and occultations, and phenomena of Jupiter's satellites. Mr Barclay thinks it so advisable to reduce and print observations at short intervals, that he has determined, wisely we think, to adopt the plan without waiting for a number to form a large volume.

AMONG Messrs Smith, Elder and Co.'s announcements of forthcoming works, we observe the following:—A translation of Prof. "Hermann's Elements of Physiology," by Dr. Arthur Gamgee, and "A Text Book of Pathological Anatomy," by John Wyllie, M.D, Lecturer on General Pathology at the School of Medicine, Surgeons' Hall, Edinburgh.

AMONG Mr. Robert Hardwicke's autumn announcements we notice the following scientific books—"Man and Apes." an Exposition of Structural Resemblances and Differences bearing upon questions of affinity and origin, by St. George Mivart, F.R.S. This work will be published simultaneously in America and England. "Waste Products and Undeveloped Substances:" a synopsis of progress during the last quarter of a century at home and abroad, by P. L. Simmonds, the editor of the "Journal of Applied Science." "Where there's a Will there's a Way, or, Science in the Cottage," by James Cash; being an account of the labours and lives of some north-country botanists in humble life. "The British Hepaticæ," with descriptions by Dr. Carrington, and drawings by J. E. Sowerby. This will be issued in twelve monthly parts. "Hooker's Synopsis Filicum," a new edition brought up to the present time by J. G. Baker, Royal Herbarium, Kew. "On Mounting Microscopic Objects," by Thomas Davies. A new edition, much enlarged, by John Matthews, M.D., F.R.M.S. This last-named work is nearly ready for publication.

THE library of the Manchester Athenæum was destroyed by fire on Sept. 24. The damage, estimated at 10,000*l*., is said to be wholly covered by insurance.

WE have received the programme of the Edinburgh Veterinary College. We hope that, under the superintendence of the new Principal, Prof. Fearnley, this important institution will become more prosperous than it has ever been, and that the principles of the veterinary art will be taught in a thoroughly scientific way. That this is likely to be the case may be seen from the following list of professors:—Dr. Balfour, F.R.S., Dr. Murie, Mr. Dewar, F.R.S.E., Dr. Young, and Mr. Wally.

THE following are some of the most important recent additions to the Brighton Aquarium:—2 Octopus (*O. vulgaris*); 1 Group of Barnacles (*Lepas Hillii*); 30 Sea-horses (*Hippocampus ramulosus*); 5 African Crocodiles; 2 Alligator Terrapins (*Chelydra serpentina*), 1 Edible Turtle (*Chelonia midas*); 1 Sturgeon (*Acipenser sturio*).

THE additions to the Zoological Society's Gardens during the past week include a Brown Capuchin (*Cebus fatuellus*) from Guiana, and two Bonnet Monkeys (*Macacus radiatus*) from India, presented by Lord Louth; two Crested Ground Parakeets (*Calopsitta novæ-hollandiæ*) from Australia, presented by Miss L. E. Lyon, and two hatched; four Alpacas (*Lama pacos*), two Llamas (*Lama peruana*) from Peru, a Vicuna (*Lama vicugna*) from South America; a Cuvier's Gazelle (*Gazella cuvieri*) from Muscat; a Sultry Hemipode (*Ortyxelos meiffreni*) from West Africa; a Southern Mynah (*Acridotheres mahrattensis*) from S. India, deposited; a Philantomba Antelope (*Cephalophus maxwelli*) from Sierra Leone, received in exchange.

MOLECULAR EVOLUTION

AT quite uncertain times and places
The atoms left their heavenly path,
And by fortuitous embraces
Engendered all that being hath.

And though they seem to cling together
And form "associations" here,
Yet, late or soon, they burst their tether,
And through the depths of space career.

So we, who sat, oppressed with Science,
As British Asses, wise and grave,
Are now transformed to fierce Red Lions,
As round our prey we ramp and rave.

Thus by a swift metamorphosis,
Wisdom turns wit, and Science joke;
Nonsense is incense to our noses,
For when Red Lions speak they smoke.

Hail, Nonsense! dry nurse of Red Lions,[*]
From thee the wise their wisdom learn,
From thee they cull those truths of science
Which into thee again they turn.

What combinations of ideas
Nonsense alone can wisely form,
What sage has half the power that she has
To take the towers of Truth by storm?

Yield, then, ye rules of rigid reason!
Dissolve, thou too, too solid sense!
Melt into nonsense for a season,
Then in some higher form condense.

Soon, ah! too soon, the chilly morning
This flow of soul will crystallise,
And those who nonsense now are scorning
May learn too late where wisdom lies.

$\frac{dp}{dt}$

THE BRITISH ASSOCIATION

WE are glad to say that the attendance at the Bradford Meeting was considerably larger than was at first stated. The total number of persons who attended the meeting is 1,983, and the total amount received 2,102*l*.

The following is a list of the grants of money appropriated to scientific purposes by the General Committee:—

Mathematics and Physics	£	s.	d
Cayley, Prof.—Mathematical Tables ...	100	0	0
Cayley, Prof.—Printing Mathematical Tables ...	100	0	0
Glaisher, Mr. J.—Efficacy of Lightning Conductors (renewed) ...	50	0	0
Balfour Stewart, Prof.—Mauritius Observatory ..	100	0	0
Balfour Stewart, Prof.—Magnetism of Iron ...	20	0	0
Brooke, Mr. C.—British Rainfall ...	100	0	0
Glaisher, Mr. J.—Luminous Meteors .	30	0	0
Tait, Prof.—Thermo-Electricity (renewed) .	50	0	0
Williamson, Prof. A. W.—Testing Siemens' Pyrometer (renewed) ...	30	0	0
Chemistry			
Brown, Prof. Crum.—High temperature of Bodies (partly renewed) ..	70	0	0
Williamson, Prof. A. W.—Records of the Progress of Chemistry .	100	0	0
Gladstone, Dr.—Chemical Constitution and Optical Properties of Essential Oils .	10	0	0
Armstrong, Dr.—Isomeric Cresols and their Derivatives ..	20	0	0
Carried forward	£780	0	0

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[*] "Leonum arida nutrix."

Brought forward	£780	0	0
Geology			
Herschel, Prof.—Thermal Conducting Power of Rocks	10	0	0
Phillips, Prof.—Labyrinthodonts of the Coal Measures	10	0	0
Bryce, Dr.—Collection of Fossils in the North-West of Scotland	10	0	0
Wiltshire, Rev. T.—Investigation of Fossil Corals	25	0	0
Willett, Mr. H.—The Sub-Wealden Exploration...	25	0	0
Lyell, Sir C.—Kent's Cavern Exploration .	150	0	0
Harkness, Prof.—Mapping Positions of Erratic Rocks and Boulders	10	0	0
Woodward, Mr. H.—Record of Geological and Palæontological literature	100	0	0
Lubbock, Sir J.—Exploration of Victoria Cave ...	50	0	0
Biology			
Lane-Fox, Col. A.—Forms of Instruction for Travellers (25*l.* renewed)	50	0	0
Stainton, Mr.—Record of the Progress of Zoology	100	0	0
Jeffreys Mr. Gwyn.—Dredging off the Coasts of Yorkshire	30	0	0
McKendrick, Dr.—Physiological Action of Light	20	0	0
Brunton, Dr.—The Nature of Intestinal Secretion	20	0	0
Foster, Dr. M.—Methods of Breeding the Embryos of Delicate Marine Organisations	30	0	0
Statistics and Economic Science			
Houghton, Lord.—Economic Effects of Trades Unions	25	0	0
Mechanics			
Froude, Mr. W.—Instruments for Measuring the Speed of Ships and Currents (renewed) ..	50	0	0
	1,495	0	0
Widow of the late Mr. Askham (Clerk to the Association)	50	0	0
	£1,545	0	0

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICS

Report of the Luminous Meteor Committee of the British Association on Observations of Shooting-stars in 1872-73.

Shooting-stars and large fireballs have appeared during the past year in more than usual varieties. Large meteors have presented themselves in considerable numbers, and ordinary shooting-stars in a more striking manner as regards the explanation of their origin than has often been the case in former years. Of all these kinds of shooting-stars, both large meteors and meteoric showers, much accurate information has reached the committee; but the extent of the knowledge acquired on all hands, has at the same time advanced so rapidly, that a smaller amount of attention has this year been directed towards the discussion of the individual descriptions, than the committee have hitherto bestowed upon them, and a more complete reduction of the separate observations will accordingly be attempted when the opportunities of the committee allow of their closer examination.

Those meteors, however, which have been observed simultaneously at more than one observing station have been selected from the collection for transcription in suitable columns in this report, and a list of large meteors is added, among which some have occurred that have without doubt been noticed, and may have attracted attention in other directions, than has hitherto come to the knowledge of the committee. Two of the largest fire-balls seen in Great Britain were aerolitic, or burst with the sound of a violent explosion on November 3 and February 3 last. The first passed over the central part of Scotland, and the second burst over Manchester and its neighbourhood at half-past five, and at 10 o'clock respectively on the evenings of those days. Aërolitic meteors and aërolites have also been noticed in the scientific journals of other countries, which have given rise to experiments on the composition of aerolitic substances, both chemical and microscopical, the conclusions of which continue to extend the range of our speculations regarding the origin of these bodies. Thus the existence of carbon and hydrogen in the atmosphere from which the largest iron meteorite yet found (a few years since upon the shores of Greenland) was expelled, confirms the discoveries of Grahame and Professor Mallet, in America, of the existence of the same gases in other meteoric irons. Dr. Wohler has thus detected the oxides of carbon as gases in the vast meteoric iron of Ovifak found in Greenland, and brought to Stockholm during the last few years by Prof. Nordenskiold, and the same gas was found by Prof. Laurence Smith in the siderite which fell recently in the United States. A connection between comets and meteorites appears to be indicated by these discoveries, in the spectra of some of which gases containing carbon appear to have been certainly recognised by Dr. Huggins.

The past year was distinguished by the occurrence of a most remarkable star shower on the night of November 27 last, to the expected appearance of which astronomers were looking forward with especial attention from the unexplained absence of the double comet of Biela (to which it belongs) from its accustomed returns in the last three of its periodical revolutions. The probability of the comet's path being marked by a meteoric stream into which the earth might plunge on or about Nov. 27 every year was already become a certainty, by the observation of such a meteoric stream on Nov. 30, 1867. On that night M. Zezioli of Bergamo, observed a distinct star-shower, according to Schiaparelli, no doubt of whose belonging to the missing comet could be entertained. Although the exact date of the shower could not be accurately foretold with certainty from the want of recent observations of the comet, yet every probability of its being seen was favourable to its reappearance last year, and those who awaited it, as well as many unexpected watchers of meteor-showers, were surprised by the brilliant spectacle which it suddenly presented. At the first approach of darkness on the evening of Wednesday the 27th of November last, the cloudy state of the sky unfortunately deprived observers in the south of England from witnessing the sight, but in Scotland, and north of the Midland Counties of England many uninterrupted views of it were obtained. On the European continent and in the United States of America, as well as in the East Indies, at the Mauritius and in Brazil observers were equally fortunate in recording its appearance, and few great star-showers have hitherto been more satisfactorily observed, or indeed more abundantly described. In an astronomical point of view the agreement of the time and other circumstances of its appearance with the supposed path of the lost comet is so exact as to prove that the calculations made by astronomers of that comet's orbit cannot be affected by any errors of a large sensible amount, and a proof almost certain is thus obtained that the disappearance of the comet is owing to no unexplained disturbances of its path; but that like some former comets of variable brightness, it has not improbably faded for a time out of view, and that at a future time a reasonable expectation may be entertained of re-discovering it pursuing its original path in repeated visits to the earth's neighbourhood, and to the field of telescopic observation.

Only partial views of the ordinary periodical meteor showers of December, January, and April last were obtained, of which some descriptions are contained in the Report.

Reductions of the scattered meteor-observations on ordinary nights of the year are an important subject of the Committee's inquiries, which have been kept in view in their operations of the past year. Captain Tupman having obligingly placed a list of nearly 6,000 such observations (made by himself) at their disposal, the greater part of which he has reduced to their most conspicuous radiant points, the present purpose of the Committee is most effectually obtained by the publication of the valuable meteor list which has thus unexpectedly come into their possession; and a graphic projection of the radiant points has been prepared, which will be printed as an illustration of the copious information that will be gathered by observers from the contents of Captain Tupman's list. The catalogue will be distributed this year to observers interested in the research; and to enable suitable lithographic charts to be added to it, it is hoped that the members of the British Association will assist the Committee with such liberal communications of their observations as they have hitherto abundantly supplied.

Note on a Natural Limit to the Sharpness of the Spectral Lines, by Lord Rayleigh, F.R.S.

In the explanation usually given of the broadening of the fixed lines with increased pressure, it appears to be assumed that their finite width depends on the disturbance produced by the mutual influence of the colliding molecules. I desire to point out that even if each individual molecule were allowed to execute its vibrations with perfect regularity, the resulting spectral line would still have a finite width, in consequence of the motion of the molecules in the line of sight. If there is any truth at all in the kinetic theory of gases, the molecules of sodium, or whatever the substance may be, are moving in all directions indifferently, and with velocities whose magnitudes cluster about a certain mean. The law of distribution of velocities is probably the same as that with which we are familiar in the theory of errors, according to which the number of molecules affected with a given velocity increases, the nearer that velocity is to the mean.

By the principles of this theory of gases the mean square of the velocity of the molecules can be deduced from the known pressure and mass. If v denote the velocity whose square is equal to the mean, it is found that for air at the freezing-point, $v = 485$ metres per second.

At the temperature of flame, the velocity may be about three times greater. For the purposes of a rough estimate it will be accurate enough to take the mean velocity of the molecules at 1,500 metres per second, and that of light at 300,000,000 metres per second. The wave-length of the light emitted by a molecule moving with the mean velocity from the eye will therefore be greater by about five millionths than if the molecule were at rest. The double of this will be a moderate estimate of the width of the spectral line, as determined by the cause under consideration. We may conclude that however rare the gas, and however perfect our instrument may be, a fixed line cannot be reduced to within narrower limits than about a hundredth part of the interval between the sodium lines. I must leave it to spectroscopists more practised and skilful than myself to say whether this result is in agreement with the appearance of the spectrum.

SECTION B.—CHEMISTRY

The report of the Committee appointed to examine the *Methods of making Gold Assays and stating the Results thereof*, was read by Mr. W. C. Roberts.

The report stated that although the amount of alloy in gold could be ascertained to within a maximum error of 0·01 per cent., or one ten-thousandth part, yet there was an amount of difference between the results obtained by different assayers which required an explanation. The committee considered that the difference between different assayers was too great to be accounted for by the ordinary causes of error in analysis, and they had therefore come to the conclusion that the nominally assayed gold must have contained some impurity which had escaped the assaying process. The committee had precipitated eighty ounces of gold from no less than a hundred gallons of chloride of gold, and they suggested that the gold thus obtained might be used as a standard with which the gold assayed by different assayers might be compared.

Mr. A. Vernon Harcourt, F.R.S., and Mr. F. W. Fison, F.C.S., explained a *Continuous Process for Purifying Coal Gas and obtaining Sulphur and Ammonium Sulphate*.

Mr. Vernon Harcourt said that the usual method of freeing coal gas from sulphuretted hydrogen was by passing it through lime. But oxide of iron was also employed in place of the lime, the advantage possessed by the oxide being that whilst the lime, after it had served its purpose, was useless and difficult to get rid of, the oxide of iron could be used repeatedly for the same purpose. The chemical changes involved were, that when the gas had passed through the oxide the latter was changed into sulphide of iron; when the sulphide was exposed to the air, the sulphur separated and the oxide was re-formed, thus enabling the oxide to be again used. This was called a *continuous* process, because the oxide could be continuously used. But the process was not quite continuous, for after the oxide had been used some thirty times, it became so clogged with sulphur as to be useless. The advantage of the process he was about to describe was that the oxide could be used over and over *ad infinitum*, and, besides, the ammonia was secured in a marketable form. The present method of freeing gas from ammonia by "scrubbing," or passing it through a large receiver containing a small quantity of water spread over a large surface, had one or two defects. It did not secure the ammonia in a good form, and it probably diminished the illuminating power of the gas, for olefiant gas was soluble in water. The new process was applicable wherever oxide of iron could be used in the purifying process. The difference from the old process was that the oxide during revivification was moistened with a solution of ferric sulphate (per sulphate of iron), and a portion of the oxide was removed from time to time, and treated as follows:—It was first extracted with water by the use of a well-known arrangement. The soluble salts were sulphate of ammonia—formed in the purification by the reaction of ammonia upon ferric sulphate—and, in smaller quantities, sulphocyanide, hypo-sulphite, and probably sulphate of ammonia. This extract was mixed with a small excess of sulphuric acid; and yielded when concentrated by evaporation, crystals of ammonium sulphate. The remainder of the substance was then boiled with dilute sulphuric acid, which dissolved the oxide and left a residue of sulphur. The actual process of extraction by acid consisted in treating the substance successively with (1) a solution of ferric sulphate containing some free sulphuric acid; (2) with a more dilute solution of ferric sulphate to which sulphuric acid had been added; (3 and 4) with more dilute solutions of ferric sulphate—all these liquids being the product of a former extraction—and (5) with water. The liquid resulting from the first of the treatments enumerated above was a strong solution of ferric sulphate, which was used as already mentioned, by being mixed with the charge of oxide before it was replaced in the purifier. The residue of the final washing consisted almost entirely of sulphur, and required only to be dried. It would be evident that all the oxide which had been freed from sulphate of ammonia and sulphur by this treatment passed into the condition of ferric sulphate, and in this condition it was replaced in the purifier. There it again became oxide by the action upon it of the ammonia in the gas, which it completely removed, fixing it as sulphate. This system had been brought into use as a manufacturing process, and had been found to be, as far as could be judged, a complete success.

Mr. Fison explained at length the apparatus by which the process was carried into effect.

Mr. J. Spiller, F.C.S., gave a short communication on *Artificial Magnetite*, the object of which was, first, to point out an error in the statement of a chemical reaction occurring in several standard works of reference; and, in the second place, to indicate the formation of crystallised magnetic oxide of iron (magnetite) in the ordinary process of manufacturing aniline from nitro-benzol by the reducing action of metallic iron. Reference was made to "Reimann's Aniline and its Derivatives," and to Wagner's "Chemical Technology," where the action of iron upon nitrobenzol in the presence of acid (Béchamp's process) is stated to give ferric oxide, or a "hydrated oxide of iron." The author pointed to the fact that the ordinary residual product in this operation was *black*, and could be so far purified by washing and elutriation from the excess of iron, usually remaining in admixture, as to give a fine black pigment, which appeared under the microscope as minute octohedra, and was strongly magnetic. Chemical analysis showed this to consist almost entirely of magnetic oxide of iron, with such impurities as were inherent to the process, or previously existed in the cast iron. The physical properties of this form of oxide were further described, and its analogy to the native varieties of magnetic ore (Cornish and Dannemora) shown.

Mr. W. C. Roberts exhibited some specimens of artificial horn silver which he had formed by mixing strong solutions of silver nitrate and common salt.

Prof. Schafarick, of Prague, read a paper *On the Constitution of Silicates*, in which he developed his views as to the manner in which certain members of this class of bodies might be graphically represented.—Prof. Crum Brown, whilst complimenting the author on the importance of the step taken, pointed out that we should guard against confusing graphic formulæ, as applied to minerals, with those applied to organic substances, because they do not represent the same kind of knowledge. Structural formulæ in organic substances represented reactions, and not merely composition; in the case of minerals we had as yet no method of following their reactions.

Prof. Crum Brown then read a paper *On the Action of Sulphide of Methyl on Bromacetic Acid*. He said bromacetic acid dissolved readily in sulphide of methyl. The solution soon became warm and separated into two layers, the lower of which solidified into a white crystalline mass. The crystals were easily purified by washing with absolute alcohol, in which they were very sparingly soluble. Analysis had given a result for this compound which showed it to be a compound of one molecule of bromacetic acid, and one of sulphide of methyl. The compound was obviously analogous to hydrobromate of betain.

Mr. Jesse Lovett described an improved gas-burner.

simply a modification of Wallace's gas-burner. The improvement consisted in a simple mechanism whereby the air and gas could be shut off by one movement.

SECTION C.—Geology

Second Report on the Discovery of Fossils in certain remote parts of the North-western Highlands, by W. Jolly.

During the past year search has been made at various points along the great limestone strike of the North-western Highlands, but, with the exception of the Durness basin, from which the fossils already collected have been alone obtained, none have been found at any new locality. It is most desirable that continued search should be made for fossils, and to determine if the fossiliferous Durness limestone be the same as that in the line of strike from Eribol to Skye.

Report on Earthquakes in Scotland, by Dr. J. Bryce, F.G.S.

Last year a report on this subject was read at Brighton, stating that there had been but little to record during the year then reported on; but whilst the Association was sitting a shock occurred in the Comrie district, an account of which is given in the report now presented. The earthquake occurred on Aug. 8, 1872, at from 8m. to 10m. past 4 o'clock in the afternoon. The successive phases, according to almost all the observers, were:—a noise or sound, loud, heavy, and rumbling; a shock with a shaking and rattling of objects; and a wave-like motion of the ground. The undulations appear to have come from the W. or N.W.; according to some observers, from the opposite direction; but these probably did not distinguish between the first impulse and the recoil.

The extent of country through which the shock was felt is greater than that of any which has occurred since this inquiry was undertaken. The limits are marked by Stirling and Blair Logie on the S.E., and by St. Fillans on Loch Earn, and Glen Lednock on the N.W. The shock was feebler at their limits than in the country between, as about the Bridge of Allan, Dunblane, &c. The breadth of the disturbed area does not appear to have extended more than two or three miles from the Allan Water; the shock seems to have emanated near Comrie. The geological formations of the district are very various in character, and it does not appear that any connection can be traced between the nature of the rock forming the surface and the severity of the shock.

Another shock, which occurred at 9.55 P.M. on April 16, 1873, is briefly described. This was in the South of Scotland, in the parishes of Tyrone, Glencairn, and others adjacent. According to one observer, there was another shock in this district at 2.46 A.M. on the following morning.

Report of the Committee for Exploring the Settle Cave, by W. Boyd Dawkins, F.R.S.

This cave is of great interest, and is being explored by a local committee, aided by a grant from the British Association. In the newest layers there is evidence of human occupation during the historic period; but in the older cave earth, which contains the remains of extinct mammalia, no trace of man has yet been discovered. The exact age of the cave earth is a matter of dispute. Mr. Tiddeman, from the physical evidence alone, regards it as pre-glacial, or rather as older than the great ice-sheet of that district. Mr. Dawkins, whilst doubting the physical evidence afforded by the cave alone, is inclined to regard the fauna as pre-glacial, and he remarks:—"It is obvious that the hyænas, bears, mammoths, and other creatures found in the pleistocene stratum could not have occupied the district when it was covered by ice; and had they lived soon after the retreat of the ice-sheet, their remains would occur in the river-gravels, from which they are absent throughout a large area to the north of a line drawn between Chester and York, whilst they occur abundantly in the first glacial river deposits south of that line. On the other hand, they belong to a fauna, that overran Europe, and must have occupied this very region, before the glacial period. It may, therefore, reasonably be concluded that they occupied the cave in pre-glacial times, and that the stratum in which their remains lie buried, was protected from the grinding of the ice-sheet, which destroyed nearly all the surface accumulations in the river-valleys, by the walls and roof of rock, which has since, to a great extent, weathered away."

Report of the Boulder Committee, by Rev. H. W. Crosskey, F.G.S.

This committee was appointed at the Brighton meeting to collect and tabulate information upon the distribution of erratic blocks throughout England and Wales. Good work has already been done in Scotland by a committee formed for a similar purpose. It is evident that some steps should at once be taken to record the existence of remarkable blocks, and if possible to take some steps to ensure their preservation.

The report, which is necessarily chiefly preliminary, describes the distribution of boulders around Charnwood Forest, and refers to the existence of Charnwood Forest boulders in Shropshire. It also contains a notice, by Mr. Pengelly, of a large granite boulder below the raised beach in Barnstaple Bay. An account is given of the place adopted by the Geological Section of the Birmingham Natural History Society for mapping the boulders of their district, a plan so effective that we reproduce the paragraph referring to it in the hopes that other districts may follow the good example here set. "The Ordnance map of the neighbourhood of Birmingham has, in the first place, been divided by ruled lines with squares of one inch wide, each square enclosing a representation of one square mile of country. Enlarged maps, on the scale of six inches to the mile, were prepared from this. On these enlarged maps the boulders are to be marked by circles, the number of concentric circles representing the diameter of one boulder in feet. For collecting specimens of the rocks of which the boulders are composed, bags were made and numbered, corresponding to each square on the map. At the same time notes were to be made of any specimen that was of unusual interest. Finally it was proposed to represent, on a duplicate map, the number of boulders and the character of the rocks by discs of colour, so that a graphic representation of the boulders as to position, numbers, and kind of rock, would be given, and the source of any class of boulders, as granite *e.g.*, could be readily traced. It was further proposed to make a rough relief map of the district, so as to judge in what way the configuration of the country had affected the distribution of the boulders.

On the Whin Sill of Northumberland, by W. Topley, F.G.S., and G. A. Lebour, F.G.S.

This paper, the result of work by the authors during the progress of the Geological Survey, was laid before the section by permission of the Director-General of the Survey.

The basaltic rocks of the North of England occur in two forms, either as *dykes* cutting vertically through the rocks, or as *beds* lying amongst them. The intrusive character of the dykes is undisputed, but there is much uncertainty prevailing as to the character of the beds of basalt. The authors endeavoured to show that it too is intrusive, and has been forced in a melted state through the rocks long after their deposition and partial consolidation.

The Whin Sill is best known in Teesdale and along the face of the great Pennine escarpment. This district was only briefly alluded to, partly because it has already been often described, especially by Professors Sedgwick and Phillips, but also because the intrusive character of the rock is less evident there than in Northumberland.

An account of the literature of the subject was then given, and a MS. section of the Northumberland coast, made in 1822, by Sir Walter C. Trevelyan, Bart., was exhibited. Although the Whin Sill of more southern districts had been mentioned by earlier writers, it was not till the publication of Sir Walter Trevelyan's paper in the Wernerian Transactions for 1823, that attention was drawn to the intrusive character of the rock.

The Whin Sill is a true basalt, and does not differ in appearance or composition from the whin dykes of the district. In Teesdale it is very uniform in its position amongst the sedimentary strata; for this reason, and because it generally alters but slightly, if at all, the rocks above, Prof. Phillips, and most geologists who have given most attention to the Teesdale district, believe the whin to be of the same date as the beds amongst which it lies.

The object of the paper was to show that through Northumberland the Whin Sill is not so constant in position, that it frequently very greatly alters the beds above it as well as those below, and that, in numerous instances, it can be shown to cut through the strata in a manner that would be impossible with a contemporaneous bed. It also varies in position to an extent of more than 1000 feet, and often comes up, not in true beds, but in bosses.

Nothing can be certainly known as to the age of this Whin Sill. That it is later than the beds with which it is associated is certain, but many considerations lead to the inference that it may not be later than the latter part of the carboniferous period.

SECTION D.—BIOLOGY.

DEPARTMENT OF ANATOMY AND PHYSIOLOGY.

The Localisation of the Functions in the Brain, by Professor Ferrier.

All are agreed that it is with the brain that we feel, and think and will, but whether there are certain parts of the brain devoted to particular manifestations is a subject on which we have only imperfect speculations or data too insufficient for the formation of a scientific opinion. The general view is that the brain as a whole subserves mental operations, and that there are no parts specially devoted to any particular functions. This has been recently expressed by so high an authority as Professor Séquard. The idea rests chiefly on the numerous facts of disease with which we are acquainted. There are cases where extensive tracts of brain are destroyed by disease, or removed after a fracture, apparently with no result as regards the mind of the individual. Along with these facts we have others which are very curious, and which hardly seem to agree with this doctrine. One of these is that when a certain part of the brain is diseased, in Aphasia, the individual is unable to express himself in words. Other curious phenomena have been well described by Dr. Hughlings Jackson, viz., that certain tumours or pathological lesions in particular parts of the brain give rise, by the irritation which they keep up, to epileptiform convulsions of the whole of one side, or of the arm or leg or the muscles of the face; and from studying the way in which these convulsions show themselves he was able to localise very accurately the seat of the lesion.

The great difficulty in the study of the function of the brain has been in the want of a proper method. When we study the function of a nerve, we make our experiments in two ways. In the first place, we irritate the nerve by scratching or by electricity, or by chemical action, and observe the effect; and in the second place, we cut the nerve, and observe what is lost. In regard to the brain and nervous system, the method has been almost entirely, until recently, the method of section. It has been stated by physiologists that it is impossible to excite the brain into action by any stimulus that may be applied to it, even that of an electric current; they have, therefore, adopted the method of destroying parts of the brain. This method is liable to many fallacies. The brain is such a complex organ that to destroy one part is necessarily to destroy many other parts, and the phenomena are so complex that one cannot attribute their loss to the failure only of the parts which the physiologists have attempted to destroy.

About three years ago, two German physiologists, Fritsch and Hitzig, by passing galvanic currents through parts of the brains of dogs, obtained various movements of the limbs, such as adduction, flexion, and extension. They thus discovered an important method of research, but they did not pursue their experiments to the extent that they might have done, and perhaps did not exactly appreciate the significance of the facts at which they had arrived.

I was led to the experiments which I shall have to explain by the effects of epilepsy and of chorea, which have been explained to depend upon irritation of parts of the brain. I endeavoured to imitate the effects of disease on the lower animals, and determined to adopt the plan of stimulating the parts of the brain by electricity, after the manner described by Fritsch and Hitzig.

I operated on nearly a hundred animals of all classes—fish, frogs, fowls, pigeons, rats, guinea pigs, rabbits, cats, dogs, jackalls, and monkeys. The plan was to remove the skull, and keep the animal in a state of comparative insensibility by chloroform. So little was the operation felt that I have known a monkey, with one side of the skull removed, awake out of the state induced by the chloroform, and proceed to catch fleas or eat bread and butter. When the animal was exhausted I sometimes gave it a little refreshment, which it took in the midst of the experiments.

First, as to the experiments on cats, I found that on applying the electrode to a portion of the superior external convolution the animal lifted its shoulder and paw (on the opposite side to that stimulated) as if about to walk forward; stimulating other parts of the same convolution, it brought the paw suddenly back, or put out its foot as if to grasp something, or brought forward its hind leg as if about to walk, or held back its head as if astonished, or turned it on one side as if looking at something, according to the particular part stimulated. The actions produced by stimulating the various parts of the middle external convolution were a drawing up of the side of the face, a backward movement of the whiskers, a turning of the head, and a contraction of the pupil respectively. A similar treatment of the lower external convolution produced certain movements of the angles of the mouth; the animal opened the mouth widely, moved its tongue, and uttered loud cries, or mewed in a lively way, sometimes starting up and lashing its tail as if in a furious rage. The stimulation of one part of this convolution caused the animal to screw up its nostrils on the same side; and, curiously enough, it is that part which gives off a nerve to the nostril of the same side.

Results much of the same character were produced by the stimulation of the corresponding or homologous parts of the rat, the rabbit, and the monkey. Acting upon the anterior part of the ascending frontal convolution the monkey was made to put forward its hand as if about to grasp. Stimulation of other portions acted upon the biceps, and produced a flexing of the fore-arm, or upon the zygomatic muscles. The part that appeared to be connected with the opening of the mouth and the movement of the tongue was homologous with the part affected in man in cases of aphasia. Stimulation of the middle temporo-sphenoidal convolution produced no results; but the lower temporo-sphenoidal, when acted upon, caused the monkey to shut its nostrils. No result was obtained in connection with the occipital lobes.

These experiments have an important bearing upon the diagnosis in certain kinds of cerebral disease, and the exact localisation of the parts affected. I was able to produce epileptic convulsions of all kinds in the animals experimented upon, as well as phenomena resembling those of chorea or St. Vitus's dance. The experiments are also important anatomically, as indicating points of great significance in reference to the homology of the brain in lower animals and in man, and likewise served to explain some curious forms of expression common to man and the lower animals. The common tendency, when any strong exertion is made with the right hand, to retract the angle of the mouth and open the mouth on the same side, had been stated by Oken, in his *Natur-geschichte*, to be due to the homology between the upper limbs and the upper jaw; the true explanation being that the movements of the fist and of the mouth are in such close relation to each other that when one is made to act powerfully the impression diffuses itself to the neighbouring part of the brain and the two act together.

The experiments have likewise a physiological significance. There is reason to believe that when the different parts of the brain are stimulated, ideas are excited in the animals experimented upon, but it is difficult to say what the ideas are. There is, no doubt, a close relation between certain muscular movements and certain ideas which may prove capable of explanation. This is supported by the phenomena of epileptic insanity. The most important guide on the psychological aspect of the question is the disease known as Aphasia. The part of the brain which is the seat of the memory of words is that which governs the movements of the mouth and the tongue. In Aphasia the disease is generally on the left side of the brain, in the posterior part of the inferior frontal convolution, and it is generally associated with paralysis of the right hand, and the reason might be supposed to be that the part of the brain affected is nearly related to the part governing the movements of the right hand.

It is essential to remember that the movements of the mouth are governed bi-laterally from each hemisphere. The brain is symmetrical, and I hold it to be a mistake to suppose that the faculty of speech is localised on the left side of the brain. The reason why an individual loses his speech when the left side of the brain is diseased is simply this. Most persons are right-handed, and therefore left-brained, the left side of the brain governing the right side of the body. Men naturally seize a thing with the right hand, they naturally therefore use rather the left side of the brain than the right, and when there is disease, there the individual feels like one who has suddenly lost the use of his right arm.

I may, finally, briefly allude to the results of stimulating the different ganglia. Stimulation of the corpora striata causes the limbs to be flexed; the optic thalami produces no result: the corpora quadrigemina produce, when the anterior tubercles are acted upon, an intense dilatation of the pupil, and a tendency to draw back the head and extend the limbs as in opisthotonos;

while the stimulation of the posterior tubercles leads to the production of all kinds of noises. By stimulating the cerebellum various movements of the eye-balls are produced.

In the discussion which ensued Dr. Geo. Harley alluded to the effect of mental emotion on the bodily functions, and the possibility of producing disease by simply acting on the nervous system. Referring to phrenology, he said it was one thing to localise function in the interior of the brain, and quite another to specify functions by manipulating the external cranium; and he quoted a saying of Flourens with reference to phrenology: "Les hommes qui la pratiquent sont des charlatans, et les hommes qui la croient sont des imbeciles."

Dr. Carpenter remarked that the great work of the brain is done in the cortical substance, and in Dr. Ferrier's experiments the first effect of the stimulus is upon that particular substance, producing an intensification of the circulation through it; being in that respect different from the ordinary stimulation of a nerve which acts upon the fibrous substance of the medullary matter of the brain. He had long since expressed his disbelief in phrenology, which maintained that the animal functions were placed at the back of the head, and the intellectual at the front. Dr. Ferrier's experiments tended to show that the real seat of the intellectual functions was in the posterior part of the brain.

Dr. Brunton, however, alluded to the faculty of will and of self-restraint as distinguishing man from the lower animals, and said that this was probably situated in the *anterior* part of the brain. It was noticeable that criminals, who were deficient in that faculty, possessed only a small portion of brain in front of the head.

Prof. Burdon Sanderson said that the stimulus in Dr. Ferrier's experiments was, contrary to Dr. Carpenter's supposition, exactly like the ordinary excitation of a nerve, and that the effect was produced in an extremely short space of time.

Note on Huizinga's Experiments on Abiogenesis, by Dr. Burdon-Sanderson.

Under the title of a "Contribution to the Question of Abiogenesis," Prof. Huizinga has very recently published (Pfluger's Archiv. vol. vii. p. 549) a series of experiments which deserve notice as constituting a new and carefully worked out attempt to support the doctrine of spontaneous generation.

Prof. Huizinga begins his paper with the words *Multa renascentur quæ jam cecidere*, using them as an expression of the recurring nature of this question. He then proceeds to say that he was induced to undertake his inquiry by the publication of the well-known work of Dr. Bastian (whom he compliments as having awakened the exhausted interest of physiologists in the subject), his special object being to repeat the much-discussed turnip-cheese experiment.

Everyone knows what Dr. Bastian's observation is. It is simply this, viz. that if a glass flask is charged with a slightly alkaline infusion of turnip of sp. g. 1015, to which a trace of cheese has been added, and is then subjected to ebullition for ten minutes and closed hermetically while boiling, and finally kept at fermentation temperature, Bacteria develop in it in the course of a few days. This experiment has been repeated by Huizinga with great care, and the accuracy of Dr. Bastian's statement of fact confirmed by him in every particular; yet notwithstanding this he thinks the evidence afforded by these results in support of the doctrine so inadequate, that he, desiring to find such evidence, has thought it necessary to repeat the experiment under what he regards as conditions of greater exactitude.

Huizinga's objections to Bastian's experiment are two. First, that when a flask is boiled and closed hermetically in ebullition, its contents are almost entirely deprived of air, and (2) that cheese is a substance of mixed and uncertain composition. To obviate the first of these objections, he closes his flasks, after ten minutes boiling, not by hermetically sealing them, but by placing over the mouth of each, while in ebullition, a porous porcelain plate which has just been removed from the flame of a Bunsen's lamp. The hot porcelain plate is made to adhere to the edge or lip of the flask by a layer of asphalt with which the edge has been previously covered. The purpose of this arrangement is to allow air to enter the flask, at the same time that all germinal matter is excluded. It is not necessary to discuss whether this is so or not.

To obviate the second objection he alters the composition of the liquid used: he substitutes for cheese, peptone, and for turnip infusion, a solution containing in a litre of distilled water:—

Grape sugar	25 grammes
Potassium nitrate	2 ,,
Magnesium sulphate	2 ,,
Calcium phosphate	0·4 ,,

The phosphate is prepared by precipitating a solution of calcium chloride with ordinary sodium phosphate, taking care that the chloride is in excess. The precipitate of neutral phosphate so obtained is washed and then added to the saline solution in the proportion given. On boiling it is converted into soluble acid phosphate, and insoluble basic salt, of which the latter is removed by infiltration. Consequently the proportion of phosphate in solution is less than that above indicated.

To the filtrate, peptone is added in the proportion of 0·4 per cent.

The peptone is obtained by digesting egg-albumen at the temperature of the body in artificial gastric juice made by adding the proper quantity of glycerin extract of pepsin to water acidulated with hydrochloric acid. The liquid so obtained is first rendered alkaline by the addition of liquor sodæ, then slightly acidulated with acetic acid and boiled. The syntonin thus precipitated is separated by infiltration from the clear liquid, which is then evaporated to a syrup and poured in a thin stream into strong alcohol, with constant agitation. The precipitated peptone is separated after some hours and washed with alcohol, and redissolved in a small quantity of water. The solution is again precipitated by pouring it into alcohol in the same way as before, and the precipitate washed and dried.

Flasks having been half filled with the liquid thus prepared (in 1,000, 2 each of nitre and Epsom salts, a trace of phosphate of lime, 25 parts of grape sugar, and 4 parts of peptone), each is boiled for ten minutes, closed while boiling, with the earthenware plate as above described, and placed as soon as it is cool in the warm chamber at 30° C. The experiment so made "gave, without any exception, a positive result in every case. After two or three days the fluid was crowded with actively moving Bacterium termo."

The readers of NATURE are aware that in June last I published a repetition of Dr. Bastian's experiments with a variation not of the liquid but of the mode of heating (see NATURE, vol. viii., p. 141). Instead of boiling the flasks for ten minutes over the open flame and closing them in ebullition, I boiled them, closed them hermetically, and then placed them in a digester in which they were subjected to ebullition under a pressure of two inches or more of mercury. The result was negative. There was no development of Bacteria.

Since the publication of my experiments Huizinga's have appeared. His result, regarded as a proof of spontaneous generation is clearly not superior to Bastian's. The substitution of a soluble immediate principle for an insoluble mixed product like cheese, and the use of a definite solution of sugar and salts are not material improvements. The question is not whether the germinal matter of Bacteria is present, but whether it is destroyed by the process of heating. Consequently what is necessary is not to alter the liquid but to make the conditions of the experiment as regards temperature as exact as possible. In this respect Huizinga's experiment is a confirmation of Bastian's and nothing more.

I have recently repeated it with the same modifications as regards temperature as those employed in my repetition of the turnip-cheese experiments. The result has been the same. In all other respects I have followed the method described by him in his paper.

I have prepared the solution of salts, grape sugar, and peptone in exact accordance with his directions. To obviate his objection as to the absence of air, I have introduced the liquid, not into flasks, but into strong glass tubes closed hermetically at each end and only half filled with liquid, the remainder of the tube containing air at the ordinary tension. Each of these tubes, after having been subjected to the temperature of ebullition under two inches of mercury for half an hour, has been kept since September 10 at the temperature of fermentation (32° C.). Up to the present time, no change whatever has taken place in the liquid.

As a control experiment I opened one of the tubes immediately after boiling, and introduced a drop of distilled water. It became opalescent in twenty-four hours.

In conclusion let me observe that I still maintain my resolution to take no side whatever in this controversy. I do not hold that spontaneous generation is impossible. I do not regard heterogenists as scientific heretics. All I say is, that up to the present moment I am not aware of any proof that they are right.

On the Electrical Phenomena which accompany the Contractions of the leaf of Dionæa muscipula, by Dr. Burdon-Sanderson.

It is well known that in those structures in the higher animals which are endowed with the property of contracting when stimulated—viz., nerve and muscle—this property is associated with the existence of voltaic currents which have definite directions in the tissue. These currents have been the subject of very careful observation by physiologists. They require delicate instruments for their investigation, but the phenomena dependent on them admit of the application of the most exact measurements. The constant current which can be shown to exist in a muscle is called the normal current. The most important fact with reference to it is that it exists only so long as the muscle is alive, and that it ceases during the moment that the muscle is thrown into action. Other characteristics of the muscle currents were referred to, which we have not space to mention.

In certain plants said to possess the property of irritability, contraction of certain organs on irritation occurs which strikingly suggest a correspondence of function between them and the motor organs of animals. Among the most remarkable are those of Drosera and some other plants belonging to the same natural order, particularly the well-known Venus' Flytrap (*Dionæa muscipula*). The Sensitive Plant, the Common Monkey Flower, the Rock Cistus, afford other examples.

Strange as it may seem the question whether these contractile movements are accompanied with the same electrical changes as those which occur in the contraction of muscle and in the functional excitation of nerve has never yet been investigated by vegetable physiologists. Mr. Darwin, who for many years has devoted much attention to the animal-like functions of Dionæa and Drosera, kindly furnished plants for the purpose of the necessary experiments, which have been made by Dr. Sanderson in the laboratory of University College, London. The result has been that the anticipations he had formed have been confirmed as to the existence of voltaic currents in these parts, and particularly in the leaf of Dionæa. By a most remarkable series of experiments (which will be published subsequently) made with the aid of Sir W. Thompson's galvanometer, he has shown that these currents are subject, in all respects in which they have been as yet investigated, to the same laws as those of muscle and nerve.

On Physiological Researches on the Nature of Cholera, by Dr. Brunton.

Without entering into the question of the nature of cholera poison, the writer regarded it as probable that its effects might be counteracted in the same way as those of other poisons—by appropriate antidotes He supposed that if a poison could be found having a similar action to that of cholera, an antidote to the former might prove a remedy for the latter. The condition of cholera collapse has been attributed by Parkes and Johnson to contraction of the vessels in the lungs, and their theory is generally adopted. The writer fround that muscarin—an alkaloid derived from a species of poisonous mushroom—caused contraction of the vessels of the lungs and some of the symptoms which are counteracted by atropia. It therefore seems probable that atropia might be useful in cholera, and in fact an American practitioner has recently employed large doses of it with success. The fact that nitrate of amyl, which also relaxes the pulmonary vessels, is useless as a remedy in cholera, as well as the absence of distension of the right side of the heart in cholera patients during life, shows that Parkes and Johnson's theory is imperfect, and that one of the most important conditions in cholera is active dilatation of the large veins in the interior of the body. The condition might be relieved by digitalis. The effect of this poison was at once observed in cholera. The rice water stools in cholera were stated to have exactly the same composition as the fluid secreted after the division of the intestinal nerves in Moreau's experiment, and the profuse secretion from the intestines in cholera was therefore attributed to paralysis of some of the intestinal nerves. Injection of Epsom salts into the intestines also produced a profuse secretion, though this might be due to irritation and not to paralysis of the nerves. This is not lessened in the least by atropia, and it seems therefore probable that atropia will not prove a perfect remedy for cholera. Dr. Brunton is still endeavouring to find a remedy which will arrest this secretion.

On the Movements of the Glands of Drosera, by Alfred W. Bennett, F.L.S.

The peculiar movement of the glands which cover the margin and the upper side of the leaf of the Sundew has often attracted the attention of botanists. The observations were all made on the commonest species, *Drosera rotundifolia.*

It should be noted in the first place that the glands of Drosera are in no sense hairs, *i.e.* cellular expansions of the epidermis of the leaf. They have been shown by Groenland and Trécul to be an integral part of the leaf itself, penetrated by a fibro-vascular bundle with spiral threads (in other words by a vein or nerve of the leaf) from one end to the other, and even furnished with stomata on their surface. They terminate in a pellucid knob within which is found their peculiar viscid secretion. Under a low magnifying power this secretion may be seen collected about the knobs, and stretching in thin glutinous strings from one to another. The secretion has probably an attraction for flies and other small insects, as, if the plant is examined in its native bogs scarcely a leaf will be found in which an insect is not imprisoned, and one leaf will very often show as many as three or four. The experiment was made of placing a very small insect, a species of Thrips, on a leaf at that time quite unencumbered beneath a low power of the microscope. Immediately on coming into contact with the viscid secretion it made vigorous efforts to escape, but these efforts only seemed to entangle it all the more deeply. The contact of the insect appeared to excite a stronger flow of the secretion, which soon enveloped the body of the animal in a dense almost transparent slime, firmly glueing down the wings, and rendering escape hopeless. It still, however, continued its struggles, a motion of the legs being still clearly perceptible after the lapse of three hours. During all this time the insect was sinking lower and lower down among the glands towards the surface of the leaf, but only a slight change had taken place in the position of the glands themselves, which had slightly converged so as to imprison it more completely. But after the struggles of the prisoner had ceased, a remarkable change took place in the leaf. Almost the whole of the glands on its surface and its margin, even those removed from the body of the insect by a distance of at least double its own length, began to bend over, and point the knobs at their extremities towards it, though it was not observed that this was accompanied by any increased flow of the secretion from them The experiment was made in the evening; and by the next morning almost every gland of the leaf was pointing towards the object in the centre, forming a dense mass over it. The sides of the leaf had also slightly curved forwards so as to render the leaf itself more concave. The nearly allied Venus's Fly-trap, or *Dionæa muscipula* of the United States, which imprisons flies by a much more sudden motion of the sides of the leaf. collapsing when irritated on the upper surface, is said to digest and absolutely consume the insects thus entrapped. What becomes eventually of the prisoners of the sundew, my experiments have not been carried sufficiently far to ascertain. It will be seen that the most singular feature in the phenomena here described is that the motion of the greater number of the glands did not begin till after the insect had become comparatively motionless; and therefore it is very difficult to attribute it to the excitement caused by the struggles on any "contractile tissue" at the base of the glands, an explanation which has been offered for the sudden and rapid motions of the stamens of Berberis or the leaves of Mimosa It is also quite certain that the impinging of raindrops on the surface of the leaf causes no similar motion, a peculiarity similar to that which Darwin has observed in the case of the motions of tendrils and of climbing stems. In order to determine what share in these motions of the glands was due to the organic nature of the substance imprisoned, and to its power of motion, the following experiments were also made:—A small piece of raw meat was placed on another leaf similar to the first. No immediate change was observable, and no increased flow of the secretion; but after the lapse of a few hours a perceptible inclination of the more distant glands towards the object took place. The next morning the piece of meat was found, like the fly, sunk down on to the surface of the leaf, with almost the whole of the glands converging towards it and above it in just the same manner. The changes here were therefore perfectly of the same kind as in the case of the fly. though apparently somewhat slower. After the lapse of twenty-four hours the piece of meat appeared decidedly lighter in colour; but an accident prevented the process of digestion being further traced. On other leaves were placed a minute piece of wood and a small piece of worsted: and in neither of these cases was the least change perceptible after the lapse of a considerable time in the position of the object, nor in that of any of the glands, either those in contact with it or the more remote ones. It would appear, therefore, that the organised structure of the fly and of the piece of raw meat had some power of exciting this motion which is not possessed by matter of a different description.

SCIENTIFIC SERIALS

Poggendorff's Annalen der Physik und Chemie, No. 6, 1873.—This number commences with a paper by M. Seebeck, on the motion of sound in bent and branching tubes. He finds, among other things, that the gradual bending of a tube has little effect on the size of wave-length, but if a tube be suddenly bent to an angle, the sound-motion is considerably affected; it would seem that the motion of the air-particles did not suddenly alter in direction with the tube.—A series of experiments on the electromotive and thermo-electric forces of some metallic alloys, on contact with copper, is detailed by M. Sundell. The alloys examined were bismuth-tin, bismuth antimony (in various proportions), and German silver; the method employed in the case of electromotive force being that of Edlund, based on the fact, that a galvanic current, passing through an electromotor, produces in it, proportionally to its electromotive force, an absorption or production of heat, according as the current is in the same direction as that of the electromotor, or contrary to it. The alloys, like the pure metals, have the same order in electromotive as in thermo-electric series; and it appears that the proportion of thermo-electric to electromotive force is constant, and equal to that for the combinations iron-copper, and copper-bismuth. Comparative experiments on various pyrometric methods—air thermometer, expansion of solid bodies, calorimeter, dissociation of a compound, and electrical resistance, lead M. Weinhold to a preference for the last (or Siemens'), as the most reliable. The calorimeter, properly used, also gives good results.—M. Lorenz, of Copenhagen, furnishes a new determination of the electrical resistance of mercury, in absolute measure. He attributes the discordance in previous results to the employment of induced *currents*, of *variable* strength, and he adopts an ingenious method in which a *constant* electromotive force *without* current, is applied. The result of five experiments is 1 mercury unit = 0·9337 Ohm's unit, or the mercury unit equal to 0·9337. 10^{10} absolute units.—Of the remaining papers we may note one by Kohlrausch on the electrochemical equivalent of silver, and mineralogical notes on wolfram, and on a new mineral, ardenite.

SOCIETIES AND ACADEMIES

LONDON

Royal Horticultural Society.—General Meeting, Aug. 20.—W. A. Lindsay, Secretary, in the chair.—The Rev. M. J. Berkeley said Kerson's seedling gooseberry, a fine variety which gained a first-class certificate at the last meeting, turned out to be not a garden seedling but one originally taken from a common hedge in the neighbourhood of Peterborough. This was not a solitary instance of a fine variety of fruit being found in such places—the Bess Pool apple having been discovered in a plantation at Nottingham. Mr. Berkeley then alluded to a disease of the crocus very destructive to the gladiolus, and which also attacked the saffron crocus and the narcissus; it was first described by Montague under the name of *Tacon*. He concluded by remarking that vegetables treated with sewage were apt to be much deteriorated in flavour.

Sept. 3.—General Meeting.—Dr. Kellock in the chair.—Adverting again to the subject of *Tacon* in the Gladiolus, the Rev. M. J. Berkeley was inclined to attribute it to "sunstroke."—A bunch of grapes was exhibited from the parent plant of the Hampton Court vine; it dated from 1761.—A fungus (*Lentinus lepideus*) was sent by Sir Gilbert Scott, from the roof of a church at Croydon.

PARIS.

Academy of Sciences, Sept. 15.—M. Bertrand in the chair.—The following papers were read:—An answer to Father Tacchini's last note, by M. Faye. The author replied to the objections raised by the Italian observer to the cyclonic theory on the ground of the appearance of prominences where there are no spots. M. Faye considered that the pores, which are vertical cyclones, are the cause of the circulation of the solar hydrogen, and hence of the prominences. He also replied to some objections relating to the direction of the circular motion in cyclonic spots.—New researches on the analysis and the theory of the pulse in normal and abnormal states, by M. Bouilland. The author announced the discovery of a secondary beat in the pulse, which he ascribed to a contraction and expansion of the arteries themselves.—On choleraic dejections as agents in the propagation of cholera, by M. Ch. Pellarin.—On the changes of form exhibited by Comet IV., 1873, by MM. Rayet and André.—On the movement of an elastic wire one end of which has a vibratory motion, by M. E. Mercardier.—On the products of the oxidation of meteoric irons and a comparison of them with the terrestrial magnetites, by M. Stan. Meunier.—Process for the preparation of a new aniline red, by M. E. Ferrière. The new colour is prepared by acting on acetate of aniline with ammoniacal cupric hydrate, and then saturating with sulphuric acid. On concentration ammoniac sulphate is deposited, and the colour remains. It is a purple red.

Sept. 22.—M. Bertrand in the chair.—On the chairman taking his seat, he at once proceeded to announce the deaths of M. Coste, of the Section of Anatomy and Zoology, and of M. Nelaton, of the Section of Medicine and Surgery; and to express in a few words the sorrow of the Academy at the grievous loss it had thus sustained. At the conclusion of the chairman's remarks, M. le Baron Larrey at once proposed that, to mark its sense of the double loss, the Academy should not hear any papers at the meeting, and that the correspondence only should appear in the *Comptes Rendus*. The following papers were accordingly printed:—Thermic researches on the condensation of gases by solids—continuation: absorption of hydrogen by platinum black, by M. P. A. Favre.—Certain observations on the winged form of the *Phylloxera vastatrix* in connection with the propagation of this insect, by M. Max. Cornu.—On the proper time for the application of the submersion treatment to vines tainted by Phylloxera, by M. L. Faucon.—On the proportion of carbonic anhydride in atmospheric air, and on its variation with the altitude, by M. P. Truchot. The author finds that the quantity of this gas diminishes as the altitude increases.—On coralline, by M. Commaille.—Note on a meteorite with a phosphorescent train seen on the night of September 28, 1873, by M. Chapelas.—The second part of M. Mercadier's note on the movement of an elastic wire, one end of which is endued with a vibratory motion.

BOOKS RECEIVED

ENGLISH.—Centrifugal Force and Gravitation: John Harris (Supplement A.)—Half Hours with the Microscope. New Edition (Hardwicke).—Zoological Record, Vol. viii., Edited by Prof. Newton (Van Voorst).—Chapters on Trees: M. and E. Kirby (Cassell).—The Amateur Greenhouse and Conservatory: Shirley Hibberd (Groombridge).—Proceedings of the Literary and Philosophical Society of Liverpool, Vol. xxvi. (Longmans).—A Discourse on the Pursuit of Truth: A. Elley Finch (Longmans).

FOREIGN.—Mikroskopische Physiographie: H. Rosenbusch (Williams & Norgate).

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THURSDAY, OCTOBER 9, 1873

FOREIGN ORDERS OF MERIT

In a recent number of NATURE (vol. viii. p. 292) we intimated that honours had been conferred upon a large number of British men of science by the Emperor of Brazil and the King of Sweden. Some of the gentlemen to whom these Foreign Orders have been offered have, however, thought it right to refuse acceptance of them, mainly from loyalty to Her Majesty's stringent regulations respecting Foreign Orders, as issued by the Secretary of State for Foreign Affairs. A correspondent, who has himself refused to accept the Foreign Orders alluded to in our note, has favoured us with a copy of these regulations, and as many people are ignorant of their nature, or are even unaware that any such regulations exist, we shall be doing a service by giving them publicity in our columns. These "Regulations respecting Foreign Orders" are dated Foreign Office, May 10, 1855, and are as follows:—

"1. No subject of Her Majesty shall accept a Foreign Order from the Sovereign of any foreign country, or wear the Insignia thereof, without having previously obtained Her Majesty's permission to that effect, signified by a Warrant under her Royal Sign-Manual.

"2. Such permission shall not be granted to any subject of Her Majesty, unless the Foreign Order shall have been conferred in consequence of active and distinguished service before the enemy, either at sea or in the field; or unless he shall have been actually and entirely employed, beyond Her Majesty's dominions, in the service of the Foreign Sovereign by whom the Order is conferred.

"3. The intention of a Foreign Sovereign to confer upon a British subject the insignia of an Order must be notified to Her Majesty's Principal Secretary of State for Foreign Affairs, either through the British Minister accredited to the Court of such Foreign Sovereign, or through his Minister accredited at the Court of Her Majesty.

"4. If the service for which it is proposed to confer the Order has been performed during war, the notification required by the preceding clause must be made not later than two years after the exchange of the ratifications of a Treaty of Peace.

"If the service has been performed in time of peace, the notification must be made within two years after the date of such service.

"5. After such notification shall have been received, Her Majesty's Principal Secretary of State for Foreign Affairs shall, if the case comes within the conditions prescribed by the present regulations, and arises from naval or military services before the enemy, refer it to Her Majesty's Principal Secretary of State for the War Department, previously to taking Her Majesty's pleasure thereupon, in order to ascertain whether there be any objection to Her Majesty's permission being granted.

"A similar reference shall also be made to the Commander-in-Chief if the application relates to an officer in the Army, or to the Lords of the Admiralty if it relates to an officer in the Navy.

"6. When Her Majesty's principal Secretary of State for Foreign Affairs shall have taken the Queen's pleasure on any such application, and shall have obtained Her Majesty's permission for the person in whose favour it has been made to accept the Foreign Order, and wear the Insignia thereof, he shall signify the same to Her Majesty's Principal Secretary of State for the Home Department, in order that he may cause the warrant required by Clause 1 to be prepared for the Royal Sign-Manual.

"When such warrant shall have been signed by the Queen, a notification thereof shall be inserted in the *Gazette*, stating the service for which the Foreign Order has been conferred.

"7. The warrant signifying Her Majesty's permission may, at the request and at the expense of the person who has obtained it, be registered in the College of Arms.

"8. Every such warrant as aforesaid shall contain a clause providing that Her Majesty's licence and permission does not authorise the assumption of any style, appellation, rank, precedence, or privilege appertaining to a knight bachelor of Her Majesty's realms.

"9. When a British subject has received the Royal permission to accept a Foreign Order, he will at any future time be allowed to accept the decoration of a higher class of the same order, to which he may have become eligible by increase of rank in the Foreign Service, or in the service of his own country; or any other distinctive mark of honour strictly consequent upon the acceptance of the original Order, and common to every person upon whom such Order is conferred.

"10. The preceding clause shall not be taken to apply to decorations of the Guelphic Order, which were bestowed on British subjects by Her Majesty's predecessors King George IV. and King William IV., on whose heads the crowns of Great Britain and of Hanover were united.

"Decorations so bestowed cannot properly be considered as rewards granted by a Foreign Sovereign for services rendered according to the purport of Clause 2 of these Regulations. They must be rather considered as personal favours bestowed on British subjects by British Sovereigns, and as having no reference to services rendered to the Foreign Crown of Hanover."

Having given these Regulations, we may be permitted, perhaps, to make some remarks upon them. It will be seen that so far as scientific men, as such, are concerned, they are positively interdicted from accepting Orders offered to them by a foreign sovereign except in the improbable case of their doing scientific work for such a sovereign. On the face of them it is evident that they are the product of a time when it was thought that such rewards gained otherwise than on the field of battle might be open to suspicion. We can well understand that there may be reasons why diplomatists, projectors, and the like are better without such Orders, but these reasons do not apply to men of culture, whom a king might delight to honour for work done for mankind at large.

It is clear, therefore, either that the triumphs of Science and her followers were little known or were unappreciated when these Orders were issued, or that such possible recipients were purposely excluded. But are not the triumphs achieved by scientific men over the multitudinous forces of nature of infinitely more importance to humanity, and far more conducive to the highest glory of any country, than the greatest military triumphs that soldiers have ever achieved? Indeed, to what is it supposed that the dire art of war itself has reached its present state of comparative perfection, if not to the advantage which has been taken of the discoveries of Science? And does not the military superiority of one nation over another depend almost entirely on the thoroughness with which scientific theories have been applied to army organisation and the *matériel* of war?

It seems to us unjust and cruel that men of science, to whose labours it is mainly owing that our country and the world generally are mounting rapidly higher and higher in the scale of civilisation, should be practically debarred from accepting the few honours that come in their way. Moreover, we should think that those who have the framing of these Regulations ought to be proud to think that our country produces so many men of science whom foreign sovereigns delight to honour, and instead of throwing obstructions in the way, should afford every reasonable facility to those who are thus honoured to accept and wear the Foreign Orders which may be offered to them. We cannot see that in any way their doing so would endanger the safety of the country nor be derogatory to the dignity and honour of our sovereign. May we not hope, then, that these Regulations as to Foreign Orders should not for ever remain as they are? They certainly permit one to infer that the only glory which those who promulgate them desire to see shed upon their country, is the barbarous glory which can be gained by a good fighter.

We shall be glad to receive the opinions of scientific men on this question.

LUBBOCK'S "MONOGRAPH OF THE COLLEMBOLA AND THYSANURA"

Monograph of the Collembola and Thysanura. By Sir John Lubbock, Bart., M.P., &c. Pp. 265. Seventy-eight plates. (Printed for the Ray Society: 1873.)

THE insects which constitute the Linnæan genus *Podura*, though small and apparently insignificant, present many interesting peculiarities of structure, and still more interesting characters bearing on the great problem of the true affinities and historical evolution of insects generally. They have, however, been comparatively neglected, and those who have worked at their classification have often done so in ignorance of each other's labours, so that the nomenclature of the group is confused. Sir John Lubbock has patiently investigated the characters of the British species, and compared them with those given by Gervais, Nicolet, Bourlet, and Tullberg. The genera he has been led to adopt are arranged in a tabular form on page 39. He gives good reasons for separating *Podura*, *Degeeria*, *Sminthurus*, and their allies from *Lepisma* and *Campodea*; and, while retaining Latreille's name Thysanura for the latter group, proposes for the remainder the new term "Collembola" (κόλλα, ἔμβολον), in allusion to the projection by which they attach themselves to foreign bodies. If this be adopted, there will be no title to designate all the insects belonging to Latreille's Thysanura; but though there is some inconvenience in restricting the meaning of a term already in use, the author would probably hold that the distinctions between the two orders are too great for them to retain with advantage a common name. The change would then be very much like what has been made in separating the herbivorous Cetacea of Cuvier from the rest, giving them a new name, and retaining the old one for the remainder. The relative affinities of either group to other Arthropoda are difficult to decide on. The absence of wings has long, and with ample reason, been discarded by entomologists as a character of importance in classification; the absence of tracheæ, though at first sight more important, does not apply to *Sminthurus* (not *Smynthurus*); the mouth is unlike either the mandibulate or the suctorial type; and the caudal appendage and ventral tube are too peculiar to be of service for comparison. On the whole, the author concludes that "if we represent the divisions of the Articulata like the branching of a tree, we must picture the Collembola and Thysanura as separate branches, though small ones, and much more closely connected with the Insecta than with the Crustacea and Arachnida."* After the chapters on the previous literature of Thysanura and their classification and affinities, comes what to many naturalists will be the most interesting part of the book, a discussion on the evolution of Insects, the origin of wings, and the light thrown on these questions by the study of the groups in hand. It would be impossible to do justice to this chapter in the limits of this article, and it is the less necessary since Sir John Lubbock has lately given our readers an exposition of his views on this subject in the series of papers lately published in these columns on the Metamorphosis of Insects. The remainder of the work consists of a general account of the anatomy of the Collembola and Thysanura, in which there are numerous exceedingly valuable original observations, and a systematic description of the characters, habitat, manners and customs of the various genera and species at present known, with copious synonymy. The value of the work is further enhanced by an appendix by Mr. Joseph Beck, on the Scales of Collembola and Thysanura, illustrated by twelve beautiful microscopic drawings, from the hand of the late Mr. Richard Beck. Thus the various points of interest offered by the groups treated of, to the microscopist, the entomologist, and the natural philosopher, are fully illustrated. Beside the figures, most of them coloured, many showing different stages of growth, which illustrate nearly fifty of the species described in the text, there are numerous careful outlines of anatomical details, which supply what is too often neglected by systematic naturalists. The tribute paid by the author to the artist whose intelligent skill has overcome the most grievous obstacles, will be endorsed by all who see these beautiful drawings.

We congratulate the Ray Society on the production of so excellent a work. This and the preceding volume by Prof. Allman on the Gymnoblastic Hydroids, will maintain its reputation, and we trust that a society to which we owe such works as Darwin's "Cirripedia," Parker's "Shouldergirdle," and Huxley's "Oceanic Hydrozoa," will continue to make so good a choice of books to publish, and will be still more widely supported than it is.

P. S.

MONCKHOVEN'S "PHOTOGRAPHY"

Traité Général de Photographie. Sixième Edition. Par Dr. v. Monckhoven. Avec figures dans le texte et trois planches photographiques. (Paris, 1873. Georges Masson, Libraire-Editeur, Place de l'Ecole de Médecine.)

THE great advance made by photography as an art, and the yearly increasing number of processes, have made it almost an impossibility for anyone not professionally engaged as a photographer to keep abreast of the tide of improvement.

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* The relation of both to the Myriopoda is expressed in a sentence which some error of the press has rendered unintelligible. It would seem to make the Collembola, alone, a group of equal "value" with Myriopoda. We may remark here that there are an unusual number of misprints.

It is therefore with great pleasure that we welcome Dr. v. Monckhoven's "Traité Général," which seems to omit nothing in the way of recent additions to the number of photographic processes.

The Doctor commences his book with an historical notice of the origin of the art, in the course of which the irrepressible Egyptians make their appearance as having undoubtedly observed the effects of light on certain bodies; but, unfortunately, they have not handed their experience in the matter to posterity. The Egyptians and Greeks, however, having been disposed of, we have sixteen pages of really very useful historical matter, so arranged that a short paragraph is devoted to each of the more important processes, and which is rendered still more valuable by numerous references to the original papers of the various investigators to whom we owe the art.

The author then proceeds to give a sketch of the nature of light. Perhaps in a treatise of this sort one cannot expect a very comprehensive definition of such a subject. Still, however, something more satisfactory than the following might have been expected . . . . "il existe nécessairement entre le soleil et nous, un certain mode de communication dont nos yeux sont l'intermédiare; c'est ce mode de communication qui constitue ce que l'on appelle la *lumière*."

We then have a sketch of the chemical action of light, and a very good description of what a photographic laboratory ought to be, but, we fear, very rarely is. Considerable space is devoted to a description of the method of preparing the various substances required, including gun-cotton and collodion; and here we may observe that Dr. van Monckhoven makes use of the old system of chemical equivalents obsolete in England, and very nearly so on the Continent, a proceeding which is to be regretted in a work which is likely to remain for some time a standard book on its subject. We have noticed that photographers are singularly conservative on this point, for, to the best of our belief, there is not even now a photographic journal which makes use of the present atomic system of notation, a system which even nine years ago was largely used by chemists. A really admirable chapter, on photographic optics succeeds that on photographic chemistry; one soon perceives how much the art has owed to the lenses constructed on the formulæ of Dallmeyer and Steinheil, and to the credit of English opticians we find that in the summary the lenses of the former are stated to surpass all others.

After dealing with cameras, printing frames, studios, and every other photographic requisite, the various processes are dealt with at length. Here we may note that specimens are given of two of the more recent mechanical printing processes, the "Woodburytype," and "Heliotype." Both are pigment methods, and so are not liable to the slow fading inevitable to the ordinary prints containing silver. Of them we can only say that while it is difficult to imagine that any process can surpass the former for artistic effect, the latter seems equally unsurpassable for any purpose requiring excessively minute and faithful reproduction of fine detail, such as is required in copying maps, prints, or diagrams.

A specimen of what is modestly termed the "retouche des clichés," is also given, but here we feel that we are treading on dangerous ground, as a portrait of a lady is the subject. Suffice it to say, that the general effect of this process seems to be like that of the elixir vitæ, and to make the happy patient young and handsome again.

We find considerable information also on photographic enamelling, and on the production of enlargements, where we observe that the heliostat and its use are described.

The work is illustrated with 280 woodcuts, executed in a style which is only found in foreign scientific works, and three specimen photographs are also given. In conclusion we must congratulate Dr. van Monckhoven on the production of so useful a book, hoping only that the chemical portion will be modernised and extended in future editions. Why do not some of our many amateur or professional photographers devote some attention to the chemical nature of their art? Of the rationale of many of the reactions we know absolutely nothing, and of the others our knowledge is not much greater. Such a research would not be of theoretical value only, but would materially aid in the attainment of that perfect application of means to ends by which alone the best results either in art or science can be obtained.

OUR BOOK SHELF

The Relations of the Air to the Clothes we wear, the Houses we live in, and the Soil we dwell on. Three popular lectures delivered before the Albert Society at Dresden. By Dr. Max von Pettenkofer, Professor of Hygiene at the University of Munich, &c. Abridged and translated by Augustus Hess, M.D., Member of the Royal College of Physicians, London, &c. (London: Trubner and Co., 1873.)

DR. HESS has done well in translating these lectures by so great an authority on hygiene as Dr. Pettenkofer. Though the author does not believe that any knowledge of real value can be imparted by means of popular lectures, still they serve a good purpose in the way of "scientific edification and elevation, which are to raise our minds and hearts and to affect us like listening to good music." Though we in this country have perhaps less need to be instructed in the rules of hygiene than the mass of people on the Continent, still, it will be universally admitted that very few are acquainted with the principles which underlie healthy living, and still fewer can be at the trouble to put them into practice. In the little volume before us, which is well translated by Dr. Hess, the author expounds in an interesting and yet thoroughly scientific manner, the *rationale* of healthy living so far as our relations to the air are concerned, and shows the scientific principles on which we should choose our clothes both as to material and make, and which should guide us in building our houses. In the third lecture he speaks of the relations of the air to the soil, or on the Ground-air, and shows how much remains to be done before the principles of hygiene and their practical application can reach anything like perfection. The following extracts will give an idea of Dr. Pettenkofer's method of treatment :—

With regard to Clothing, the author says :—"When exposed to *luminous heat*, the *materials* of our clothing do not show very great differences, but in experimenting on shirtings of different *colours*, the following result was obtained :—When white absorbed 100, pale straw colour absorbed 102, dark yellow 140, light green 155, dark green 168, Turkish red 165, light blue 198, black 208. In the shade these differences nearly vanish. Krieger, in experimenting on tin cylinders filled with warm water, has found that a double tight covering by the same material does not retard the heat loss much more than a single one; but when the outer layer was loose it retarded it very much. From this follows the practical truth, that we can produce a very different effect by the same number of clothes according to their make.

"Generally our clothing has been considered as an apparatus for keeping the air from us. This conception is utterly erroneous, and we can bear no garment which does not allow of a continual ventilation of our surface. Just those textures which are most permeable to the air keep us warmest. I have examined different materials for their permeability to air, and taking the permeability of air passing through flannel as 100, linen allowed 58, silk 40, buckskin 58, chamois 51, kid 1 part of air to pass through them. If the above-stated notion were correct, kid would keep us 100 times, chamois warmer by half, than flannel, and so on, while everyone knows that it is quite the reverse."

With reference to Fur the author says:—"A fur is so arranged that its fine hair projecting into the air intercepts all the heat which flows from the surface of the body by radiation and conduction, and distributes this heat through the air which circulates between the single hair-cylinders. Thus the air, however cold it may be, reaches the nerves of our skin as a warmed air. Furred animals in winter, when touched superficially, give a very cold sensation; it is only near the skin that their hair feels warm. In a severe cold, certainly little of our animal heat comes as far as the points of the hair, from which it would escape by radiation or conduction, as the current of air in the fur cools the hair from its points towards its roots, and a severe cold penetrates only a little farther into the fur, without reaching the skin of the same. This can take place only at an exceedingly low temperature, or when a very cold air is in violent motion. In a well-furred animal the changes of temperature in the surrounding air only change the latitudes at the cold and warm zones in the fur; the place where the temperature of the body and the air equalise each other, moves between the roots and points of the hair, and for this reason a furred animal is not warmer in summer than in winter. In summer its heat leaves at the points, in winter near the roots of the hair."

Journal of the Proceedings and Annual Report of the Winchester and Hampshire Scientific and Literary Society, vol. i., part ii., 1871-2 (Winchester: Warren and Son, 1873).

WE are glad to see from the Third Annual Report of this Society that it continues prosperous, the number of members being, in 1872, 105. We hope good use will be made of the valuable herbarium of flowering plants, ferns, lichens, &c., collected and arranged by the late Mr. Hill, which has come into the possession of the society, through the generosity of the Mayor, Mr. R. P. Forder, and the President. The present part of the journal contains a number of papers, literary and scientific, read at various meetings of the society. The principal one is the Introductory Address delivered at the commencement of the third session, by the Rev. Canon Kingsley, on "Bio-Geology—the science which treats of the distribution of plants and animals over the globe, and the causes of that distribution." The address is an eloquent one, it can easily be imagined, shows extensive knowledge and great shrewdness, and contains many valuable hints both to young and old naturalists. Most of the other papers are also by clergymen, the principal ones being the following:—"On the Dawn of Thought in Greece," by the Rev. W. Awdry; "On the Metamorphosis of Lepidoptera," by Mr. J. Pamplin; "the Planet Jupiter," by the Rev. E. Firmstone, in which the author gives many interesting facts and speculations as to the condition of that planet; "Vesuvius previous to and during the Eruption of 1872," by the Rev. C. A. Johns, in which the author describes an ascent he made shortly before the last eruption, and appends a condensed abstract of Palmieri's account of the eruption. Appended to the journal is a valuable list of 315 works on the Geology, Mineralogy, and Palæontology of the Hampshire Basin, compiled by Mr. William Whitaker, of the Geological Survey.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Wyville Thomson and the Ventriculidæ

I TRUST that you will afford me a little space for a few remarks upon some passages in Prof. Wyville-Thomson's book, the "Depths of the Sea," which, owing to many engagements, has only just come into my hands. So earnest a labourer in the wide field of truth will not, I hope, deem me discourteous if I point out one corner where his feet have slipped; and if it be objected that, after all, it is only in a small spot, the learned Professor will, I am sure, agree with the answer that, even in the smallest steps towards truth, attainable accuracy is important.

In 1847-48 my father published a series of papers in the "Annals and Magazine of Natural History," which were afterwards collected into a volume, on the "Ventriculidæ of the chalk, their microscopic structure, affinities, and classification." This work, which still remains, I believe, the authority on its subject, introduced order and classification where before all was confusion, expressly founding these upon two guiding principles of anatomy, the existence of which had been proved by searching tests. These two principles—the first being the structure, the second the fold, of the membrane—I am careful to recall, as I think there is considerable misapprehension regarding them. The chief locality of these fossils was in the south and west of England.

In his chapter on the Continuity of the Chalk, Prof. Thomson brings forward several families of ancient fauna as palæolontological evidence in support of his argument. Among these he devotes some attention to the Ventriculidæ (he calls them *Ventriculites*, but why? In the same sentence he uses the family name *Hexactinellidæ*); but, though he acknowledges my father's work, and refers to his "minute and most accurate description of their structure," it does not appear by what follows that he has quite comprehended it: "He (Mr. Toulmin Smith) found them to consist of tubes of extreme tenuity, delicately meshed, and having between them interspaces usually with very regular cubial or octohedral forms" ["Depths," &c., p. 482]. This description (the Professor will forgive me for saying so) does not convey a very clear idea of any structure, and certainly does not apply to the Ventriculidæ: if the word "tube" here means the body of the creature, it may in one sense be partially true of a few species in each of the genera—*Ventriculites*, *Cephalites*, and *Brachiolites*; but if it is intended to apply to the substance of the structure, I must say that it denotes a complete error. My father's words are, that "the membrane of the Ventriculidæ is composed of very delicate fibres," "the fibre is single and solid, never fistular," and that in this structure "*there are no tubes whatever*" (pp. 21, 25, 30). My father carefully describes this membrane, and marks it as the essential characteristic of the whole family of Ventriculidæ. Among the thirty-five species, for the most part marked by strong differences, he points out that *Ventriculites simplex* is the type of the whole family, consisting of a single membrane without a trace of fold.

Now, Prof. Thomson gives a figure of the octohedral structure to which I will not take exception, but he writes underneath it, "*Ventriculites simplex*, Toulmin Smith. Section of the outer wall, showing the structure of the silicious net-work." This implies, while citing my father's name (1) that this structure is proper to that species; and (2) that there is an inner wall. It also speaks of the net-work as silicious, while, two pages before, it is said, that "Mr. Toulmin Smith supposed that the skeleton of the Ventriculite had been originally calcareous." But though mistakes of this sort might easily arise through misapprehension, I must say I was very much astonished to see the figures, one of the entire fossil, the other of the "outer surface," given as "*Ventriculites simplex, Toulmin Smith*," from Mr. Sanderson's collection ("Depths," &c., pp. 483, 484.) A glance at Fig. 1, on the second plate in my father's book, will show that the name has been misapplied to this specimen, which, as far as can be judged from the drawing, appears to be either *Ventriculites quincuncialis*, or one of the *Cephalites*, both quite different in outward appearance from the plain *simplex*. I know that it is often not so easy to distinguish the species of those preserved in flint as of those in chalk, but in this instance it is quite evident that it is not *simplex*.

My object in writing the above has been to vindicate my father's scientific accuracy, and to recall the facts he worked out. With regard to another point: it is stated by Prof. Thomson that some of the beautiful sponges discovered in the late deep-sea dredgings, especially the *Holtenia* and its allies, and the Ventriculites, "belong to the same family, in some cases to very nearly allied genera," or, as Dr. Carpenter puts it ("Good Words," October 1872, p. 703):—"Here we found the type of the old Ventriculites, which were supposed to be extinct, still living on in the deep sea." Much as my father would have delighted in the exquisite beauty of these new forms (the *Euplectella* he had examined in 1848), I do not think that he could have acknowledged the *Holtenia* as belonging to the ancient Ventriculidæ; nor, if the use of the word "type" depend for its force upon the character of structure, can it be truly said to be a type of that family. True, it possesses a silicious skeleton, but so does the Euplectella; and neither from Prof. Thomson's description ("Depths," pp. 70–72), nor my own examination, can I discover in the Holtenia any trace of or resemblance to the delicate structure and folded membrane of the Ventriculidæ. With great deference, therefore, to the opinion of these investigators (if I am wrong I will gladly learn), it appears to me that the modern type of the old Ventriculite is yet to be found.

I will add that the series of specimens figured in my father's book is in the British Museum, open to examination by students, together with a large portion of hi collection of the Ventriculidæ.

Highgate, Sept. 27 LUCY TOULMIN SMITH

"Deidamia"

I NOTICE in Prof. Wyville Thomson's extremely interesting papers the name *Deidamia* v. Willemoes-Suhm, used for a crustacean genus. This name must be changed, inasmuch as it is preoccupied in Articulata by Dr. Clemens in 1859. Dr. Clemens has used the title for a valid genus of North American Sphingidæ. I propose, therefore, for the genus in Crustacea, the name *Willemoesia*, in honour of its discoverer, with the two species *leptodactyla* and *crucifer*, the former the type.

AUG. R. GROTE,
Curator of Articulata, B.S.N.S.

Buffalo, U. S., Sept. 15.

Dr. Sanderson's Experiments and Archebiosis

IN a communication made to the British Association during its recent meeting at Bradford, Dr. Sanderson criticises the experiments of Prof. Huizinga, and also throws doubt upon the validity of the conclusions which I have drawn from experiments of my own. The "Note" appears in your columns this week; and seeing the nature of the conclusion drawn by Dr. Sanderson from his experiments, I am not a little surprised to find no mention in it of one most important point, viz., the temperature at which Bacteria are killed when immersed in fluids.

It must be obvious to all who understand the real nature of the question at issue, that no valid conclusion can be drawn by Dr. Sanderson from his experiments, unless he is able to argue from a definite conviction as to the temperature at which Bacteria are killed in fluids.

Now a study of Dr. Sanderson's writings would show the reader that up to the time of their publication he had every reason to believe that Bacteria were uniformly killed in fluids at a temperature of 100° C. If he still believes this to be true, he cannot (in the light of facts which he has learned concerning the productivity of previously boiled fluids in closed flasks) refuse his assent to my main proposition, viz., that Bacteria are capable of arising in fluids independently of living reproductive or germinal particles.

But the conclusion which Dr. Sanderson does draw from his experiments, and his imputation that facts do not warrant the conclusion of Prof. Huizinga and myself, would seem to imply that he is in possession of some new evidence subversive of his previous opinion, and tending to contradict views which I have recently published concerning the death-point of Bacteria in heated fluids. ("Proceedings of Royal Society," Nos. 143 and 145, 1873.)

As Dr. Sanderson is entirely silent upon this point, I venture to ask, both for my own information and for that of your readers, whether he still believes that Bacteria are killed by a temperature of 100° C. in fluids; and if not, upon what grounds he has changed his opinion?

In the face of his expressed intention (not a little contradicted, as I venture to think, by his public action) of taking no part in the "spontaneous generation" controversy, I ask Dr. Sanderson this question, because I cannot suppose that he would publicly throw doubt upon the validity of the conclusion which Prof. Huizinga and I have drawn from our experiments, in the absence of fresh evidence of his own upon the thermal death-point of Bacteria.

At present he has publicly expressed the opinion that we are not warranted in our conclusions, whilst he has given no sufficient information either to the world of science or to ourselves by which to test the correctness of his own conclusion. This seems neither just to us nor to himself.

H. CHARLTON BASTIAN

University College, Oct. 3

Mr. D. Forbes's Criticism of Mr. R. Mallet's Volcanic Theory

AFTER the lapse of half a year Mr. D. Forbes has recurred in NATURE for Sept. 4, 1873, to my remarks published in NATURE of March 20 last, to his remarks upon my Theory of Volcanic Energy and Heat contained in his review of my translation of Palmieri's "Incendio Vesuviano," which appeared in NATURE of February 6 preceding.

I pray your permission to make some remarks upon Mr. Forbes's last production. They are the last by which I shall prolong this unpleasant controversy.

Mr. Forbes affirmed that if anything was certain, it was that the ejecta of volcanoes in all ages and all over the world are *identical* chemically or mineralogically, and upon this assumption passes a summary condemnation upon my theory, which he predicts will never receive acceptance from anyone—chemist, or mineralogist, or geologist. This rash and I will now say discourteous prediction I at once disposed of by giving the names of two authorities, whose competence even Mr. Forbes could not question, who had already accepted my views.

To this Mr. Forbes now says, that, as these gentlemen possessed for their guidance in assenting to the bare statement of my views, no better information than that upon which he dissented from them, so they may have been mistaken and not he. How is Mr. Forbes sure they had no better information, and can it be possible that he is so dull in weighing the force of evidence as to see no difference in probability of error between two assumed equally competent men—one of whom can assent to a proposition upon his prior knowledge and without waiting for proof; and another, who dissents, before he has heard what can be advanced in favour of the proposition and against his own previous knowledge or supposed knowledge? This, however, is now immaterial except as an indication of Mr. Forbes's capacity for weighing evidence.

To Mr. Forbes's grand objection I replied that it is based upon error as to fact—that it is not true that all volcanic solid ejecta are identical at all times and everywhere.

While I denied, and do again deny, that identity, chemical or mineralogical, exists in those bodies, I admitted that they do present a great general resemblance—which is just what we should expect.

I added a very important remark, namely that whether it were true or false that all volcanic ejecta were identical, chemically or mineralogically—the fact, whether one way or the other, did not apply to or affect my theoretic views as to the nature and origin of volcanic energy and heat; one way or the other, the identity or dissimilarity between the ejecta as found at the surface must be the same, whether they be derived from materials already and constantly in fusion, or be fused by elevation of temperature locally and temporarily produced; the materials fused being the same in both cases.

This last objection, which is fatal to Mr. Forbes's criticism, whether the foundation on which he has rested it be true or false, he either has not noticed or finds it convenient now to ignore.

I illustrated the want of identity, chemical or mineralogical, and yet the great general similarity at all times and places of volcanic ejecta, by the analogy of the blast furnace, in which the same materials in the same proportions do not even in any one furnace, or at all times, produce identical slags.

What is Mr. Forbes's reply? That the *intention* of the iron master is to produce slags always the same, as the indication that the furnace is working well.

Doubtless the intention and desire of the iron-master is to produce good iron, and at all times as nearly as he can such a slag as indicates that he is doing so. But, as a matter of fact, he is not able to reach this. He can only approximate to constancy in the chemical or mineralogical constitution of his slags, which are never identical, even for short periods. Is this substitution of the intentions of the iron-master for the actual facts of the blast furnace slags, on Mr. Forbes's part, worthy of the candour of the searcher for truth; or does it not rather resemble the dialectic wriggle of the advocate?

Complete identity between any two rocky masses, ejected or otherwise, can only exist where the same elements in the same proportions are combined in the same way, and in the same molecular aggregation. If the mere presence in greater or less proportion in the mass, of certain crystallised minerals in any variable proportion, such as felspar, pyroxene, or leucite, in the magma of lavas, were enough to constitute identity, then nearly all the known rocks of the world, crystalline, igneous, and sedimentary, might be viewed as identical, for all consist of a few elements and of a few prevailing simpler minerals.

While still seeming to maintain his original statement, Mr. Forbes now substitutes for *identity*—a great *similarity* in all volcanic rocks. Further discussion is therefore needless—nor indeed would discussion of my views as to volcanic heat, &c., lead to any good result—with a gentleman whose notions of scientific method are such, that after six months' consideration he holds any distinction between hypothesis and theory to be mere hair-splitting, and whose notions of physico-mechanics are of that confused character, that he views pressure and work to be quite the same, and that it is matter of indifference whether we talk of "pressure converted into its equivalent, heat," or of work transformed into heat.

Would Mr. Forbes enlighten your readers by stating in figures what is the equivalent in heat, of the pressure of a weight of ten pounds, resting upon a rigid level plane?

Were Mr. Forbes of any real authority upon volcanic subjects, there might have been more ground for his sweeping and anticipatory condemnation of my views as to volcanic energy, which, however, in that case, he would never have uttered; but on looking down the list of his published papers, I do not find any treating of vulcanology simply, nor am I aware that he has ever enlarged the boundaries of our knowledge in that department by a hair's breadth.

Mr. Forbes appears to think that chemists, mineralogists, and geologists are the sole arbiters of all questions as to the nature and origin of volcanic heat and energy. Whatever they may have done to add to our knowledge of the visible and tangible phenomena of volcanic vents or cones, they have as yet contributed really nothing to discovering the nature and origin of volcanic heat itself, if we except some valuable negative evidence drawn from the gaseous emanations by chemists of late years, subversive of the older theories of the chemical origin of volcanic heat, still not quite extinct. It is much more to the physicist and theoretic mechanician dealing largely with the *physique du globe*, that we must look for further light, and whose province it will be to decide when the right key shall have been found to that enigma of ages, the true nature and origin of volcanic heat and energy.

I am done, sir, with this controversy, unwillingly entered upon, not in irritation, as Mr. Forbes states, but because I felt justified in protesting against new and I believe important views being obscured *in limine*, by objection based only on error.

My paper containing those views will ere long be before the world. My 100 separate copies (as author) from the "Phil. Trans." are already in the hands of or on the way to many men of science. The volume itself of the "Transactions" will no doubt appear before the end of the year, and to the verdict of the real men of science of the world, versed in the subject and competent to judge of it, I leave the result.

London, Oct. 6 ROBERT MALLET

On the Equilibrium of Temperature of a Gaseous Column subject to Gravity

FROM Mr. Clerk-Maxwell's reply to my note on this subject which appeared in your columns a short time since, it would appear that he does not profess so much fully to explain the difficulty suggested by me as to show that it is capable of explanation, referring your readers to his other works for further information. I would not, therefore, have troubled you further on the subject had it not occurred to me on reading Mr. Maxwell's letter that I could state the case in such a way as to render clearly apparent the grounds for taking different views on this point.

Let a vertical column of gas, subject to gravity and in a state of equilibrium as to pressure and temperature, be divided by a horizontal plane P into two parts, A above and B below.

In the time Δt let a mass M_1 of particles pass in their free course from A to B, and a mass M_2 from B to A.

Let the portion of A from which the particles composing M_1 proceed be called the upper stratum, and the corresponding part of B the lower stratum, then the following consequences may be deduced:—

1. From the equilibrium of density

$$M_1 = M_2 \; .$$

2. From the equilibrium of temperature the amounts of work in M_1 and M_2 while passing through P are equal.

3. From the effect of gravity the work in M while in A reckoning from the commencement of the free course of each particle composing M_1, is less than at P, while that in M_2 is greater.

4. Whence it follows that of the two equal masses M_1 and M_2 in the upper and lower strata respectively M_1 contains less work than M_2.

5. The work in M_1 while in the upper stratum reckoned as before, is the same as that of any other equal average mass in that stratum, and the same is the case also of M_2.

6. The average amounts of work in equal masses in the two strata, and the consequent temperatures of the strata are unequal, the lower stratum having the higher temperature.

I suppose Mr. Maxwell would deny the truth of statement (5). I presume he would argue as follows:—

"Of all the particles in the lower stratum which in the time Δt have at the commencement of their free course a velocity and direction such as would take them through P, gravity in selecting those which compose M_2 excludes those whose velocities are insufficient to overcome the effects of their weights, while in forming M_1 particles of low velocity are selected (included?), which, but for the effects of gravity, would not have cut P in their free courses, consequently the particles in M_1 have an average velocity less than that of the upper stratum from which they come, while the particles of M_2 have a greater average velocity than that of the lower stratum, and consequently the inequality of the average velocity of the particles in the two strata cannot be inferred from the inequality of the average velocities of the particles composing M_1 and M_2 while in those strata."

This argument, therefore, assumes the theory that in a given mass of uniform temperature there are particles moving with every velocity from nothing upwards to a certain limit, and mixed in certain proportions. That this is actually Mr. Maxwell's view I own I might have remembered, but I suppose I overlooked it from an impression in my own mind that the molecular motion was to be regarded as being of a planetary (or in the case of gases a cometary) nature. That in masses of the same temperature velocities were to be regarded as practically uniform, except in so far as affected by the distance of the particles apart, and that the so-called impacts of particles were more properly to be regarded as perihelion passages of bodies moving among each other in hyperbolic orbits. If this view is the more accurate one, then obviously the argument which I have assumed that Mr. Maxwell would use, falls to the ground.

Is there no possibility of testing the nature of the thermal equilibrium of a column of still air? The result would at any rate throw an unexpected light on the nature of molecular motion.

Graaff Reinet College, July 19 F. GUTHRIE

The Sphygmograph

DR. GALABIN, in his letter published in your last number, criticises my explanation of the cause of the small wave in the first part of the sphygmograph trace, which he calls the tidal wave. In his criticism he does not take into consideration the hæmodromograph traces of Chauveau, on which my explanation is entirely based, and without a reasonable interpretation of which no explanation can be considered satisfactory. The hæmodromograph trace proves that the "tidal wave" of Dr. Galabin has a shock origin, as I have shown in the "Journal of Anatomy and Physiology" (Nov. 1872), and that the dicrotic wave is its resulting tidal wave.

Dr. Galabin appeals to the "tidal wave" in the trace from the artery at the foot, in proof of his explanation; I have taken many from that locality, and find that the tidal wave is never represented at all (as my explanation requires), for it is thrown so far back that it becomes blended with the primary rise.

My explanation of the details of the cardiograph is questioned, because my tracings are said to have been taken with "a lever moving on a pivot, and balanced between two springs." Such was undoubtedly the case in my cardio-sphygmograph observations, but not in my paper on the cardiograph trace, when the instrument employed was, what Dr. Galabin recommends, the ordinary sphygmograph, applied to the chest-wall.

As long as Dr. Galabin has not full faith in the reliability of the sphygmograph and its indications, it is almost impossible to maintain an argument with him, for it is hardly worth discussing points which may be only the results of instrumental imperfections. These are now understood, and can be easily eliminated.

A. H. GARROD

Venomous Caterpillars

THE caterpillars mentioned by R. Benson in your paper of August 14, are not at all uncommon in Calcutta. One day my little girl was brought to me with what appeared to be a good sized hairy caterpillar under her arm, and crying as if in pain, and on my trying to remove it in a hurried way, I discovered that it was nothing but a mass of small hairs. The child had put her arm into an empty tub on the inner edge of which the caterpillar was crawling. As soon as she pressed it, she started as if she had been stung. All the servants crowded round the child and pointed to their heads, but as I was not a proficient in their language I could not make out what they meant. I tried to do what I could with my fingers to remove the hairs, but this seemed very painful, and the swelling round about kept increasing. The ayah, however, soon appeared, attracted by the child's crying, and seemed to know what was to be done. She got some of my hair, made a kind of small brush of it, and gently passed it over the injured part. In a few moments the hairs were all removed, and nothing was left but a white blister. This remained for two or three days and then subsided. In the Calcutta schools the boys call these caterpillars "woolly bears," and if stung by them ask for "a head," and a few rubs soon removes the disagreeable appendages.

C. H. C. B.

Calcutta, Sept. 9.

Harmonic Echoes

LORD RAYLEIGH'S notes on Harmonic Echoes recall to my recollection a little experience which I had in hearing what I supposed to be overtones reflected.

I have frequent occasion to cross a portion of an open public park in which there are few trees. When any sharp sounds are heard in the neighbourhood, as, for instance, the sound of the rod in the beating of carpets in a field near at hand, curious responses to the blows of the rod are heard, and these responses or echoes have not the same pitch as the originating sound. I was puzzled for some time to account for this echo in an open park, with almost nothing above the level of the grass but the iron railings, till I satisfied myself, by occupying various positions, that the echoes were reflections of sound from these narrow fences. But why the difference in pitch between the originating sound and the echo? This, I concluded, *might* result from the overtones of the sound being reflected from the thin iron bars which constitute the railing. It was also observable that it was only the sharp sound emitted by the beating rod which was echoed, and not the dull sound arising from the carpet when struck. The hands struck sharply together will also cause an echo from the fences, which is higher in pitch than the sound of the clapping hands. It would be very interesting to experiment on this point by sounding, at a proper distance, notes of known pitch before narrow, upright, or horizontal bars, and then ascertaining the pitch of the echo, and the relation of the latter to the size of the reflecting surface.

W. J. M.

Glasgow

IT appears tolerably well established that harmonic echoes are selective echoes; that is to say, echoes which, from whatever cause, select and return one of the harmonies of the original without the fundamental.

It may perhaps be found that there are other selective echoes than the harmonic kind. In one of the galleries of the very large parish church of Monkstown, co. Dublin, the sound of S is heard with peculiar intensity, both in the singing and in the responses. This is not an echo, but it may perhaps be a fact of the same kind with selective echoes.

Old Forge, Dunmurry JOSEPH JOHN MURPHY

Carbon Battery Plates

COULD you oblige me with information (or state where it could be obtained) respecting the process of manufacture of hard carbon battery plates, as I have some experiments on hand which necessitate the manufacture of plates of a peculiar shape, and I can neither get them made nor obtain sufficient information to enable me to make them well.

Warrington T. W. FLETCHER

Brilliant Meteor

ON the evening of September 7, at about 9.7 P.M., while walking in a northerly direction in one of the streets of Tiverton, I saw a very large and brilliant meteor slowly descend from east to west, but in an almost vertical direction. The sky was almost entirely covered with a thin veil of cloud, which obscured the stars, so that I was not able to note its course with reference to them; but the altitude of the point at which it first appeared was about 45°, its path was inclined to the vertical at an angle of about 5°, and it disappeared behind a roof at an elevation of about 20°, at a point about 90° to the north of the moon which could be seen through the clouds. The light of the meteor was greenish and flickering, and far exceeded in intensity that of Venus when at her maximum brilliancy, but I could not see any train.

T. PERKINS

Reading School

NORTHERN LIMIT OF PHANEROGAMIC VEGETATION

CAPTAIN MARKHAM has most kindly presented to the Herbarium of the Royal Gardens, Kew, a small but very interesting collection of plants brought back by him from his recent Arctic voyage. Amongst them are four specimens which he obtained from Dr. Bessel, who collected them in lat. 82° N., the most northern position from which any phanerogamic vegetation has hitherto been procured. The locality appears to have been on the east side of Smith's Sound. The species are *Draba alpina*, L.; *Cerastium alpinum*, L.; *Taraxacum Dens-leonis*, Desf. var.; *Poa flexuosa*, Wahl.

JOS. D. HOOKER

THE WEALDEN BORING

THE readers of NATURE will be interested in learning that the lowest beds now reached by the Sussex boring are not Wealden, but of marine origin; that the most distinct of the shells yet examined by me is a Lingula, that it is *Lingula ovalis*, a shell of the Kimmeridge clay. The specimens which contain it were placed in my hands by Mr. Peyton, with Mr. Willett's consent. We are, in fact, already below the Wealden, in the pelagic sea-bed far from its ancient shore.

J. PHILLIPS

THE NEW MARINE ANIMAL FROM WASHINGTON TERRITORY

AT the meeting of the British Association in 1872, I exhibited before Section D specimens of some long white bodies resembling peeled willow-wands, which I had received from Barraud's Inlet, Washington Territory, with the information that they were the "backbones of a fish." Subsequently I published what intelligence I could collect upon the subject in this journal,* and urged the expediency of further investigation in order to discover the true nature of these curious objects. I also called the attention of various correspondents in America to the same subject, and sent them copies of the article in NATURE.

It appears that the problem has now been satisfactorily solved, and that Prof. Kölliker, Mr. Mosely, and other naturalists, who held that these organisms were the axes of an unknown Alcyonarian polyp of the family Pennatulidæ were correct.

In a paper communicated to the Californian Academy of Sciences on the 18th of August last, of which I have received a separate copy, Mr. R. E. C. Stearns states that a specimen of the Polyp, of which these bodies are the axes, had been presented to the Academy by Dr. James Blake. Mr. Stearns describes the polyp at full length, and proposes to call it *Verrillia blakei*. He describes the general aspect of the species as resembling that of *Pavonaria quadrangularis*, but states that the polyps are arranged in "two unilateral longitudinal series."

I may add, that a communication from Dr. Edward L. Moss on the same subject, has been received by the Zoological Society of London, and will be read at one of the meetings next session. P. L. SCLATER

THE RAY SOCIETY†

THE Council, in presenting their thirtieth Annual Report, congratulate the members upon the continued prosperity of the Society.

The lapse of time, so marked by the production of a long series of volumes on zoology and botany, issued under the auspices of the Society, has scarcely lessened the original dimensions of the Printed List of Monographs in preparation and in progress; the completion of old memoirs being ever counterbalanced by offers of works from new authors. A recent proposal by Mr. G. B. Buckton to describe the British Aphides is a case in point. This addition will occupy the place left void by the publication of Sir John Lubbock's very valuable and interesting contribution to the study of insect life.

Since the last annual meeting some attempt has been made, not unsuccessfully, to reduce the arrears in the issue of the volumes. The monograph for the year 1871, the "Collembola and Thysanura," by Sir John Lubbock, Bart., M.P., has already been distributed to the members; the work for the year 1872, the "British Annelids," Part I., containing the Nemerteans, by Dr. W. C. McIntosh, has been so far finished that it will be ready in a few weeks' time for the binder; whilst the volume for the year 1873, the "Spongiadæ," vol. iii., by Dr. Bowerbank, is, with the exception of a single plate, completed.

The Council have considered that it would be to the advantage of the Society if members could obtain the past annual volumes at the original (or in some cases at less than the original) subscription price. With this view resolutions have been passed: first, that the annual volumes, or sets of annual volumes, issued during the last ten years should be purchasable by members at the subscription price of one guinea; and, secondly, that the books in stock, published earlier than the year 1863, should be supplied at a lower cost than that named in previous reports; and, thirdly, that certain of the volumes belonging to the years 1865, 1866, 1867, and 1868, formerly not distributed separately, should be offered to members for sums less than that of the year's subscription.

In accordance with these resolutions, a list of books and prices has been prepared. The volumes may be obtained on application to the secretary.

The volumes in preparation for future years are:—

Mr. St. George Mivart's "Monograph of the Tailed Amphibia."

Rev. O. P. Cambridge's supplementary volume on "British Spiders."

Messrs. Douglas and Scott's work on the "British Hemiptera Homoptera."

Dr. Gaertner's work on "Hybridism in Plants" (Bastarderzeugung), translated from the German by W. Carruthers, F.R.S.

Prof. Haeckel's "Morphologie." A new edition, revised by himself, and translated from the German.

Mr. Hancock's Monograph of the "British Tunicata."

Mr. Andrew Murray's work on the "Coniferæ."

Rev. H. B. Tristram's "Synopsis of the Fauna and Flora of Palestine."

Prof. Westwood's Monograph of the "Mantidæ," with illustrations by Mr. E. A. Smith.

Mr. Buckton's Monograph on the "British Aphides."

The Council, in conclusion, would urge the members to assist in the work of obtaining new subscribers, seeing that very many old friends are being removed from the list of the Society year by year through death and various causes.

ON THE INTERNAL NOSE OF THE PECCARIES AND PIGS

IN examining the sections of the skulls of the Wild Boar the Babirussa, the Phacochoer, and the Peccary, I was struck with the great difference in the form and development of the internal part of the organ of smelling of the peccary as distinguished between it and the other genera.

The Wild Boar, Babirussa, and Phacochoer, have the nasal cavities on each side of the head large, broad, and continued from the outer to the internal nostrils in a simple manner, and they are only separated from the palate by a thin bone, as they are in the sheep and the generality of allied animals. In these animals the turbinal bone arises from the centre of the outside of each nasal cavity, and is divided above into two plates which are rolled backwards, towards the outer side of the nose. There is a perforation between the hinder edge of the intermaxillary bone and the palatine bone in front of the palate behind the cutting teeth which opens directly into the front of the nasal cavity just within the nostrils, as figured in Huxley's "Elementary Atlas," t. i. 4 *d*.

In the peccary the internal nostrils open into a small cavity, which soon becomes tubular, pervading a large hollow cellular part which occupies the space above the palatine bones, and then gives off a large opening on the outer side to the turbinal bones, and is continued in a smaller tube to a small opening on each side of the front part of the palate, behind the cutting tooth. This aperture is evidently analogous to the large perforation in front of the palate of the pigs, but is quite of a different structure. There is a cavity further in near the external nostrils, which forms an opening to the pituitary convolutions, to which I see nothing like in the skull of the pigs. The naso-turbinal is fixed by its upper edge to the upper part of the nasal cavity, and is rolled inwards, and there is a lamina on the lower side from the expanded part of the tubular internal nostril, which meets the one from the upper edge. The whole structure of this part is quite different from that in the pigs, and Phacochoer, and justifies the separation of the Peccaries as a different group from the pigs. I may also remark that in this genus there is a well-marked bony plate on each side of the brain cavity, that separates the edge of the cerebrum from the cerebellum. This septum is only slightly marked in the skull of the wild boar, and is entirely absent in the Babirussa and Phacochoer.

J. E. GRAY

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* See NATURE vol. vi. p. 436.

† Extracted from the Report.

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE*

VI.

AT the time when the metric system was originated, the French standards of weights were the series known as the *Pile de Charlemagne*, the unit being the *Livre poids de marc* of 16 *onces*, and double the *poids de marc*. The metric equivalent of the *poid de marc* was subsequently determined to be 244·753 grammes. The *once* was divided into 8 *gros* (or drachms), and the *gros* into 72 *grains*. The old French *Livre* of 9216 French *grains* was therefore equal to 489·506 grammes, and 7554 English troy grains. The French *grain* was thus equal to 0·818 English troy grain. In determining the new unit of metric weight, it was necessary to ascertain the actual value in terms of the existing system of the *livre* and its subdivisions, of the provisional weights used; and from accurately comparing them with the old standards, it was deduced from the ascertained weight of the measured cylinder, that the weight of a cubic decimetre of distilled water at its maximum density, or at 4° C., which was 0·9992072 of the provisional kilogram, was equal to 18827·15 grains of the *poids de marc*. This, accordingly, was definitively adopted as the true weight of the kilogram, the new unit of metric weight.

The determination by the French Commission of the weight of a cubic decimetre of water at its maximum density differs somewhat from later authoritative determinations made in England and other countries, as may be seen from the following tabular statement:—

Date.	Country	Observer.	Weight of cubic decimetre of distilled water at 4° C.
			Grammes.
1795	France .	Lefevre-Gineau . . .	1000·000
1797 & 1821	England	Shuckburgh and Kater	1000·480
1825	Sweden .	Berzelius, Svanberg, and Akermann . .	1000·296
1830	Austria .	Stampfer	999·653
1841	Russia .	Kupffer	999·989
		Mean	1000 084

But the latest and most carefully executed determination by Kupffer agrees so closely with the French determination, that the actual weight of the primary kilogram may be taken as nearly identical with its theoretical definition, and sufficiently accurate for all practical purposes.

From the provisional brass kilogram, with its error thus ascertained by the French Commission, two new standard kilograms were constructed by Fortin, one of platinum, the other of brass, and each was determined, after numerous comparisons and the requisite corrections, to be of the true weight when weighed in a vacuum. The platinum weight was constituted the primary metric standard kilogram, and is known as the *Kilogramme des Archives*. Its form is that of a cylinder of about 39·4 millimetres in diameter, and 39·7 metres high, having its edges slightly rounded, being similar to that of the English platinum kilogram shown of the actual size in Fig. 12. The density of the *Kilogramme des Archives* has never been precisely determined, as it has been deemed hazardous to weigh it in water from a fear of its not being entirely free from the arsenic used in preparing the platinum, and of dissolving this arsenic, and thus diminishing the weight of the kilogram. Prof. Miller has assumed the volume of the *Kilogramme des Archives* when in its normal temperature of 0° C to be equal to the volume of 48·665 grammes of water at its maximum density, as determined by its cubic measurement, and consequently its density to be 20·5487. Other computations, however, differ slightly from this determination.

The brass kilogram was intended as the commercial standard, for regulating all ordinary metric weights in air, and was deposited at the Ministère de l'Intérieure Paris. One uniform shape is adopted in France for all brass kilograms. They are made in the form of a cylinder surmounted with a knob. The height of the cylinder is equal to its diameter, and the height and diameter of the knobs are equal to one half those of the cylinder. Like the platinum *Kilogramme des Archives*, the brass standard kilogram was never weighed in water, and its volume has been computed from its cubic measurement to be equal to that of 124·590 grammes of water at its maximum density, thus making its density 8·206. In our standard air, $t = 62°$ F. $b = 30$ in., the platinum standard kilogram will thus displace 59·25 milligrams of air, and the brass kilogram 151·75 mgr.; the apparent weight in air of the brass kilogram is consequently about 92 mgr. less than that of the platinum standard. This brass kilogram was assumed by the French Commission to be 88·5 mgr. lighter than the platinum standard, when weighed in ordinary air.

The primary platinum metre and kilogram were presented by the Commission on June 22, 1799 to the Corps Legislatif at Paris, and were legally constituted as the standards of length and weight of the new metric system throughout France by the law of Dec. 9, 1799. They were deposited at the Palais des Archives.

A platinum copy of each of the primary metric standards of the metre and kilogram was constructed at the same time, and deposited at the Paris Observatory. These standards, known as the *Mètre de l'Observatoire*, and the *Kilogramme de l'Observatoire*, were considered as next in authority to the primary standards.

The unit of capacity of the metric system, the *litre*, represents theoretically the measure of volume of a cubic decimetre, or the cubic contents of a metallic vessel of this capacity when at the temperature of melting ice. But practically, there is no material primary standard litre, and the legal measure of the litre is determined from the kilogram; that is to say, the litre actually is a measure containing a kilogram weight of distilled water at its maximum density. Such a measure can only be verified by computation, as the vessel itself must be taken at a different temperature from the water contained in it, the vessel at 0° C., the water at 4° C. Authoritative tables are therefore prepared for ascertaining the allowance to be made in every case for differences of temperature from the normal temperature, as well as for the difference of weight of air displaced by the metallic weight and the larger volume of water.

For metric measures of surface, the *are*, equal to 100 square metres in the unit; and for solid measures, more particularly for measuring wood, the *stere*, or cubic metre, is the unit.

The number and denominations of the metric weights and measures actually used in France and other countries, for which specific standards are provided, are as follows. they include the double and the half of each decimal unit, with a duplicate unit to make up the number 9 units:—

6 Metric Measures of Length .
- Double metre
- Metre, divided into tenths or decimetres, &c.
- Half-metre, " "
- Double decimetre, divided into centimetres and millimetres
- Decimetre, " "
- (For land) Chain of double dekametre, or 20 metres, divided into metres, and links of 2 decimetres

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* Continued from p. 389.

30 Metric Weights	20, 10, 5, 2, 1, 1	kilograms	
	500, 200, 100, 100	grammes	(hectograms)
	50, 20, 10, 10	"	(dekagrams)
	5, 2, 1, 1	"	
	0·5, 0·2, 0·1, 0·1	gramme	(decigrams)
	0·05, 0·02, 0·01, 0·01	"	(centigrams)
	0·005, 0·002, 0·001, 0·001	gramme	(milligrams)

13 Metric Measures of Capacity	Hectolitre,	or	100	litres
	Demi-hectolitre,	"	50	"
	Double dekalitre,	"	20	"
	Dekalitre,	"	10	"
	Demi-dekalitre,	"	5	"
	Double litre,	"	2	"
	Litre,	"	1	litre
	Demi-litre,	"	0·5	"
	Double decilitre,	"	0·2	"
	Decilitre,	"	0·1	"
	Demi-decilitre,	"	0·05	"
	Double centilitre,	"	0·02	"
	Centilitre,	"	0·01	"

Total number of metric weights and measures used in France and other countries, 49.

For dry commodities, the demi-dekalitre is the smallest measure used. The litre being equal to a cubic decimetre, or 1,000 cubic centimetres, in volume, is also equal to 1,000 grammes weight of distilled water at its maximum density; consequently the

Demilitre	=	500	cubic centimetres, or	grammes weight of water.
Double decilitre	=	200	"	"
Decilitre	=	100	"	"
Demi-decilitre	=	50	"	"
Double centilitre	=	20	"	"
Centilitre	=	10	"	"

There are also graduated measures of 5, 2, and 1 cubic centimetres or grammes weight of water.

The earliest recognition by the British Parliament of the metric system thus established in France took place soon after the close of the war. On March 15, 1816, Mr. Davies brought forward a motion in the House of Commons, which was carried, for comparing the imperial standard yard with the French standard metre. The Government entrusted the necessary operations to the Royal Society, who obtained for the purpose two platinum metres from Paris. These had been verified by M. Arago, by comparison with the French standard. One was an end-standard, like the "Metre des Archives," but was nearly twice as thick, being 7·3 milimetres in thickness. On one plane surface the word "METRE" is engraved, and [illegible] the other "FORTIN A PARIS," and "Royal Society, 44." This end-standard was determined to be exactly the length of a metre at the temperature of melting ice. The other was a line standard, the bar being nearly equal in width, but only 5·3 millimetres thick, and it is about 4 centimetres longer. On the upper surface is engraved "Royal Society, 45," and transverse lines, so fine as hardly to be seen with the naked eye, are cut about 2 centimetres from each end for defining the length of the metre, as shown in the following figure :—

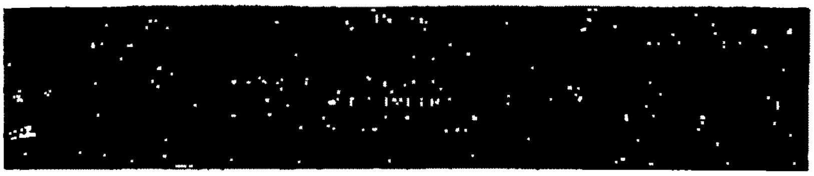

Fig. 11.—Decimetre and its nearly equivalent length of four inches

The length of a metre is to be taken between the two transverse lines at the mid-width of the bar, and it has been determined to be less than a metre by 0·01759 millimetre, taken at the standard temperature of melting ice.

On being brought to this country, the two platinum metres were carefully compared by Captain Kater with the length of 39·4 inches on the Shuckburgh scale, considered by him to be the British scientific standard of length. Full details of the comparisons made with Captain Kater's microscopical comparing apparatus are given in Phil. Trans. 1818. It was required to determine the length of the platinum metre at its standard temperature of 32° Fahr. in terms of the brass standard yard of 36 inches at its standard temperature of 62° Fahr. Allowance was made for the different rates of expansion of the two metals, the co-efficient of expansion of the platinum being taken to be 0·00000476 for 1° Fahr., as determined by Borda, and that of brass 0·0000101, as found by Kater's experiments. The length of the metre at 32° Fahr. was thus determined from the *mètre à bouts* to be 39·37086 inches of the Shuckburgh scale at 62° Fahr., and from the *mètre à traits* 39·37081 inches, after allowing for its error = 0·00069 inch. The mean length of the metre was therefore 37·37084 inches of the Shuckburgh scale, and as this scale had been found 0·00005 inch longer than the Parliamentary standard, the true length of the mètre was finally determined by Captain Kater to be 39·39079 British inches.

Ever since this period, this authoritative equivalent of the metre in imperial measure has been recognised as the true equivalent, and it received the sanction of Parliament, in the Act of 1864, for legalising contracts made in this country in terms of the metric system. It is, however, to be observed that it is the *scientific* equivalent of the metre in imperial measure. For all *commercial* purposes, on the other hand, the measure of a metre is always used at ordinary temperatures just as a yard measure is used, and the comparison of the two should therefore be more properly made at the same average temperature of 62° F. At such temperature a brass metre is equal to 39·382 inches, and this length is to be taken as the commercial equivalent of the metre in British measure. Of course, this difference of the equivalent in imperial measure of the metre at its legal and at its ordinary temperature, amounting only to $\frac{3}{1000}$ inch is perfectly immaterial in commercial measurements of small quantities, and the metre may safely be estimated as equal to $39\frac{3}{8}$ of our inches, and the decimetre at 3·94 inches, as shown in Fig. 11.

No satisfactory comparison of the primary kilogram with our unit of imperial weight was made until the year 1844, after the construction of the new imperial standard pound, under the authority of the Standards Commission. The comparison of the standard units of weight of the two countries was then undertaken by Prof. Miller, at the request of the Commission. He found that previous determinations of the weight of the kilogram varied amongst themselves from a minimum of 15432·295 gr. to a maximum of 15438·355 grains. Under these circumstances, he proceeded to Paris in the autumn of 1844, and obtained permission from the French Government to compare the Kilogramme des Archives with our English weights. For the comparison, he took with him the Parliamentary copies Nos. 1 and 2 of the standard pound, and two auxiliary platinum weights together, equal to about 1432·35 grains. The mean result of 60 comparisons was to find the Kilogramme des Archives epual to 15432·34813 grains. But Prof. Miller was not satisfied with this result, as one of the auxiliary weights was found to contain a small cavity filled with some hygroscopic substance, which rendered its weight slightly variable.

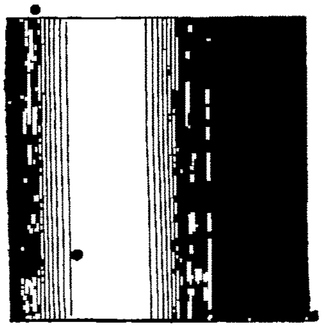

FIG. 12.—Platinum Kilogram 𝕮.

He therefore considered it requisite to make further comparisons directly with the English standard pound.

For this purpose, a platinum kilogram, constructed by Gambey, was procured at Paris by Prof. Miller, and was accurately compared by him with the Kilogramme des Archives. This platinum kilogram, designated as 𝕮 by Prof. Miller, is similar in form to the prototype, but is a little smaller, in consequence of the somewhat greater density of the platinum of which it is composed. Its density was determined by hydrostatic weighings to be 21·13791. From the mean of 100 direct comparisons with the Kilogramme des Archives, 𝕮 was found to be lighter in a vacuum than the French standard by 1·56 mgr. (0·02412 gr.). For ascertaining the weight of 𝕮 in terms of the new imperial standard pound, Prof. Miller subsequently compared this kilogram with the imperial standard pound, together with each of its Parliamentary copies successively, and one of four auxiliary platinum weights, each of 1432·324 grains, constructed for the purpose, and accurately verified in terms of the imperial standard, by means of supplementary platinum weights. The mean result of 166 direct comparisons of 𝕮 was to find its value = 15432·32462 grains. The Kilogramme des Archives was consequently determined to be equal in a vacuum to 15432·34874 imperial grains, or 2·20462125 standard platinum lb.; and the imperial standard pound equal to 453·5926525 metric grammes. These equivalents have since been generally accepted, and were legalised in this country by the Metric Act, 1864.

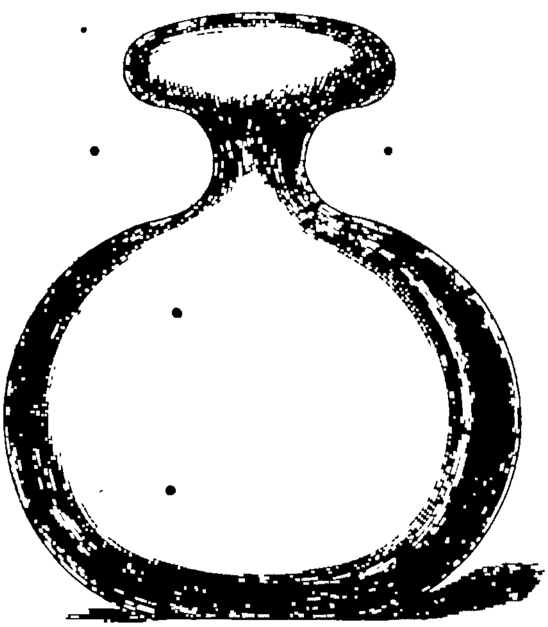

FIG. 13.—Gilt Gun-metal Kilogram 𝕳.

The platinum kilogram 𝕮 has since been deposited in the Standards Department, together with a second kilogram, of gilt gun metal, also made under Prof. Miller's directions, and intended as a standard for the adjustment of commercial metric weights, like the French *kilogramme laiton* deposited at the Ministère de l'Intérieure at Paris. This gilt gun metal kilogram was constructed by Oertling and has been denoted as 𝕳 by Prof. Miller. Its form is spherical with a knob. Its density is 8·3291°. The mean result of 24 comparisons with 𝕮 showed that in a vacuum the weight of 𝕳 was 1·47 mgr. less than 𝕮, and 3·04 mgr. less than the Kilogramme des Archives. In standard air (*t.* = 18°·7 C., *b.* = 755·64 mm.) 𝕳 displaced 143·92 mgr. and the Kilogramme des Archives 58·36 mgr. 𝕳 was then found to be 88·6 mgr. lighter in air than the French platinum prototype, and only 0·06 mgr. lighter than the French commercial brass standard kilogram.

Although the metric system was established in France as the legal system of weights and measures in 1799, it was not until more stringent provisions of law for enforcing its exclusive use were passed in 1837, that metric weights and measures began to be generally adopted in that country. Since that period it has been gradually adopted in other countries, and there is now every prospect of its finally becoming universally in use, and being acknowledged as an international system of weights and measures. Attention has been already drawn in NATURE, vol. vii. p. 197, to the proceedings of the International Metric Commission at Paris for the construction of uniform metric standards for all countries who have adopted or contemplated the adoption of the metric system, as well as to the material, an alloy of platinum and indium, adopted for the new standards, and the peculiar form of the new International standard metre. It will therefore be sufficient here merely to show the adopted form of the new standard metres, as compared with that of the existing Standard Metre des Archives, in the following figures, all of the actual size:

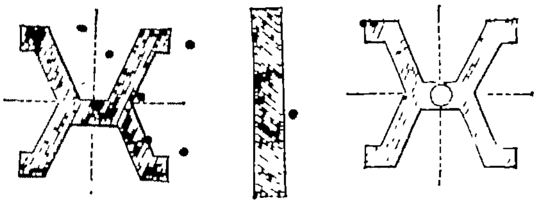

FIG 14.—Form of New Standard Metres.

The form of the new International kilogram will be the same as that of the Kilogramme des Archives, a cylinder of equal diameter and height, with the edges slightly rounded, as already described.

H. W. CHISHOLM

(*To be continued.*)

NOTES

A LETTER has been addressed by Dr. Anton Dohrn to the Colleges, and other bodies of the University of Oxford, giving an account of the cost, extent, and purposes of his zoological establishment at Naples, pointing out the incalculable advantages furnished by the establishment to students of biology, and urging that at least one out of the many fellowships belonging to Oxford should be devoted to the purpose of affording a suitable man the opportunity of pursuing the practical study of biology at the Naples station. We have already printed the Report presented to the British Association by M. Dohrn, from which it will be seen that the University of Cambridge has hired a table; we believe the University of Oxford has refused to do so, and hence this appeal to the separate Colleges, by M. Dohrn. Let us hope that at least one of these bodies will come forward and maintain the credit of the University.

PROF. HENRICI'S Introductory Address for the session at University College, delivered on Thursday last, dealt chiefly with the distinction between the results of Mathematical teaching in Germany and in England; that while in Germany almost every great mathematician (as an example the late Prof. Clebsch was pointed out) was the founder of a school, in England, on the contrary, no mathematical school had been founded in recent times. This the lecturer did not attribute to the paucity in this country of mathematicians of the very highest eminence,—indeed the names of Sylvester, Cayley, and Sir Wm. Thomson are alone sufficient to show that no country of Europe is ahead of England in this respect,—but rather to the want of personal influence exercised by them on younger minds, which has become almost impossible by the antiquated institutions of our ol d Universities. While the number of mathematical students at Cambridge exceeds that at a large number of German universities put together, the proportion of these students who are pursuing their studies for any higher purpose than that of taking a good degree—after which they allow them to be all but entirely neglected—is very small; and hence England is lamentably deficient in mathematical inquirers of the second and third class. Without wishing to see the German system introduced into this country in its entirety, Prof. Henrici pointed out some of the defects of our English system which he considered to conduce to this end; especially the encouragement given by the mode of examination to "cramming;" the small number of professorships; the fact that the remuneration of the professors is to a great extent dependent on the student's fees, and hence the comparatively high scale of charges; the slight encouragement given to the pursuit of pure science as a means of livelihood; and, above all, the want of that personal communication and interchange of ideas between teacher and pupil which tends so greatly to a promotion of the love of science.

A SPECIAL Meeting of the Council of the Society of Arts was held on Wednesday, Oct. 1, to consider the subject of National Museums and Galleries, and their bearing on public education. A Standing Committee was appointed for the purpose of bringing under Parliamentary responsibility the national museums and galleries, so as to extend their benefits to local museums, and to make them bear on public education. The following are the several objects in view for effecting this purpose:—1. All museums and galleries supported or subsidised by Parliament to be made conducive to the advancement of education and technical instruction to the fullest extent, and be made to extend their advantages to the promotion of original investigations and works in science and art. 2. To extend the benefits of national museums and galleries to local museums of science and art which may desire to be in connection, and to assist them with loans of objects. 3. To induce Parliament to grant sufficient funds to enable such objects to be systematically collected, especially in view of making such loans. 4. For carrying out these objects most efficiently, to cause all national museums and galleries to be placed under the authority of a minister of the Crown, being a member of the Cabinet, with direct responsibility to Parliament; thereby abolishing all unpaid and irresponsible trustees, except those who are trustees under bequests or deeds, who should continue to have the full powers of their trusts, but should not be charged with the expenditure of Parliamentary votes. 5. To enter into correspondence with all existing local museums and the numerous schools of science and art (including schools for music) now formed throughout the United Kingdom, and to publish suggestions for the establishment of local museums. 6. Also to cause the Public Libraries and Museums Act (18 and 19 Vict. c. lxx.) to be enlarged, in order to give local authorities increased powers of acting. We congratulate the Society of Arts on the step it has taken: we believe it is the most important piece of work it has ever set its hands to.

THE Council of the Society for the Promotion of Scientific Industry, looking to the enormous waste there is in the consumption of coal, whilst its cost is every day increasing, have resolved that an exhibition shall be held in Manchester of all appliances and apparatus, that tend to the economic use and saving of fuel, for the purpose of inducing attention to, and eliciting opinions of practical men on the matter, and of giving all consumers of coal an opportunity of comparing the various appliances, with a view to their adoption of that which will best serve their purpose. The exhibition will comprise:—1st. Appliances which may be adapted to existing furnaces, &c., whereby an actual saving is effected in the consumption of fuel. 2nd. Appliances which may be adapted to existing furnaces, &c., whereby waste heat is utilised. 3rd. New steam generators and furnaces, boilers and engines specially adapted for the saving of fuel and appliances, whereby waste products are utilised, and the radiation of heat prevented, &c., &c. The exhibition will include appliances used for manufacturing, agricultural, and domestic purposes. Either the apparatus itself, or diagrams, or models may be exhibited, and no limit is placed upon the class of articles to be exhibited. Exhibitors will be required to deliver their exhibits free of charge at the place of exhibition, and to remove them at the close of the exhibition; they must also erect them if necessary at their own expense. Every exhibit must be accompanied by a full description, which must include a statement of the particular work the apparatus is intended to perform. A duplicate of this statement must be handed in when application is made to exhibit. Exhibitors will be given every opportunity of explaining the speciality of their apparatus. All articles are exhibited at the risk of the exhibitor, though every reasonable care will be exercised. Further information may be obtained from the secretary of the society.

SIR SAMUEL AND LADY BAKER, with their nephew and some black servants, arrived at Paris on Monday morning, *en route* for London. The whole of them are in excellent health, and bear strong traces of exposure to an African sun. Interesting information concerning Sir Samuel's work in Africa, will be found in the *Daily News* of the 7th and 8th inst.

SIR HENRY RAWLINSON delivered the inaugural address on the commencement of the winter session of the Midland Institute at Birmingham on Monday evening. Referring to Arctic explorations, he said he indulged the hope that the year will not close before an assurance has been given that the *Challenger* Expedition will be supplemented by the despatch during next spring of a well-equipped Admiralty vessel which will be commissioned to endeavour to reach the Pole by pushing through Smith's Sound from Baffin's Bay in the track of the American ship, *Polaris*, whose fate has recently elicited so much sympathy throughout England.

SURGEON E. J. MILLIGAN, of the steamship *Africa*, writes from Sierra Leone, on the 12th ult., to the *Irish Times*, stating that on August 17, when returning from Loanda homeward, they steamed up the River Congo, and when at Banana one of the passengers, M. Cressy, received a letter from a friend stationed 300 miles up the river. It contained the intelligence that about 200 miles farther in the interior a white man, accompanied by a number of native attendants, was proceeding in the direction of the West Coast. His supplies becoming short, he was prevented from proceeding by a tribe, and retained prisoner until some should be secured. From the description given by the native traders to M. Cressy's friend of this person, and also from the fact that no other white man is known to be in this region, it is generally inferred that it is Dr. Livingstone.

We regret to record the death of Sir Paul Edmund de Strzelecki (perhaps better known as Count de Strzelecki) who died on Monday morning at his residence in Savile Row, at the age of 77 years. Early in life he was a great traveller, and explored a great portion of Australia. He was elected a Fellow of the Royal Society in June, 1853, was a D.C.L., and a member of several of our learned societies.

Prof. Watson, of Ann Arbor, telegraphs to the Detroit *Tribune:* "On July 24 I observed a star of the twelfth magnitude, which, on Saturday night last (August 16), was missing from the place where first seen. A little to the west I saw a star of the eleventh magnitude, which proves to be the new planet (No. 133), and at present I suppose it to be that seen July 24."

The Fungus-show at the Royal Horticultural Society on Oct. 1st was a great success; never had there been a greater or better arranged display of these plants, classified under the two sections of "edible" and "poisonous." A new economical use for this class of plants was indicated by the Rev. Mr. Berkeley, who produced a cap made out of the beaten out interior mass of *Polyporus fomentarius*, the amadou or German tinder of commerce, which he described as both warm and light. It is stated that large use is made in Hungary of this material for caps and waistcoats, and it is also used for caulking boots.

One of the important and beautiful publications which characterise the Smithsonian Contributions to Knowledge is just issued under the title of "A Contribution to the History of the Freshwater Algæ of North America," by Horatio C. Wood, jun., M.D.

Now that so much attention is being paid to the introduction into our colonies of useful foreign trees and crops, we desire to call special attention to the publication at Brisbane of "The Olive and its Products: a treatise on the habits, cultivation, and propagation of the tree, and upon the manufacture of oil and other products therefrom," by L. A. Bernays, F.L.S., Vice-President of the Queensland Acclimatisation Society. The work has special reference to the advantages to be derived from the introduction of the olive into Queensland, and is printed and published at the expense of the Colonial Government.

News has been received to the date of May 1, from Mr. Henry Elliott, who has been engaged for two years past in making explorations and observations in the fur-seal islands in the Behring Sea. He announces the continued prosecution of his labours, the results of which were transmitted to the National Museum in the summer of 1872. He has especially devoted himself to an investigation of the habits of the fur-seal, walrus, and sea-lion, and has made a topographical survey of the rookeries upon a portion of the islands on which these animals come to bring forth their young. His work in 1872 was devoted mainly to St. Paul Island, but he expected, very soon after the date of his letter, to visit St. George and the other islands of the group, there to prosecute similar inquiries.

We consider it extremely creditable to the *Leeds Daily News* that it chronicles regularly and at considerable length the proceedings of the Leeds Naturalists' Field Club and Scientific Association, and we should like to see other provincial, and indeed metropolitan papers follow *its example*. The principal paper read at the Society's meetings during September was by Mr. James Abbott, on the structure and development of the Hepaticæ. The Society continues, we are glad to see, to investigate very thoroughly the natural history of the district.

We heartily endorse the following sentiment of the *Athenæum* in reference to the meeting of the British Association:—"The opinion is gradually forcing itself upon many of those who attend the meetings of the Association that some change in its method of procedure is becoming necessary. For the scientific men, on whom rests, more or less, the responsibility of keeping up the sectional business, either by doing official work or attending the meetings and taking part in the discussions, the labour is too exacting on an occasion which should have something of relaxation about it. Again, the tendency of the papers is necessarily to take a technical direction, which must put them beyond the range of the non-scientific audience. The sectional business is consequently unsatisfactory, both to those who take part in it and to those who attend as listeners. The Association should fulfil two functions—first, that of bringing together scattered scientific men, who otherwise rarely or never meet; secondly, of giving the general public some idea of what the scientific world is doing. For the first object, more leisure is required during the meetings—more opportunity of talking over amongst themselves the work which different men are occupied with. To attain the second object, instead of miscellaneous papers, short addresses, carefully prepared, might be delivered, with one or two invited speakers to follow. These addresses should be given at morning meetings, which might advantageously break up at one, leaving the afternoons free.

Messrs. Sampson Low, Marston, and Co. announce the following books to be published during the forthcoming season:—"The Heart of Africa; or, Three Years' Travels, Discoveries, and Adventures in the Unexplored Regions of the Centre of Africa," by Dr. George Schweinfurth. The district explored by Dr. Schweinfurth embraces the wide tract of country extending southward from the Meschera on the Bahr el Ghazal, and between the 10th and 3rd degrees of north latitude. The work will form two large octavo volumes, and will be illustrated by about 130 woodcuts from drawings made by the author during his journey.—"A Whaling Expedition to Baffin's Bay and the Gulf of Boothia. With an account of the rescue by his ship of the survivors of the crew of the *Polaris*," by Captain Markham, with maps and illustrations. The maps to this work will give the first authentic delineation of Hall's discoveries, and also contain several important corrections of the old charts.—"The Land of the White Elephant; or, Lights and Scenes in South-Eastern Asia," being a personal narrative of travel and adventure in Farther India, embracing the countries of Burma, Siam, Cambodia, and Cochin-China, by Frank Vincent, jun., with maps and plans.—"The Wild North Land," a winter journey with dogs across Northern North America, by Captain W. F. Butler, with a map; and a new work on Peru by Thos. J. Hutchinson, F.R.G.S., entitled, "Two Years in Peru, with Exploration of its Antiquities."

Messrs. Trübner's List of forthcoming books includes the following scientific works:—"From the Indus to the Tigris:" a narrative of a journey through the countries of Balochistan, Afghanistan, Khorassan, and Iran in 1872; together with a synoptical grammar and vocabulary of the Brahoe language, and a record of the meteorological observations and altitudes on the march from the Indus to the Tigris, by H. W. Bellew, C.S.I., Surgeon to the Bengal Staff Corps. "The Rod in India;" being hints how to obtain sport, with remarks on the natural history of fish, otters, &c., and illustrations of fish and tackle, by H. S. Thomas, F.L.S., F.Z.S. A third and enlarged edition of the "Celt, the Roman, and the Saxon," a history of the early inhabitants of Britain, down to the conversion of the Anglo Saxons to Christianity, illustrated by the ancient remains brought to light by recent research, by Thomas Wright, M.A., F.S.A.

A DEPUTATION from the Trades' Guild of Learning waited on Tuesday afternoon on a Sub-Committee of the London School Board, at the invitation of the School-Management Committee, in order to urge upon the Board the adoption of systematic training in mechanics, &c., with the object of adapting the scientific instruction, provided or contemplated in the Board schools, to the future employments of the children. A memorial to the same effect has been presented to the Board, and is now under their consideration, in favour of the elementary teaching of applied science and art in the schools, in such a manner as to lay the foundation of a connected system of technical education.

NEWS has been received of the death at Quito, Ecuador, in June last, of Dr. William Jameson, an eminent naturalist, who resided for many years in Quito as a professor of chemistry and botany in the University. His contributions, both in zoology and botany, to public institutions in America and Europe have been very extensive.

DR. DAVID MOORE reprints from the "Proceedings of the Royal Irish Academy" a complete Muscology of Ireland, under the title "Synopsis of all the Mosses known to inhabit Ireland up to the present time."

THE additions to the Zoological Society's Gardens during the past week include two Black-headed Parrots (*Caica melanocephala*) from Demerara, presented by Judge Lovesy; a Brown Bear (*Ursus arctos*), European, presented by Mr. M. B. Wilson; a Thicknee (*Oedicnemus crepitans*), British, presented by Mr. Patey; a Lesser Black-backed Gull (*Larus fuscus*), British, presented by Mr. C. W. Wood; a Hairy Armadillo (*Dasypus villosus*), from River Plate; a Burrowing Owl (*Pholeoptynx cunicularia*), from the same place, deposited; a Wattled Crane (*Grus carunculata*), from Africa, and two Bataleur Eagles (*Helotarsus ecaudatus*).

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICS AND PHYSICS

On Etherial Friction, by Prof. Balfour Stewart, LL.D., F.R.S.

Professor J. C. Maxwell has made a series of experiments on the friction of gases. In these experiments a horizontal disc was made to oscillate in an imperfect vacuum near a similar disc at rest, and it was found that the motion of the oscillating disc was carried away by the residual gas of the vacuum at a rate depending on the chemical character of the gas, and depending also upon its temperature, but nevertheless independent of its density.

While the temperature of the arrangement remained constant, it was found by Prof. Maxwell that this fluid friction was rather greater for atmospheric air than for carbonic acid, while for hydrogen it was, I think, about half as great as for air.

On the other hand, when the temperature was made to vary the result was found to be proportional to the absolute temperature.

These experiments do not show that there is no such thing as etherial friction, that is to say, friction from something which fills all space, and is independent of air; but we may argue from them that such an etherial friction must either have been nearly insensible in these experiments, or it must, as well as the friction from the gas, have varied with the absolute temperature, in which case the two frictions would not be separated from one another by the method of the experiment.

Prof. Tait and myself have made some experiments upon the heating of a disc by rapid rotation in vacuo. In these experiments we found a mere surface heating due to air which varied not only with the quality, but also with the quantity of the residual gas; and we also found a surface effect (more deeply seated however than the former), which appeared to be a residual effect, and which it is possible may be due to etherial friction. We made no experiments at varying temperatures, but we made use of various residual gases, and we found that the heating effect for carbonic acid was perhaps a trifle less than for air, while that for hydrogen appeared to be about four times less than that for air. Now comparing Prof. Maxwell's experiments with ours, we have in the former a stoppage of motion which is rather less for carbonic acid than for air, and about half as large for hydrogen as for air. In the latter again we have a heating effect rather less for carbonic acid than for air, and only about one-fourth as large for hydrogen as for air. Thus it appears that the stopping effect of hydrogen in Prof Maxwell's experiments is relatively greater in comparison with air than is its heating effects in our experiments, when compared with that of air. The effects of these various gases would bear to one another more nearly the same proportion in both experiments, if we might suppose that in Prof. Maxwell's experiments there was mixed up with gaseous friction a very sensible etherial friction; but in that case it would be necessary to suppose that the etherial friction was proportional to the absolute temperature.

During the meeting of the British Association at Edinburgh, I brought before this section reasons for imagining that if we have a body in visible motion in an enclosure of constant temperature, the visible motion of the body will gradually be changed into heat. The nature of the argument was such as to render it probable (although not absolutely certain) that in such a case the rapidity of conversion will be greater the higher the temperature of the enclosure.

I will now refer to some experiments by Prof. Tait which formed the subject of the last Rede Lecture. These experiments were suggested to Prof. Tait by an hypothesis derived from the theory of the dissipation of energy which led him to think that the resistance of a substance to the conduction of electricity, and also of heat, would be found proportional to the absolute temperature. Mattiessen and Von Bose in the case of electricity, and Principal Forbes in the case of heat, had already proved that as a matter of fact the law was not very different from that imagined by Prof. Tait. The result of these experiments has been to confirm the truth of this law.

The following considerations also connected with the dissipation of energy point to the same conclusion. Perhaps we may regard the etherial medium as that medium whose office it is to degrade all directed motion, and ultimately convert it into universally diffused heat, and in virtue of which all the visible differential motion of the universe will ultimately be destroyed by some process analogous to friction.

Now in order to imagine the way in which either may possibly act in bringing about this result, let us imagine some familiar instance of directed motion, as for instance a railway train in motion. The train, let us suppose, and the air in it, are both in rapid motion, while the air outside is at rest. Now as the train proceeds, suppose that a series of cannons loaded with blank cartridges are fired towards the train. A series of violent sounds will go in at the one window, and out at the other of each carriage. Each sound will push some air from the stratum of air at rest into the carriage on the one side, and it will push some air from the carriage into the stratum at rest on the other side. Now in this operation it would seem that part of the visible motion of the train must be taken from it. To make another comparison, it is as if a series of individuals were jumping into the train at the one side, and out of it at the other, the result being that each carries away so much of the motion of the train, and therefore renders it difficult for the engine to drive the train. Each individual comes to the ground with an immense forward impetus, and rubs along the ground till this is lost; in fact, he carries with him so much motion of the train, and converts it into heat by friction against the ground.

Now something similar to this must happen to a substance in visible motion in an enclosure of constant temperature. The rays of light and heat will play very much the same part as the waves of sound, or as the crowd of people in the above illustration, at least if we except those which fall perpendicularly upon the surface of the moving body. The moving body is like the train, and the rays of light and heat are similar to individuals entering the train from a stratum of ether at rest and leaving the train into a stratum of ether at rest again, each probably transmuting into heat a certain small portion of the visible motion of the train as it were by a species of friction. Of course the intensity of such an influence would depend upon the intensity of the rays of light and heat. Now it matters not what the particular kind of motion be which constitutes this train—we may assert that all directed motion will suffer from such a cause, and possibly according to the same laws. Visible motion, such as that of a rotating disc, or of a meteor, is of course one form of such motion; but a current of electricity or of heat may equally represent some form of directed motion. In fine, we may perhaps suppose that all forms of directed motion are resisted by this peculiar influence, which evidently depends upon what we may term the temperature of the ether, or at least upon the intensity of those vibrations which the ether transmits.

On a Periodicity of Cyclones and Rainfall in connection with the Sunspot Periodicity, by Charles Meldrum.

At the Brighton meeting (1872) it was stated that the cyclones of the Indian Ocean between the Equator and lat. 25° S., were much more frequent during the maxima than during the minima sunspot years. Since that time the subject has been more fully examined, and I now beg to present a catalogue of all the cyclones known to have occurred during the last twenty-six years The Tables given last year only contained cyclones of sufficient violence to dismast or otherwise disable vessels at sea, whereas the accompanying Catalogue gives all the cyclones of force 9 to 12, that is, "strong gale" to "hurricane."

The number of cyclones for each year from 1847 to 1873, is as follows:—

	Years.	No. of Hurricanes.	No. of Storms	No of Whole Gales	No. of Strong Gales.	Total No of Cyclones.	No. of Cyclones in Max. and Min. Periods.
	1847	5	0	0	0	5	
Max.	1848	6	2	0	0	8	26
	1849	3	2	3	2	10	
	1850	4	3	1	0	8	
	1851	4	2	1	0	7	
	1852	5	0	3	0	8	
	1853	1	1	5	1	8	
	1854	3	1	0	0	4	
Min.	1855	3	2	0	0	5	13
	1856	1	0	2	1	4	
	1857	2	1	1	0	4	
	1858	3	1	3	2	9	
Max.	1859	3	2	6	4	15	39
	1860	7	4	2	0	13	
	1861	5	2	2	2	11	
	1862	4	2	2	2	10	
	1863	5	2	1	1	9	
	1864	2	2	1	0	5	
	1865	2	2	3	0	7	
Min.	1866	1	4	2	1	8	21
	1867	0	4	2	0	6	
	1868	3	2	2	0	7	
	1869	3	1	3	2	9	
	1870	2	1	5	3	11	
Max.	1871	3	2	3	3	11	36
	1872	6	5	1	1	13	
	1873	4	5	3	0	12	

The observations for the years 1847-1850, are probably not so complete as those for the subsequent years during which the Meteorological Society of Mauritius made it a special duty to collect storm statistics. Still it is evident that not only the years 1860 and 1872, but also the year 1848, were remarkable both for the number and violence of cyclones, while the years 1856 and 1857 were quite the reverse. By taking the number of cyclones in each maximum and minimum sunspot year, and in each year on either side of them, so as to form maxima and minima periods of three years each, we obtain the results given in the last column of the above table, showing that during the maxima periods 1848-1850, and 1859-1861, the number of cyclones was 65, whereas in the minima periods 1855-1857, and 1866-1868, it was only 34, or little more than one half. In 1856, there was only one hurricane of small extent, and in 1867, no hurricane at all. Indeed it is doubtful whether several of the cyclones in those years classed under "storms," should not have been put down in the columns of "whole gales" and "strong gales."

As, during the last twenty-two years, information respecting the hurricanes of the Indian Ocean has been carefully and systematically collected and tabulated, I believe that the results now given are substantially correct, and it seems to me that they point unmistakeably to a close connection betwen sunspots, or solar cyclones, and terrestrial cyclones, or what might be called earth-spots, by an observer, on another planet.

Most of the severest cyclones have already been traced, and the others will also be traced. When this shall have been done, an attempt will be made to express numerically the amount of cyclonic area and cyclonic force for each year. The catalogue gives little more than the number of cyclones, but from what is already known, there is little doubt that their extent and force were also far greater in the maxima than minima years.

Being desirous of extending the investigation as far back as possible I have been examining the lists of former hurricanes, and it is interesting to find that the evidence from this source strongly corroborates the correctness of the conclusions deduced from the observations of the last twenty-six years. From a "chronological table" published in the "Mauritius Almanack" of 1869, we obtain the following list of Mauritius hurricanes.—

Years	No. of hurricanes	Years	No. of hurricanes
1731	1	1818	1
1754	1	1819	2
1760	1	1824	2
1766	1	1828	1
1771	1	1829	1
1772	1	1834	1
1773	1	1836	1
1786	1	1844	1
1806	1	1848	1
1807	2	1850	1
1815	1		
		Total	[illegible]

Probably the above list gives only the hurricanes that were remarkable from their destructive effects in the island; and much stress should not be laid on observations taken at a single locality. But it is rather suggestive that out of the twenty-four hurricanes mentioned, seventeen fall within, or very nearly within, maxima sun-spot periods, and only seven within minima periods Thus;—

Max Years	No. of hurricanes	Min. Years	No of hurricanes
1760	1	1731	1
1771 1772 1773	3	1754	1
1786	1	1766	1
1806 1807	3	1824	2
1815 1818 1819	4	1834	1
1828 1829	2	1844	1
1836	1	Total	7
1848 1850	2		
Total	17		

The same "chronological table" contains the following remarks:— 1760, Dec 1, "Meteorological Phenomena," 1815, Feb. 5, "Meteorological Phenomena."—I have not ascertained what these phenomena were; but it is not improbable that they were auroras. The aurora of the 4th Feb., 1872, was described in the newspapers as a *phénomène météorologique*.

Baron Grant, in his History of Mauritius, p. 194, regrets the destruction of the woods near Port Louis, because, he says, the town was thereby "exposed to the violence of the winds, as well as to the heat of the sun;" and in a foot-note it is remarked: "These inconveniences, however, are fully counterbalanced, if it be true that the *cessation of hurricanes since* 1789 has been caused by the great diminution of the woods." As the history was published in or after 1801; it would appear that during the twelve years 1789–1801 no hurricanes occurred. Now, since according to the Table of Sunspots the years 1788 and 1804 were maxima years, and the intervening minimum occurred in 1798, our theory would lead us to expect a comparative cessation of hurricanes during the period mentioned.

If time permitted, I would adduce similar evidence respecting the hurricanes of Bourbon and other parts of the world.

The hurricanes of the Indian Ocean are well known to be attended with torrential rains. So much is this the case that the popular belief at Mauritius is that cyclones are the cause of our rains. Heavy rains over extensive areas are certainly concomitant with cyclones in the Indian Ocean, and it was therefore resolved to examine whether there was a rainfall periodicity. As far as the Mauritius observations went, the matter was clear; but it was desirable to extend the investigation to other localities. The Queensland and South Australia observations, which were the only ones available at the time, gave a similar result, and as Adelaide is far beyond the limits of the region of cyclones, it was surmised that there was a rainfall periodicity generally. The Cape observations afterwards gave additional support to this view. The rainfall of England was next examined, and also found to bear out the hypothesis.

It would occupy much more time than I can at present spare to enter fully into this question of rainfall periodicity. With the help of the researches of Mr. Lockyer, Mr. Symons, and Dr. Jelinek of Vienna, I have now examined 93 tables of rainfall for various parts of the world, and I find that, scarcely without exception, more rain falls in the maxima than in the minima sunspot years. I beg to append a table showing the general results for the quarters of the globe. It will be seen that, so far as the observations go, Europe, Africa, America, and Australia give very favourable results. Asia is only represented by three stations, one of which is Jerusalem, where the excess of rain in one minimum period exceeds the excess in the maxima periods for two stations in India. France is the only European country (of which the rainfall has been examined) that gives an unfavourable return, but it must be remarked that we have as yet got only five stations there, which are all inland, and probably do not fairly represent the rainfall of the whole country.

By taking the longest possible series of observations for several stations, the periodicity comes out, and there is, I think, strong evidence that the rainfall for the whole globe is subject to an annual variation.

Having given the facts, as far as I have been enabled to do so, I abstain from offering any theoretical remarks. If cyclone and rainfall periodicities be fully established, a corresponding (direct) temperature periodicity should exist, and this presumable variation of solar heat may be the indirect cause of the periodicity of auroras and magnetic disturbances.

(The catalogue of cyclones was appended.)

On the Effect of Pressure and Temperature on the Widening of the Lines in the Spectra of Gases, by Arthur Schuster, Ph. D.

One of the questions in Spectrum Analysis yet open to discussion, is what influence pressure and temperature exert on the widening of the lines, which is sometimes observed when an electric current passes through certain gases. The subject of this communication is to point out a little ambiguity which has crept into the very statement of the question at issue, and to show the only way by which a decisive answer can be arrived at, and, in my opinion, has already been arrived at. I shall begin by assuming that the convection of electricity has no direct influence on the character of the spectrum; that is to say that under the same pressure, and at the same temperature, the gas will always show the same spectrum, whether the temperature has been produced by the passage of an electric current or by any other means. In the present state of science this is the only reasonable assumption that can be made, and it has been tacitly made, I think, by every one who has written on the subject.

Let us imagine a vessel filled with hydrogen, and let the temperature of the gas be brought up to incandescence. The heat communicated to the vessel is partly used to increase the translatory motion of the gas, and thereby to increase its pressure, and the other part of the heat has increased the periodical motion in the molecules of the gas, which is generally admitted to be the cause of its incandescence. If the temperature is such that the lines are widened we can account for this fact in two different ways. We may think that the forces which maintain the molecule in vibration, and which are such that at a lower temperature only perfectly isochronous vibrations can take place are somewhat altered, so that the bonds which keep the molecules together are loosened and now allow vibrations to take place, the period of which is somewhat altered and varying. We might secondly explain the widening of the lines by saying that they are caused by the disturbances caused by the frequent shocks of other molecules. If we increase the number or the force of these shocks by increasing either the number of molecules or their velocity we might well obtain disturbances large enough to change a little the period of vibration. These are the only two explanations that can be given, and if we say, therefore, that temperature is the cause of the widening of the lines we can only mean that part of temperature which has its equivalent in the vibrating energy within one molecule. If we say that pressure is the disturbing cause we include that part of heat which increases the pressure with increasing temperature. Let us now see whether we can obtain a clear answer to the question which has now been clearly put.

It is evident that no result can be arrived at by subjecting the same quantity of gas in the same vessel to different temperatures, for we cannot increase the vibrating energy of the molecules without increasing at the same time and in the same proportion (as Clausius has shown), their translatory velocity. By varying in the same ratio the two possible causes we shall never be able to say which is the right view to be taken.

There are two ways open to us to mend this difficulty. We might increase the temperature of the gas *under the same pressure.* If the perturbation caused by the shocks of other molecules cause the widening of the lines this widening ought not to take place as we have reduced the number of these shocks in the same ratio as we have increased their force. If on the contrary the disturbance in the period of vibration has its cause within the individual molecules it ought to remain.

We might, secondly, decide the question in subjecting the gas at the *same temperature* to different pressures. If perturbations are the cause the lines would be widened. Which of these two ways is most easily pursued in experimenting? Can we easily heat up a gas to incandescence under constant pressure? I think not. If an electric discharge takes place in a gas only comparatively few particles of the gas are heated up, and at a very small distance from the points through which the discharge takes place the gas is hardly heated up at all. But if the heat is not diffused through the whole mass of gas, the increase of pressure caused by this gas will also be merely confined to the luminous streak, and we can therefore obtain no answer to our question (as this has been attempted) by filling a tube with a certain quantity of gas, and altering the strength of the passing current or the mode of discharge.

We are, therefore, compelled to abandon this route and to turn our eyes to the second way which I have indicated; but here we meet another difficulty, and even one over which we cannot easily get. We cannot alter the pressure of a gas without altering its electric resistance, and, therefore, also the strength of the electric current and the heat developed. We can only decide the question by subjecting the gas at the *same* temperature to different pressure. Now have there ever been any such experiments made? I think there have, and even very decisive ones. Frankland and Lockyer have found that if we increase the pressure of hydrogen while an electric current is passing through it the lines begin to expand till the spectrum becomes continuous, and finally the resistance becomes so large that the electric current will not pass at all. On the other hand Gassiot and Plucker have observed that if we diminish the pressure of hydrogen its electric resistance force diminishes, attains a minimum, then increases again, and if we keep up exhausting the tube it becomes again so great that the current cannot pass. Plucker says that a tube exhausted to its utmost limits shows the lines of hydrogen and silica. He mentions at one place, I think, that the lines are very fine and distinct. If there would have been any widening he would have been sure to mention it. Now it is not too much to assume that the resistance of the gas at the moment when the discharge just ceases to take place is the same whether the increase of resistance is produced by too great a pressure or too great an exhaustion. At this moment, therefore, the current is the same and the same energy must be converted into heat by resistance. But in the case in which the current does not pass on account of the excessive diminution of pressure, only a much smaller quantity of gas has to be heated than in the other case. It must, therefore, be heated up to a much higher temperature, and yet the spectrum is not continuous and the lines are not even widened. We are, therefore, compelled to accept Frankland and Lockyer's original conclusion, that pressure and not heat is the cause of the widening of the lines.

The question is one of considerable importance. If temperature would widen the lines, the widening ought always to begin at the same temperature, and the hydrogen in the solar protuberances which show only narrow lines could not be at a higher temperature than the hydrogen in our vacuum tubes, the moment the lines begin to widen. If our conclusion, however, is correct the breadth of the lines will give us no indication whatever as to the temperature of the gas.

Dynamometers, by R. S. Ball, LL.D., F.R.S.

If we adopt that force which acting on one gramme for one second will impart the velocity of one centimetre per second as the unit, then one million of such units is a convenient magnitude for practical purposes. The large figures on the dynamometers represent these million units, for which it is hoped that ere long a suitable name will be adopted. The dynamometers are intended for educational purposes. They are exhibited to the Association with the desire of aiding the present movement in favour of an improved system of fundamental unity.

SECTION C.—GEOLOGY

Concluding Report on *the Maltese Fossil Elephants*, by Dr. A. Leith Adams, F.R.S.

For thirteen years Dr. Leith Adams has prosecuted his researches upon the fossil elephants of Malta, and he now presented the final report upon this subject. Three forms of fossil elephants occur here which are unknown elsewhere, all of small size. The largest is the *Elephas Mnaidriensis* (L. Adams), which attained a height of seven feet. In the crown sculpturing of the molars this species resembles *Elephas antiquus*; as regards the ridge-formula, its nearest ally is *Loxodon meridionalis*. *Elephas Melitensis* (Falconer and Burk) varied in size; its average height was about five feet; this too belonged to the Loxodon group. The smallest bones known to the author belonged to an elephant only three feet high, called *Elephas Falconeri*, by Busk. Although there appears to be some evidence for separating this from the other forms, yet the author stated that "there is no difficulty in arranging a graduating series of specimens from the smallest up to the largest bones ascribable to the *Elephas Melitensis*."

The elephants all occur in the same deposit, and with them there are remains of *Hippopotamus Pentlandi* and *H. minutus*. There is also a gigantic dormouse and a large extinct swan, besides some reptilian remains not yet fully worked out.

The report concludes as follows:—"It must be apparent that this (for the most part) unique fossil fauna restricted to a small mid-ocean island, presents several interesting contrasts with reference to the Mammalia in general, and elephants in particular, which frequented Europe during late geological epochs. For example, between Rome and Sicily we find remains of the *Elephas primigenius*, *Elephas antiquus*, and *Elephas meridionalis*. In the caves of Sicily, traces of the African elephant have been discovered, and also molars, barely distinguishable from those of the Asiatic species, and which, under the name of *Elephas Armeniacus*, are traceable eastward into Asia Minor, in the direction of the present habitat of the living species. It looks, indeed, as if the eastern basin of the Mediterranean had been at one time a common ground where all these extinct and living elephants met, and from whence, with other animals, they have disappeared or been repelled to distant regions."

Sub'Wealden Exploration.—1. *General Report*, by Henry Willett.

In this report Mr. Willett gave a summary of the results achieved up to the present time, the details having already been published in his quarterly reports.

The boring was commenced at the time of the last meeting of the British Association at Brighton, and its object is to explore the rocks underlying the Weald of Sussex. A bore of 6¼ inches diameter was at first adopted, but at the urgent recommendation of Mr. Prestwich, one of 9-inch diameter was employed. The bore has now reached a depth of 300 feet, and the engineer (Mr. Bosworth) has contracted to increase it to a depth of 418 feet at the cost of only 1*l.* per foot. Of the 300 feet of strata already passed through, about 70 were previously known, but the remaining 230 are new to science; 50 feet of this consists of valuable beds of gypsum.

Mr. Willett has designed a novel form of drill which possesses the following advantages:—(1) It cuts only the circumference; (2) it makes better progress; (3) the central core is left intact; (4) the tool not unfrequently extracts the core itself. The gypsum was extracted by this means, and it is believed that no such cores have been brought to the surface from similar depths in this country.

Sub-Wealden Exploration.—2. *Geological Report*, by W. Topley, F.G.S.

The author commenced by repeating the protest, often made already, that the Sub-Wealden Exploration was not a "search for coal." It is simply an endeavour to explore the rocks which underlie the Weald and especially to reach the Palæozoic rocks. Whatever these rocks may prove to be, if reached at all, the boring will have succeeded. The results of this boring cannot fail to have important bearings upon the question of the probable occurrence of coal measures beneath the South-East of England, but the discovery of coal is not the object in view.

An account was then given of the lowest beds exposed at the surface in Sussex, and of the reasons which have led many geologists to consider them as representatives of the Purbeck Beds. The thickness of Purbeck Beds previously known in Sussex was somewhat over 300 feet; probably about 230 additional feet of strata have been made known by the boring, in which there are some valuable beds of gypsum.

The boring commences about 250 down in the known Purbeck Beds; up to Sept. 1 it had reached a depth from the surface of 294 feet. It is not safe at present to speculate upon the geological age of the lowest beds reached in our boring, but additional evidence will probably soon be obtained.*

The author then pointed out that most of the bore holes which have been put down to the Palæozoic rock through newer strata have reached those older rocks at about 1,000 feet below the sea. There is a probability then that at or about this depth the palæozoic rocks will occur beneath the Weald. These places, however, are off, or to the north of, the westerly prolongation of the Axis of Artois, whilst the boring is to the south of that line; it is therefore possible that different conditions may prevail here.

Attention was then drawn to the fact, already pointed out by Mr. Godwin-Austen, that the dip of the carboniferous limestone in the Boulonnais is to the south, whilst in the Pays de Bray the same limestone has been found at a depth of 57 feet from the surface, underlying Kimmeridge clay. It is then probable that under the secondary rocks near to the south of Boulogne there is a basin of palæozoic rocks, in which the coal measures may be preserved; this basin might possibly be prolonged to the west below the Wealden district of the south-east of England.

In the course of the discussion which followed the reading of these reports, Sir John Hawkshaw stated that many people, himself included, took an interest in this question chiefly from the hope that coal might be found; but even if in this respect we were doomed to disappointment it would still be of great importance to show that, at that particular spot, no coal existed. Prof. Phillips thought that the object sought was neither coal, gypsum, nor salt; but that *something* exists below the Wealden is certain, and that something we are now searching for. A discussion then took place as to the best mode of conducting deep borings. Mr. R. Russell, C.E., spoke of the great value of the diamond boring process; but from remarks made by other speakers it appeared that, although the diamond is admirably adapted for boring small holes in hard rocks, it is not so well suited for conducting such an operation as that under discussion.

On the Arenig and Llandeilo Rocks of St. David's, by Henry Hicks, F.G.S.

The object of this paper was to follow out the succession of the rocks in the neighbourhood of St. David's, commenced in previous papers communicated to the British Association. The section was now completed to the top of the Llandeilo series. The Arenig and Llandeilo groups were each divided into an upper and a lower series, the author believing that in each case there was sufficient evidence to enable him to do so. The Lower Arenig series it was stated occur as black slates and flags about 1,000 ft. in thickness, and are characterised by many species of graptolites as well as by numerous trilobites entirely restricted to the series. The Upper Arenig series occur as fine-grained, soft black shales, not much cleaved, also about 1,000 ft. in thickness, resting conformably on the Lower Arenig series. Their graptolites are distinct from those found in the lower beds, as are also all the other fossils. The Lower Llandeilo series, the lowest rocks recognised by Sir R. I. Murchison in the typical Llandeilo district, occur at St. David's as black slates and hard-grey flaggy sandstones, and are about 1,500 ft. in thickness. The most characteristic fossils are *Didymograptus Murchisoni*, *Diplograptus pristis*, *Asaphus tyrannus*, *Calymene Cambrensis*, and *Illænus perovalis*. The Upper Llandeilo series occur as black slates and flags, several thousand feet in thickness, forming several folds of strata, and resting conformably on the Lower Llandeilo series. The typical fossils are *Ogygia Buchii*, *Barrandea Cordayi*, *Calymene duplicata*, *Cheirurus Sedgwickii*, *Trinucleus fimbriatus*, *Ampyx nudus*, and *Lingula Ramsayi*.

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* Since this Report was read, Prof Phillips has broken up and carefully examined parts of the cores brought up from the bottom of the boring, in them he has found *Lingula ovalis*, which occurs in the Kimmeridge Clay. (See p. 487.)

The author doubted whether any other spot hitherto examined in Britain could show so continuous a section of these rocks; still he believed that there was ample evidence to prove, from researches made in other parts of Wales and in Shropshire, that the succession here made out was, in most of its important details, capable of being applied to many other districts.

SECTION D.—BIOLOGY

DEPARTMENT OF ANTHROPOLOGY

On the Relation of Morality to Religion in the Early Stages of Civilisation; by Edward B. Tylor, F.R.S.

Investigations of the culture of the lower races of mankind show morality and religion subsisting under conditions differing remarkably from those of the higher barbaric and civilised nations. Among the rudest tribes a well-marked standard of morality exists, regulating the relations of family and tribal life. There also exists among these tribes some more or less definite religion, always consisting of some animistic doctrine of souls and other spiritual beings, and usually taking in some rudimentary form of worship. But, unlike the higher nations, the lowest races in no way unite their ethics and their theology. As examples, the Australians and Basutos of South Africa were adduced. The Australians believe spiritual beings to swarm throughout the universe; the Basutos are manes-worshippers, considering the spirits of deceased ancestors to influence all the events of human life, wherefore they sacrifice to the spirits of near relatives, that they may use their influence with the older and more powerful spirits higher in the line of ancestry. Yet these races and many others have not reached the theological stage at which man's good or evil moral actions are held to please or displease his divinities, and to be rewarded or punished accordingly. The object of the present paper is to trace the precise steps through which the important change was made which converted the earlier unethical systems of religion into ethical ones. This change appears to have been a gradual coalescence between the originally independent schemes of morality and religion.

In order to show the nature of such coalescence between religion and other branches of culture, not originally or not permanently connected with it, the author traced out on an ethnological line the relations between religion, and on the one hand the rite of marriage, on the other hand the profession of medicine.

First as to marriage:—The evidence of the lower races tends to show that at early stages of civilisation, marriage was a purely civil contract. Its earliest forms are shown among savage tribes in Brazil and elsewhere. The peaceable form appears well in the customs of the marriageable youth leaving a present of fruit, game, &c., at the door of the girl's parents; this is a clear symbolic promise that he will maintain her as a wife. Another plan common in Brazil is for the expectant bridegroom to serve for a time in the family of the bride, till he is considered to have earned her.

The custom of buying the wife comes in at a later period of civilisation, when property suited for trade exists. The hostile form of marriage, that by capture, has also existed among low tribes in Brazil up to modern times, the man simply carrying off by force a damsel of a distant tribe; the antiquity of this "Sabine marriage" in the general history of mankind being shown by its survival in countries such as Ireland and Wales, where within modern times the ceremony of capturing the bride in a mock fight was kept up.

Now in none of these primitive forms of marriage, as retained in savage cultures, did any religious rite or idea whatever enter. It is not till we reach the high savage and barbaric conditions that the coalescence between marriage and religion takes place; as where among the Mongols the priest presides at the marriage feast, consecrates the bridal tent with incense, and places the couple kneeling with their faces to the east to adore the sun, fire, and earth; or, as where among the Aztecs the priest ties together the garments of the bridegroom and bride in sign of union, and the wedded pair pass the time of the marriage festival in religious ceremonies and austerities. So complete in later stages of culture did this coalescence become, that many have come to consider a marriage hardly valid unless celebrated as a religious rite and by a priest.

Second, as to the relation of the profession of medicine to religion. In early animistic philosophy, one principal function of spiritual beings was to account for the phenomena of disease. As normal life was accounted for by the presence of a soul operating through the body, in which it located itself, so abnormal life, including the phenomena of disease, was accounted for in savage and barbaric culture as caused by some intruding spirit. Thus spiritual obsession and possession becomes the recognised theory of disease, and the professional exorciser is the doctor curing disease by religious acts intended to expel or propitiate the demon. Since the middle period of culture, however, this early coalescence has been gradually breaking away, till now in the most civilised nations the craft of healing has become the function of the scientific surgeon or physician, and the belief and ceremonies of the exorcist survive in form rather than in reality.

By these cases it is evident that coalescence between religion and other matters not necessarily connected with it may take place at different periods of culture, and also that this coalescence may terminate after many ages of adhesion. Having shown this, the author proceeded to ascertain exactly when and how in the history of civilisation the coalescence of morality and religion took place.

First, where manes-worship is the main principle of a religion, as among some North American tribes and the Kafirs of South Africa, the keeping up of family relations strongly affects the morality. It is, for instance, a practice among the ruder races to disinter the remains of the dead or to visit the burial place, in order to keep the deceased kinsman informed as to what takes place in his family, in which he is often held to take the liveliest interest. Thus it is evident that any moral act of an individual damaging to his family would be offensive to the ancestral manes, whose influence must therefore strengthen kindly relations among the living members of the tribe. Higher in the social scale this ethical influence of manes-worship takes more definite form, as when in China the divine ancestor of an emperor will reproach him for selfish neglect or cruelty to his nation, and even threaten to induce their own highest divine ancestor to punish him for misdeeds. Thus amongst the ancient Romans, the Lares were powerful deities enforcing the moral conduct of the family, and punishing household crime.

Second, the doctrine of the Future Life begins at the higher levels of savagery to affect morals. In its first stage the doctrine of metempsychosis is seen devoid of moral meaning, men being re-born as men or animals, but when the distinction appears in the higher savagery between migration into vile or noble animals, it is not long before this distinction takes the form of reward or punishment of the good and wicked by their high or low re-incarnation, an idea which is the basis of the Buddhist scheme of retributive moral transmigration through successive bodies. In its earlier stages this doctrine was of mere continuance, as where South-American tribes expected the spirits of the dead to pass to another region where they would live as on earth. Here the distinctions of earthly rank are carried on, the chief's soul remaining a chief, and the plebeian's soul a plebeian, but no sign of moral retribution appears. The first stage of this seems to be where warriors slain in battle are admitted to the paradise of chiefs in the land of the Great Spirit. This idea, which comes into view in several districts, leads to the fuller moral scheme in which goodness of any kind—valour, skill, &c.—are more and more held to determine the difference between the next life of the good man in happy hunting-grounds, or of the bad man in some dismal wilderness or subterranean Hades. In the higher nations this element becomes more and more distinctly marked, till the expectation of future reward and the fear of future punishment becomes one of the great motives of human life.

Third, when theology among the rudest tribes is mostly confined to consideration of ghosts, demons, and nature-spirits, the intercourse with these leads to little inculcation of moral action. It is when ideas of the great deities become predominant, when men's minds are turned to the beneficent action of the sun, or heaven, or earth, or to a Supreme Deity yet above these, that it is conceived that the order of nature includes moral order of human conduct. Then, as in the religion of ancient China, the universe and its Supreme Deity are regarded as furnishing the model and authority regulating man's actions towards his kindred and his subjects. Thus appears, not in the beginning, but in the middle of the development of religious ideas among mankind, the leading principle of a moral government of the world and its inhabitants.

In these three ways it appears from the evidence of ethnology that the vast transition was made from the earlier unethical to the later ethical systems of religion. Its course, so different from that imagined by the older speculative theologians, has to be ascertained from examination of the actual stages through which the religions of the world have passed. The very attempt to make this investigation on a basis of facts is, however, a novelty.

SCIENTIFIC SERIALS

THE *Monthly Microscopical Journal* commences with an article, illustrated with a plate, "On Organic Bodies in Fire Opal," by Mr. H. J. Slack, in which the author, from the appearances which he finds and describes, expresses an opinion, though not a decided one, that these minute bodies may be vegetable fossils, possibly algæ, though the evidence he adduces is extremely slight. Dr. G. W. Royston Pigott continues his researches on the high-power definition of organic particles, and re-affirms that the generally received description of the Podura scale is erroneous, on account of the employment of spherically over-corrected objectives.—Mr. Wenham criticises Dr. Pigott's paper in the preceding number of the Journal, remarking truly that the patience of microscopists must eventually become exhausted by the repetition of the same theme. He then shows that Dr. Pigott claims, without foundation, discoveries with regard to the improvement of object-glasses and the "colour test."—Dr. Maddox, On the apparent relation of nerve to connective-tissue corpuscles, &c., in the Frog-Tadpole's Tail, describes, in connection with the observations of Eberth, Kuhne, and Moseley, cases in which nerve-fibres seem to lose themselves in connective tissue corpuscles. His results are not very decided, and hardly tend to settle the question.—Mr. Edwin Smith describes a new sub-stage for the microscope, and certain appliances for illumination.—The paper read before the Royal Society, by Messrs. Pode and Lankester, is given in full. Their experiments are divided into eight series, in which infusions of hay and turnip, mixed or unmixed, with cheese finely divided or in lumps, are boiled and some of them sealed. When the cheese was finely comminuted, Bacteria did not appear; when in lumps, they were frequently found. In a boiled turnip infusion, placed in a retort of which the end was left open, there was no cloud developed after many weeks, which is quite contrary to the observations of Dr. Bastian (NATURE, Feb. 6, p. 275.)

THE *Geological Magazine* contains Prof. T. Sterry Hunt's article from the *Canadian Naturalist*, on the history of the names Cambrian and Silurian in Geology. The subject is divided into three parts: 1. The history of Silurian and Upper Cambrian in Great Britain from 1831 to 1854. 2. That of the still more ancient rocks in Scandinavia, Bohemia, and Great Britain up to the present time. 3. The history of the Lower Palæozoic rocks in North America.—Mr. E. Hardman describes and gives analyses of the Siliceous Nodular Brown Hæmatite (Göthite) in the Carboniferous Limestone Beds near Cookstown, Co. Tyrone. The ore contains as much as 52·2 per cent. of iron, on the average, and no sulphur.—Mr. J. C. Mansel-Pleydell has a brief memoir on the geology of Dorsetshire, which is an interesting summary of the most important points in the unbroken series from the Liassic to the Quaternary formations found in the county.—Mr. Joshua Wilson, in endeavouring to arrive at the time when the Gulf Stream reached the British Coast and so dispersed the then abundant glaciers, ingeniously shows that a raised beach, containing Arctic shells, mentioned by Geikie in his "Scenery of Scotland," must have been produced before that event, otherwise it would have been removed by the *offshore* under-current which always accompanies an onshore wind.—Dr. Winkler's description of *Pterodactylus micronyx* in the Teyler Museum, from the Lithographic Stone of Eichstatt, in Bavaria. The specimen is very small and complete. There are four phalanges in the long finger of the hand. In the foot there are two in hallux; three on the second and third; and two, with no metatarsus, on the fourth (Stummel).—In a letter to the editor, Mr. T. W. Danby, after comparing the new method of writing crystallographic formulæ proposed by Mr. Rutley, shows that it is not so advantageous as that of Dr. Whewell, modified by Prof. W. H. Miller; it is therefore doubtful whether its partial acceptance will not place a further obstacle in the student's path.

THE numbers of the *Journal of Botany* for August, September, and October, fully maintain the character of this magazine. In addition to the short notes and queries in each number, which often contain points of great interest to the systematic or physiological botanist, the following articles may be mentioned as of special value:—Dr. Alfred Nathorst, of the Geological Survey of Sweden, contributes a paper on the Distribution of Arctic plants during the Post-Glacial Epoch, which he considers to exhibit gradual changes of climate from the Forest-bed down to the Boulder-clay.—Prof. Church gives an analysis of the giant puff-ball, *Lycoperdon giganteum*, which he finds to contain, when dried, as much as 66·78 per cent. of albuminoids, and the ash 46·19 per cent. of phosphorus pentoxide.—Mr. J. G. Baker describes a very interesting new genus of ferns, *Diplora*, of the tribe *Aspleniæ*, from the Solomon Islands.—From the same botanist we have a valuable synopsis of the East Indian species of *Dracæna* and *Cordyline*.—Mr. J. Ball commences a description of some of the new species, sub-species, and varieties of plants collected by Dr. Hooker and himself in Morocco in 1871; the flora belongs essentially to the Mediterranean type, and the number of novelties is not comparatively large.—Mr. Carruthers gives his very valuable annual Review of the Contributions to Fossil Botany published in Britain in 1872, comprising 23 distinct papers or abstracts.—In these numbers we have also parts vi. and vii. of the Rev. E. O'Meara's Recent Researches in the Diatomaceæ.

THE second part of vol. xxix. of the *Transactions of the Linnean Society*, just published, is occupied by a continuation of Colonel Grant and Prof. Oliver's "Botany of the Speke and Grant Expedition." The number of new species described in this part is thirty-five; and it is illustrated by thirty-five full-sized 4to plates, the expense of which is munificently borne by Col. Grant.

Der Naturforscher, August.—The eruption of Vesuvius last year attracted much scientific observation, and we have in the present serial an abstract of a valuable paper by M. Heim on the nature and formation of lava, of which he distinguishes two kinds, "lump" lava and "cake" lava (*Schollen* and *Fladen*), differing, he found, not in chemical constitution, but merely in vapour-contents. In the physical division we may note M. Wiedemann's experiments in measuring the elliptical polarisation from reflection on bodies with surface colours, for a series of angles of incidence, and different parts of the spectrum. Meteorology is represented by M. Dufour's recent observations on reflection of solar heat from the Lake of Geneva; and an interesting paper entitled "Polar Lights and Earth Lights." There is a description of M. Zöllner's new mode of estimating the absolute temperature of the sun, which is based simply on a knowledge of the density relation between two different layers of the hydrogen atmosphere, the distance between them being known. The value his formula gives is 61350°. Among botanical subjects treated are, autumn colouring of leaves and formation of vegetable acids, summer dryness of our trees and shrubs, and passage of radiant heat through leaves. Some physiological experiments by M. Rosenthal, on the time-relations of reflex phenomena, are described; and there is a variety of other matter, much of which has already been noticed in these columns.

Annalen der Chemie und Pharmacie. Band. clxviii. Heft. 1, July 16.—The number opens with four papers by Prof. Ad. Claus, on azophenylen, on di-iodhydrin, on the action of ammonia on dichlohydrin, and on the preparation of dichlorhydrin. The first of these contains a long and exhaustive account of the body in question and of its compounds. The formula of azophenylen is $C_{12}H_8N_2$. By the action of ammonia on it a body having the formula $C_{12}H_{10}N_2$ is produced.—On diiodhydrin, by the same author. This body has the formula $C_3H_6I_2O$.—On the action of ammonia on dichlorhydrin, by the same. The result of the action is the production of chlorhydrinimid, a body of the formula $C_{12}H_{27}N_3Cl_3O_4$.—Preparation of dichlorhydrin, by the same. The method consists in acting on glycerin with chloride of sulphur.—Application of the periodic law to the cerium group, by D. Mendelejeff. —On the preparation of ethylen and its bromide, by E. Erlenmeyer and H. Bunte.—On the action of nascent hydrogen on the oil of bitter almonds, by Hugo Amann.—On the bromised benzol sulpho acids, by A. Woelz. The author has prepared dibrombenzol sulpho-acid, and gives an account of its salts and of its reaction with fused potassic hydrate.—An investigation of piperin and its products of decomposition, piperic acid and piperidin, by R. Fittig and I. Remsen.—On ethylen-protocatechuic acid by the same author, and T. Macalpine.—New compound of the Naphthalin group, by J. P. Battershall.—On the action of a mineral sulphur water on cast-iron, by Dr. E. Ptiwo uk. The author found an iron water-pipe, through which this water passed converted as regards its inner side into a mixture of sulphide of iron, hydrated oxide of iron and free sulphur. The centre stratum was also altered, containing only 79·2 per cent. of iron.—On sulph-hydantoin (glycolyl-sulpho-urea), by R. Maly.—Determination of boiling points at the normal barometric pressure, by Dr. H. Bunte.—Preparation of trimethyl-carbinol, by Linnemann's method, by A. Butlerow.

SOCIETIES AND ACADEMIES

LONDON

Royal Microscopical Society.—The opening meeting of the session was held at King's College, Oct. 1, C. Brooke, F.R.S., president, in the chair.—The secretary read a paper by Dr. Maddox descriptive of an organism found in a pond of fresh water in the New Forest, near Lyndhurst, which it was proposed to name *Pseudo-amaba violacea*. The general appearance of the organism was minutely described and figured, and the results of a series of continuous observations upon a growing slide under the microscope were detailed.—A paper by Mr. F. Kitton, of Norwich, describing some new species of Diatoms, was taken as read, and the attention of the meeting was called by the president to one of great beauty named by Mr. Kitto *Aulacodiscus superbus*.—Mr. F. H. Wenham made some interesting observations upon the microscopical appearance of glass which had been subjected to the action of the American sand-blast process, showing that the erosion of the surface was entirely due to the percussive force of the particles of sand, and that the results of this were demonstrated by the polariscope. A number of specimens were exhibited in the room.—Mr. C. Stewart, the hon. sec., exhibited under the microscope, and minutely described, a beautiful preparation of the spermatophores of the common squid; he also explained and illustrated the general structure of the generative organs of the male cuttle-fish.

PHILADELPHIA

Academy of Natural Sciences, April 3.—*Conchological Section.*—Dr. W. S. W. Ruschenberger, in the chair.—Dr. F. A. Hassler presented the following memorandum of experiments by W. M. Gabb and himself to ascertain the tenacity of life in *Littorina muricata*. The specimens, 140 in number, were collected by Mr. Gabb in St. Domingo, September 1870, and hung in a basket in his office. A few (five or six) were moistened after three months, then each month until May 1871, when all were alive. May, June, July, and August, 1871, 25 were moistened each month, and all found to be living except two in July and two in August. These were each month laid aside and not moistened again until September. At this time 40 of the original lot remained, all were moistened, and 29 found to be alive. In September, of the 100 which had been moistened during May, June, July, and August, 89 were alive. The 118 living ones were all placed together Feb. 18, 1872, the lot was again moistened and about 60 revived at once, and after several hours all but 24 were or had been crawling. These 24 were rejected. March 30, 1872: of the remaining 94, ten were moistened, nine were alive; these nine were placed aside with a few which had given evidence of life since the last experiment, Feb. 18. Sept. 18, 1872 all moistened and found living; they were also all alive in December. On Feb. 12, 1873, two found to be dead, and were separated from the others. March 26: All moistened, and though exposed for three days, only one began to crawl; this one was separated, also 27 others which were known to be dead, leaving 65 undetermined.

PARIS

Academy of Sciences, Sept. 29.—M. Bertrand in the chair.—The following papers were read:—Notes on the yellow elastic tissue, and remarks on its history in relation to a memoir by M. Bouilland, and some criticisms on it by M. Bouley, by M. E. Chevreul.—Researches on the elastic tissue of the elephant and the ox, by M. Chevreul.—New researches on the analysis and theory of the pulse, by M. Bouilland. The author continued his former papers on this subject, dealing with the abnormal pulse in this paper.—Remarks on M. Bouilland's late paper on the pulse, by M. Bouley.—Reply to M. Bouley by M. Bouilland. —Remarks on No. 21 of the "Mémorial de l'officier du Génie," by General Morin. The general drew attention to many interesting notes on the late sieges of Paris, contained in this number.—Note on magnetism, by M. J. M. Gaugain. This was a fourth instalment of the author's paper.—On the part played by gases in the coagulation of albumin, by MM. E. Mathieu and V. Urbain.—On a new method of treating cholera, and probably yellow fever, by means of sub-cutaneous injections of carbolic acid and carbolate of ammonia, by M. Déclat. The author recommended drinks containing carbolic acid in doses of from thirty to forty centigrammes per day, and from four to six injections of five grammes each of carbolic acid solution (2½ per cent.). These doses are to be largely increased in severe stages of the disease.—Comparison of the *Phylloxera vastatrix* of galls with those of roots, by M. Max. Cornu.—On the size and variation of the sun's diameter, by S. Respighi. The author, in his letter, discussed Secchi's late observations on the same subject.—On the action of the respiratory apparatus after an opening of the thoracic wall, by MM. G. Carlet and J. Straus.—On the classification of the fish of the family of *Triglidæ*, by M. H. E. Sauvage. —Researches on the action of heat on the carbuncular virus, by M. C. Davaine.—On a deposit of *Endogenites echinatus* in the Museum (fossil vegetable collection), by M. E. Robert.—On the influence of sulphates in the production of goitre in relation to an epidemic form of that disease in a barrack at St. Etienne, by M. Bergeret.

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ERRATA.—Vol. viii. p. 480 1st col. l. 23 from bottom, for "Kenson's" read "Henson's"; l. 14, for "Montague" read "Montagne."

THURSDAY, OCTOBER 16, 1873

D'ALBERTIS' EXCURSION INTO THE INTERIOR OF NEW GUINEA

IN a preceding number of NATURE (vol. viii., p. 305) some account has been given of the new Paradise-birds and other novelties recently discovered by Signor Luigi Maria D'Albertis in the interior of New Guinea. Signor D'Albertis, who is now in New South Wales, has lately published in the *Sydney Herald* an account of his month's excursion into the interior of that *terra incognita*, from which the following particulars are taken:—

D'Albertis started from Andai, a small village about ten miles from Havre Dorey, where, along with his companion Dr. Beccari, he had been resident with a Dutch missionary. By the aid of presents to the Corono, or headman of Andai, and promises of further payment on arriving at his destination, he succeeded in obtaining the services of six natives to carry his baggage and provisions to Atam, a populous village in Mount Arfak, where there was a Corono with whom he had already made acquaintance.

An early hour on September 4, 1872, was fixed for the traveller's departure, Dr. Beccari, the botanist, proposing to remain at Andai during the absence of his companion. After crossing a small creek in a canoe, the forest was entered. Besides six natives, D'Albertis was accompanied by a Malay interpreter and the wife of one of the natives, making eight persons in all. After a short walk over level ground a steep hill was reached, and crossed by a narrow pathway, fatiguing and difficult. The forest around was mountainous and gloomy, the silence being relieved only by the deep cooing of pigeons and the hoarse voice of a black Megapode (probably *Megapodius freycineti*). One of the latter served as dinner for the day. After arriving at the summit of the hill, an hour's walk across a level forest-country succeeded, whence a descent was made to a stream of water, deliciously clear and fresh. After this, hills were again ascended, gradually increasing in height, and the road became more and more difficult. Here the Lesser Bird of Paradise (*Paradisea papuana*) was met with, and the large Crowned Pigeons (*Goura coronata*) were very numerous. At 4 P.M. a height of 1,500 feet above the sea, which was seen to the east, not very far distant, had been attained, and after a short descent an extensive watercourse, at this time nearly dry, was reached. Here natives were first encountered; a tribe of men, women, and children, accompanied by dogs and pigs, emerged from behind the large stones of the water-course. The men were armed with bows and arrows and the *parang*, a large knife, narrowed near the handle and widened towards the extremity. Some of the men approached and were friendly and inquisitive, whilst others kept at a distance, and formed small picturesque groups about the rocks of the watercourse. The women were very timid, and also kept apart in groups along with the children. Upon inquiries through the interpreter, it appeared that these Papuans were returning from an expedition to the sea-side to procure salt. After taking leave of some of the natives, who were going in another direction, D'Albertis accompanied the others to their house, which was situated about 500 feet above the torrent. Here the forest was of the same gloomy character, but relieved by occasional clearings. At sunset a magnificent view over the harbour of Dorey and the island of Mansinam was obtained, and the birds raised their voices in chorus to salute the passing day. The house in which the night was passed contained four families. It was built on trunks of trees and entered by a long ladder. The stranger was well received, and presented with sugar canes, in return for which he gave his hosts tobacco.

The following day (Sept. 5), after some little difficulty, a start was made about 8 A.M., the chief of the house and some women accompanying the party. After descending to the watercourse passed on the previous day, the ascent of Mount Putat was recommenced, under the shade of large and umbrageous trees. At noon, the summit and village of Putat were reached, whence a fine view of the coast of Dorey and island of Mansinam were obtained. To the south-west rose some high mountains covered with dense vegetation. After an interval of repose, our traveller was anxious to depart, but was answered by the natives, that they had already arrived at Atam, and that they were not going any farther. It was not without much difficulty, and Signor D'Albertis showing them by his pocket barometer that they had not arrived at the requisite elevation of the place in question, that it was ultimately arranged that a fresh start should be made on the following morning.

The next day, accordingly, the party quitted the village of Putat, escorted by about 20 additional men, women, and children, and after descending to about 700 or 800 feet above the sea-level, commenced to re-ascend up the bed of another watercourse. About noon, a small stream of fresh water afforded an opportunity for refreshment, and at evening, after a further ascent, night-quarters were discovered in some uninhabited huts. On continuing the journey next day the party still ascended, until the summit of the mountain at an elevation of 3,600 feet was obtained. Here a halt was made in some huts similar to those used for the previous night, and Atam was visible to the west on the farther side of a deep valley. At this spot the Superb Bird of Paradise (*Lophorina atra*) was first seen, but examples were not obtained. To the south of the halting-place lofty mountains arose, considered to be 9,300 feet in height: to the east the view was impeded by thick forests of noble trees.

On continuing the journey a steep and difficult descent of about 900 ft. was made to the bed of a large river, containing more water than other streams previously passed, and said by the natives to flow into the Bay of Geelvink. After following up this river-bed for two or three miles, a rough track led away to Atam, the first houses of which were reached about 3 P.M. Here Signor D'Albertis determined to stop, being much exhausted by the journey, the latter part of which had been rendered fatiguing by the slipperiness of the paths caused by heavy rain. Next day messages were sent for the Corono or headman of Atam, who was resident higher up the mountain. D'Albertis was anxious to proceed farther himself, but his guides refused, stating that they had accomplished their agreement to bring him to Atam, and of this our traveller was satisfied, finding himself now at an elevation of 3,500 ft. above the sea-level.

Whilst waiting for the Corono, D'Albertis rambled about in the vicinity of his habitation, and found a fine young male of the Six-shafted Bird of Paradise (*Parotia sexpennis*), which had never been previously obtained except through native agency, and in imperfect condition. Other examples of both sexes were subsequently obtained, the adult male being always found alone in the thickest parts of the forest, whilst the female and young birds are usually met with at a lower elevation. Respecting this Paradise bird D'Albertis states that it is very noisy and feeds upon various kinds of fruit, more especially on a kind of fig which is very plentiful upon the mountain ranges. To clean its rich plumage, it scrapes a round place clear of grass and leaves, where the ground is dry, and rolls itself in the dust like a gallinaceous bird, at the same time elevating and depressing its plumage, and also raising and lowering the six remarkable plumes on its head, from which it derives its specific name. On the following day (Sept. 9), D'Albertis was fortunate enough to obtain adult specimens of the Six-shafted Paradise Bird just described, and also of the Superb Paradise Bird which he had observed on his way up the mountain. The latter is found on the same mountains, and feeds upon similar fruits; it flies about from branch to branch among the trees of the forest, uttering a cry of "ni-ed, ni-ed," and from this peculiar note is named by the natives, "Niedda," while the Six-shafted Paradise Bird is called "Coron-a." After skinning his Paradise Birds, Signor D'Albertis roasted their flesh for his dinner, and found it of an excellent flavour; his meal, however, was interrupted by the arrival of the Corono and his suite. Hearing a noise at the door, he turned and saw a number of men armed to the teeth. They entered, and defiled before him in silence, laid down their arms, and arranged themselves about the room. They were all adorned with necklaces and bracelets formed of shells, whilst quantities of flowers of bright and rich colours ornamented their hair, ears, and arms. After the men, followed women and children, until the house was full; last of all came the Corono himself, armed like the others, and lavishly adorned with flowers. He was followed by his son and daughter, both albinos, with hair of a clear white colour, eyes blue, and skin very white. Having entertained the Corono with a cup of cognac, Signor D'Albertis received a present of yams, maize, and oranges in return, and was informed that he was welcome to the country. Next day he received numerous visits from natives, and made large additions to his zoological collections. Finding the locality so rich, Signor D'Albertis determined to take an adjacent house, for which a rent of 4 metres of blue calico and four brass bracelets was demanded. On September 11 possession was taken of the new habitation, and the Italian flag hoisted on the summit. The house was divided by some pieces of bark into two rooms, one of which served as a bedroom and a workshop, whilst the other was the reception-room, and also served as a kitchen. When the news spread abroad that a white man had arrived, the visits of the Papuans became very frequent. Most of them brought yams, maize, or tobacco, for which Venetian beads were given in payment. On September 13 the guides who had brought Signor D'Albertis from Andai left him to return home, taking messages to his companion Beccari, to endeavour to send up a new stock of provisions, which were running very short.

Established in his new quarters, Signor D'Albertis set to work on his collections of birds and insects, and succeeded in amassing a large number of interesting specimens. But his provisions quickly began to run short, leaving him only a small quantity of rice to subsist on together with the flesh of the birds prepared for his collections. Salt was not to be had, and powder and shot also began to fail, and endeavours to get a fresh supply of ammunition and provisions up from Andai did not succeed. In consequence of a quarrel between the Arfaks and the people of Dorey, in which one of the natives was killed, his friendly intercourse began to be interrupted. Neither women nor children brought him insects, and soon afterwards they refused to sell him yams and maize. The Corono informed him, through the interpreter, that they were expecting an attack at Atam, and intended to leave the village. This D'Albertis did not believe until they commenced destroying the plantations, when his position becoming critical from want of provisions, he arranged with the Corono to return to Andai at the end of the month.

On September 29, accordingly, D'Albertis left Atam at sunrise, accompanied by about forty persons, his health having been much improved by his sojourn in the mountain air. Returning by a shorter route, he avoided Putat, and on arriving, on October 1, at Andai, found, to his regret, that Signor Beccari had gone on to the former village, so that if he had passed through it he could have obtained a fresh supply of provisions.

During his month's residence at Atam, Signor D'Albertis obtained 122 specimens of birds, and a large collection of insects, besides some mammals and other specimens. The only part of these that have yet reached Europe is the series of birds, of which an account was given in a previous number of NATURE (vol. viii. p. 305). The mammals obtained are stated to embrace several species of *Cuscus*, one of which is believed to be new, two or three species of Tree-kangaroo (*Dendrolagus*), a *Pteropus*, a Squirrel, and several species of Mice and Bats. The Insect collection is rich in *Cetoniæ* and *Melolonthæ*.

Soon after his month's excursion to the Arfak mountains, Signor D'Albertis was compelled, by continued attacks of fever, to leave New Guinea and proceed to Sydney, in the Italian frigate *Vettore Pisano*. Dr. Bennett informs me that his health is now re-established, and that he will probably return to Europe in a few months.

This interesting narrative serves to show us that the dangers and difficulties of penetrating into the interior of New Guinea, though considerable, have been somewhat over-rated. Though Signor D'Albertis has been the first to publish an account of his adventures in this country, I believe that the naturalist Rosenberg, in the employment of the Leyden Museum, had already made an expedition into nearly the same district.* Where these two pioneers have found their way, others will doubtless quickly follow, and we may thus hope to acquire, before long, a complete knowledge of one of the most wonderful floras and faunas of the world's surface.

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* Several of the new birds described by Dr. Schlegel, in his article on Rosenberg's collections (*Ned. Tijdschr.* iv p. 33), were also obtained by D'Albertis, but the only locality assigned to them is "l'intérieur de la grande presqu'île septentrionale de la Nouvelle-Guinée."

P. L. S.

THE MOTION OF PROJECTILES

A Mathematical Treatise on the Motion of Projectiles, founded chiefly on the results of Experiments made with the author's Chronograph. By Francis Bashforth, B.D., Professor of applied Mathematics to the advanced class of Royal Artillery Officers, Woolwich, and late Fellow of St. John's College, Cambridge. (London: Asher and Co., 1873.)

WE are told in the Preface to this work that "the consideration of the motion of a projectile naturally divides itself into three parts—first, its motion in the bore of the gun; second, its motion through the air; and third, its motion during its penetration into a solid substance." The author directs his attention chiefly to the second of these parts. Galileo was the first person who determined with anything like accuracy the motion of a solid body moving through space under the action of gravity. Treating the vertical and horizontal motions as perfectly independent (which of course is in accordance with Newton's laws of motion), he showed that a particle moved in a parabola. In this theoretical investigation gravity is supposed to be constant, and to act in parallel directions, while the effect of the resistance of the air is totally disregarded. The parabolic motion is approximately true for bodies whose velocities are small, but the greater the velocity of a projectile, the more does its path deviate from a parabola, and, in the present days of large guns and heavy charges, we can at once see the importance of solving with the greatest possible accuracy the problem of the motion of a projectile through the air, considering the air as a resisting medium materially affecting the motion of the shot. Newton solved the problem of the motion of a body through a medium whose resistance varies as the first power of the velocity, and John Bernoulli extended it to the case of resistance varying as any power of the velocity.

Experiments, however, show that the resistance cannot be regarded as varying as any single power of the velocity, though, within certain limits, the third power gives pretty accurate results.

Mr. Bashforth has applied himself to the task of throwing Bernoulli's solution into a practical shape, so that by means of copious tables, of which his book contains more than 100 pages, such problems as the following may be solved:—"The 16-pounder muzzle-loading gun fires an ogival-headed shot 16 lb. in weight, and 3·54 inches in diameter. If the angle of projection be 2°, and the initial velocity 1,358 feet per second, find the trajectory and time of flight." "A Rodman shot weighing 452 lb. is fired with an initial velocity of 1,400 feet per second, at a target 500 yards off, find the striking velocity."

Experiments were made by Robins and Rumford last century to ascertain the pressure of fired gunpowder and several persons have attacked the problem during the present century. General Mayevski attempted to solve the problem by firing shot, into the back of which a rod was screwed, the rod running through an aperture in the breech of the gun, and carrying a knife edge which cut two thin wires at a given distance, the interval of time between the two breakages being measured as accurately as possible. Captain Rodman made use of the following arrangement:—A gun was mounted in a gun-pendulum, and a revolving cylinder was placed with its axis parallel to that of the gun. When the gun was fired, a tracing point on the gun drew a curve on the revolving cylinder, the shape of which curve determined the whole motion of the gun's recoil. Mr. Bashforth suggested that much greater exactness would be procured if the tracing-point were connected with the projectile. He managed to do this to some extent by firing a shot through a number of equi-distant vertical screens, made of very thin metal wires. By an ingenious arrangement, the time of the shot breaking a wire in each screen was registered by means of an electric current on a revolving cylinder, special care being taken that all the registrations should be made under the same circumstances, so as to eliminate what we might call the personal error on the different registrations. This gave the times of transit of the shot over the successive intervals between the screens: from them, the velocities at the different screens can be calculated with great exactness, and also the resistance of the air on the shot. Mr. Bashforth has made great numbers of experiments with shots of different shapes and sizes, fired with different charges of powder, and from them has with great labour calculated the tables above referred to, which are sufficient for the solution of the problems we have given above as examples of what Mr. Bashforth has been able to accomplish.

The work is one which is too mathematical to do full justice to in our columns, but we have no hesitation in recommending it to such artillerists as are not unacquainted with mathematical analysis.

OUR BOOK SHELF

Half-hours with the Microscope. By E. Lankester, M.D. (Hardwicke.)

THIS excellent and well-known little work would scarcely require to have special attention now drawn to it, if it were not that the present edition contains an additional chapter, which adds much to its value as a text-book for amateurs. Until now the subject of polarised light has been omitted, and as the many beautiful and striking results which can be obtained by its employment are among the most important and attractive in the whole field of microscopy, any work on the subject in which it is omitted must be necessarily incomplete. The author, evidently feeling this, has added a "Half-hour with Polarised Light," which he has entrusted to the hand of Mr. F. Kitton, who, in the short space allowed him, has explained the theory of this rather intricate subject in a clear and popular manner, and has described some of the most striking of the phenomena exemplified by it, such as the appearance of the slides of iodo-sulphate of quinine, asparagine and sulphate of copper in gelatin, together with the methods for arriving at them. The addition of this chapter has made this work as complete as it is useful to the commencing microscopist.

Proceedings of the Belfast Natural History and Philosophical Society. (Belfast, 1873.)

WE welcome with pleasure the first number of the Belfast Society's Proceedings, which includes a number of papers read during the session 1871-2, some of which are already known to our readers. We need only name the principal papers. There is, first, the Presidential Address of 1871, "On Motive Power," delivered by Mr. J. J. Murphy, who has also a short paper on "The Bernina Lakes;" then comes Prof. James Thomson's admirable paper, "Speculations on the Continuity of the Fluid State of Matter, and on Transitions between the Gaseous, the Liquid, and the Solid States." This is followed by two short papers, one by Dr. J. D. Everett on "The Reduction of Observations of Wet and Dry Bulb Thermometers," and another on "Recent Changes of Coast-level at Ballyholme Bay, Co. Down," by Mr. Robert Young, C.E., who has also an excellent paper on "The Duty of Preserving National Monuments." Mr. John Anderson contributes a paper on "The Geological Formation of County Down," the Rev. Dr. Macloskie a long paper on "The Silicified Wood of Lough Neagh;" and there are also one or two papers of antiquarian and social interest. Appended is an interesting obituary notice by the secretary, Mr. Taylor, of the late Mr. Robert Patterson, F.R.S., one of the founders of the Society, and who, amid the cares attendant on the carrying on of a large commercial establishment, managed to find time to prosecute to very good purpose the study of natural history, and even to write admirable zoological text-books, and take an active part in the promotion of science and of social progress. The first number is edited by Mr. Murphy and Dr. H. Burden, and we hope the Society will produce material enough to bring out an equally good number every year.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Dr. Huizinga's Experiments

In a letter published in last week's NATURE, in which Dr. Bastian comments on a short paper read by me at Bradford on certain experiments of Dr. Huizinga, he challenges me to deal with his "main proposition," which is "that Bacteria are capable of arising in fluids independently of living reproductive or germinal particles."

In so far as relates to the subject of my communication, I have done so by showing that in the case of Huizinga's liquid Bacteria can be prevented from arising by heating the liquid to a temperature somewhat above boiling.

I hope that Dr. Bastian will allow me to decline to enter on the general question, and will believe that in doing so I am not insensible either to the difficulties of the subject, or to the value and importance of his own experimental investigations.

Oct. 13 J. BURDON SANDERSON

Experiments on the Development of Bacteria in Organic Infusions

THE correspondence in your journal on this subject (relating chiefly to the statements of Dr. Bastian) in which I took a part some six or seven months since—renders it necessary, in justice to myself, and I may add, in justice to the memory of my friend, Dr. Pode, whose loss has prevented me from continuing a series of experiments on the nutrition of Bacteria, commenced in the spring—to give some account in your columns of experiments carried out by us, which demonstrate that Dr. Bastian's assertions as to infusions of turnip and turnip-cheese are devoid of foundation in fact. The paper in which our results are given in detail was sent in to the Royal Society at the end of last March and printed in May (Proceedings, No. 145). Since you are not able to afford space for the reproduction of that paper in full, I must beg to refer your readers, for details, to that publication of the Royal Society. Here I may be allowed to sketch briefly the results and their bearing on Dr. Bastian's statements. The following passage from that gentleman's "Beginnings of Life" (vol. i. p. 429) induced us to make experiments similar to those mentioned in it, with the view of testing the correctness of his conclusion as to matter of fact:—

"On the other hand, the labours of very many experimenters have now placed it beyond all question of doubt or cavil that living *Bacteria*, *Torulæ*, and other low forms of life will make their appearance and multiply within hermetically-sealed flasks (containing organic infusions) which had been previously heated to 212° F., even for one or two hours. This result is now so easily and surely obtainable, as to make it come within the domain of natural law." And in a note is added, "in a very large number of trials I have never had a single failure when an infusion of turnip has been employed; and from what I have more recently seen of the effects produced by the addition of a very minute fragment of cheese to such an infusion (see Appendix C, pp. xxxiv.-xxxviii), I fully believe that in 999 cases out of 1,000, if not in every case, a positive result could be obtained."

The extract which follows is from a paper by Dr. Bastian in NATURE, vol. vii. p. 275, and is perhaps more remarkable than the preceding, because it is of later date and refers to a simple infusion of turnip.

"Taking such a fluid, therefore, in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask, with the assurance that it contains no living organisms. Having ascertained also, by our previous experiments with the boiled saline fluids, that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments. But when this is done, the previously clear turnip-infusion *invariably* becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria*." The italics are my own.

Dr. Pode and I give in our paper the details of 53 experiments, of which 11 were made with hay-infusion, the rest with turnip- or turnip-and-cheese infusion. We had some trouble at first in ascertaining some of the conditions under which Dr. Bastian experimented—since he does not state them in his book In the first place we ascertained through these columns the specific gravity of Dr. Bastian's turnip-infusion. We made a number of experiments after obtaining that information, which are recorded in our paper, and which invariably gave opposite results to those obtained by Dr. Bastian. At the beginning of this year we ascertained through Dr. Sanderson, in the columns of NATURE, that Dr. Bastian made use of two-ounce retorts; and that particles of cheese visible to the naked eye were present in his infusions at the time of boiling. Dr. Sanderson also stated that Dr. Bastian attached importance to the peeling of the turnips used. With this additional information we made further experiments, which tend to explain the failure of Dr. Bastian to keep his infusions free from Bacterian contamination.

There are four points which require attention in these experiments, and which were attended to in our series, but we must suppose were not attended to by Dr. Bastian.

In the first place the infusions were examined by the microscope at the time of sealing the tubes, as well as subsequently. What we sought to determine was whether a change had occurred in the infusion. Spherical and other particles besides dead Bacteria occur in freshly-boiled infusions, which might lead to erroneous conclusions when seen subsequently, if their previous existence had not been ascertained.

Secondly, we employed small tubes, five inches in length and of half-inch bore. It appeared to us not at all improbable, from the results of some experiments made by us with retorts such as Dr. Bastian used, that a boiling for five or ten minutes, before closure, of an ounce of liquid in a vessel of that peculiar shape, might sometimes give a development of Bacteria, owing to the protective effect of "spluttering," and the large mass to be guarded.

Thirdly, in the majority of cases—though we had no reason to doubt the efficiency (as proved by the results) of the boiling for five minutes in one of our small tubes, *cæteris paribus*—yet, to ensure thorough exposure of every part of the tube and liquid to the boiling temperature, we submerged many of our tubes in boiling water for a quarter or a half an hour after their closure. This method we finally adopted as the most certain to ensure the destruction of Bacterian contamination; it is essentially the same as that subsequently adopted by Dr. Burdon-Sanderson in the experiments described in a letter in NATURE, vol. viii. p. 141, the difference being that, "to make assurance doubly sure," Dr. Sanderson raises the temperature of the water in which his tubes are submerged above 100 C. by increasing the pressure under which ebullition is effected, beyond the normal atmospheric limit.

A fourth point to which we gave attention was the possible preservative effect of "lumps" on Bacteria or their germs. No one would have supposed that Dr. Bastian neglected the precaution of removing large particles of cheese from his experimental infusion. We always strained our cheese emulsion very carefully, or else filtered it. Prof. Cohn found that an infusion made by boiling a pea in water developed Bacteria when the pea was left in it; but if the pea were removed, and the infusion subsequently reboiled, no Bacteria were developed. We found that lumps of cheese could really act as protective hiding-places for Bacterian contamination. In a retort—similar in every respect to Dr. Bastian's—this result was first obtained, though other retorts similarly treated were barren. Accordingly we prepared twelve tubes exactly alike, with the exception that in six the cheese was added as an emulsion, in the other six in the form of lumps. The tubes were closed, and submerged in boiling water for five minutes. Of the "emulsion"-tubes, one burst in the boiling, the other five were barren; of the "lumpy"-tubes, four developed Bacteria in quantity, two remained barren.

In the experiments recorded in NATURE, vol. viii. p. 141, by Dr. Sanderson, it is shown that even when "lumps" are avoided, and the infusion heated by submergence in boiling water, this may not prevent the development of Bacteria when a large bulk of material is employed. But boiling for such a length of time as one hour, or heating to 101° C., always gave him a barren infusion. Dr. Sanderson does not believe that there is a definite relation between the precise temperature to which the infusion is exposed and the destruction of Bacterian contamination, but that the *longer* heating, or the heating to a *higher* degree, will increase the *chance* that Bacteria or their germs are destroyed. Further, Dr. Sanderson's results agree with those of Dr. Pode and myself as to simple turnip infusion. With this infusion I understand that he has not found the same length or amount of heating necessary as with the turnip infusion to which a fragment of cheese has been added.

And now, I wish very briefly to point out where Dr. Bastian's statements are affected by these results. It is necessary that this should be clearly and simply put, because I find that many persons are under the impression that the investigation of the grounds of Dr. Bastian's statements has shown that there was some solid foundation for them. This is, however, in my opinion, not the case. It is *not* "beyond all question of doubt or cavil that living Bacteria, Torulæ, and other low forms of life will make their appearance and multiply within hermetically-sealed flasks (containing organic infusions) which had been previously heated to 212° F. even for one or two hours." On the contrary, no organic nor inorganic infusion has been contrived by Dr. Bastian nor by anyone else which will develop Bacteria, still less Torulæ, after exposure for one hour (or even less) to 212° F. This is the conclusion given by the impartial examination of the subject, indicated in the experiments above quoted.

Moreover, the statement in the second quotation from Dr. Bastian is abundantly contradicted by the experience of Dr. Sanderson, Dr. Pode, and myself. Such a turnip-infusion, placed as directed by Dr. Bastian, does *not* invariably become turbid in one or two days, owing to the presence of myriads of Bacteria. We have often kept such infusions free from Bacteria for many days, and I preserved one in a retort with its beak inclined downwards for more than six months, clear as crystal, but amply capable of sustaining the life of Bacteria, as was proved by its accidental contamination a week ago.

It is my opinion that the only *positive* addition to knowledge which this inquiry about the development of Bacteria in infusions has led to is, that when you have cheese-emulsion, or similar material present in an infusion, you must be a little more careful about heating it than when you have not, if you wish to destroy by the agency of heat the life of Bacteria or their germs contained in the infusion. How it is that cheese-emulsion helps the Bacterian contamination to escape destruction we do not know. Possibly in the same way as the larger lumps do. But that matter remains for inquiry when more is ascertained as to the natural history of the Bacteria. I think we may now feel fully satisfied that "archebiosis" or "abiogenesis" is not in any way rendered more probable than it was before by Dr. Bastian's experiments with organic infusions. Prof. Smith and Mr. Archer, of Dublin—eminent authorities in the study of the lower algæ—have criticised in detail and suggested explanations of some of the statements in the third part of "The Beginnings of Life," viz., statements relating to the transformation of various species of organisms into others. They show (the reader may consult Prof. Smith's paper in the October number of the *Quarterly Journal of Microscopical Science*, 1873) that the asserted "facts" of transmutations are *not* facts. It is abundantly demonstrated that the fundamental observations recorded by Dr. Bastian are erroneous, and that he has been mistaken.

Exeter College, Oxford, Sept. 26 E. RAY LANKESTER

Variations of Organs

MY father finds that in his letter, published in your number for September 25, he did not give with sufficient clearness his hypothetical explanation of how useless organs might diminish, and ultimately disappear. I therefore now send you, with his approval, the following further explanation of his meaning.

If one were to draw a vertical line on a wall, and were to measure the heights of several thousand men of the same race against this line, recording the height of each by driving in a pin, the pins would be densely clustered about a certain height, and the density of their distribution would diminish above and below. Quetelet experimentally verified that the density of the pins at any distance above the centre of the cluster was equal to that at a like distance below; he also found that the law of diminution of density on receding from the cluster was given by a certain mathematical expression, to which, however, I need here make no further reference. A similar law obtains, with reference to the circumference of the chest; and one may assume, with some confidence, that under normal conditions, the variation of any organ in the same species may be symmetrically grouped about a centre of greatest density, as above explained.

In what follows I shall, for the sake of brevity, speak of the horns of cattle, but it will be understood that my father considers a like argument as applicable to the variations of any organs of any species in size, weight, colour, capacity for performing a function, &c.

Supposing then that a race of cattle becomes exposed to unfavourable conditions, my father's hypothesis is that, whilst the larger proportion of the cattle have their horns developed in the same degree as though they had enjoyed favourable conditions, the remainder have their horns somewhat stunted. Now, if we had made a record of the length of horn in the same species under favourable conditions, we should, as in the case of the heights of men, have a central cluster, with a symmetrical distribution of the pins above and below the cluster. According to the hypothesis, the effect of the poor conditions may be represented by the removal of a certain proportion of the pins, taken at hazard, to places lower down, whilst the rest remain *in statu quo*. By this process the central cluster will be slightly displaced downwards, since its upper edge will be made slightly less dense, whilst its lower edge will become denser; and further, the density of distribution will diminish more rapidly above than below the new central cluster.

Now, if horns are useful organs, the cattle with shorter horns will be partially weeded out by natural selection, and will leave fewer offspring; and after many generations of the new conditions, the symmetry of distribution of the pins will be restored by the weeding out of some of those below the cluster, the central cluster itself remaining undisturbed.

If, on the other hand, horns are useless organs, the cattle with stunted horns have as good a chance of leaving offspring (who will inherit their peculiarity) as their long-horned brothers. Thus, after many generations under the poor conditions, with continual intercrossing of all the members, the symmetry of distribution will be again restored, but it will have come about through the general removal of *all* the pins downwards, and this will of course have shifted the central cluster.

If, then, the poor conditions produce a *continuous* tendency to a stunting of the nature above described, there will be two operations going on side by side—the one ever destroying the symmetry of distribution, and the other ever restoring it through the shifting of the cluster downwards.

Thus, supposing the hypothesis to be supported by facts (and my father intends to put this to the test of experiment next summer), there is a tendency for useless organs to diminish and finally disappear, besides those arising from disuse and the economy of nutrition.

Down, Beckenham, Oct. 4 GEORGE H. DARWIN

Oxford Physical Science Fellowships

I WRITE this letter that in future candidates for Oxford Fellowships in Physical Science may be aware that outsiders are ineligible.

In June last the Warden of Merton College informed me that the election to a Physics Fellowship would *not* be limited to graduates of Oxford, and would altogether depend on the result of the examination held at Merton on Oct. 7. Candidates had no other information than was afforded by the notice in your columns.

Although I found that great difficulties were thrown in the way of outsiders in their not being allowed an opportunity of examining the physical apparatus which was to be used in the examination, and with which Oxford men are well acquainted, I read for the examination, not having the slightest doubt about my eligibility after receiving the Warden's letter.

It is now nearly four months since I received the letter, and although the authorities must have been very well aware of the grave error which had been fallen into, I was not informed that a blunder had been committed until the morning of the examination. It is now found by the Warden, on consulting the registrar of the university, that only Oxford graduates can compete for these Fellowships.

Oxford, Oct. 8 JOHN PERRY

Simple Method of Studying Wave Motion

IT is difficult for a student to obtain a clear idea of the movement of the particles of a liquid or gas propagating a wave. To assist him models have been devised, but as a rule they are expensive and complicated. The following plan, based on the principle of the stroboscope, I have found extremely convenient. Take a piece of cardboard about 3 ft. long and 18 in. broad. Put this into the tin drum of a "zoetrope," pressing the card well against the interior of the drum, so that it stands up forming a cardboard cylinder. With a lead pencil mark where the inside fold of card comes, and you have the right size of the cardboard to form the cylinder. Divide now the length of the cardboard into 12 equal strips. On each strip paint dots representing the wave you want to study, taking care that each wave is represented $\frac{1}{12}$ behind its predecessor. Lastly, cut out 12 slits, about 8 in. by ¼ in., between each representation of the wave; restore the card to the drum of the zoetrope, and then turning the cylinder and observing through the slits, the wave is seen, as the cylinder revolves, to advance with its characteristic motion, while by stopping out all but one of the particles represented the exact character of its oscillation, whether circular, elliptical, or linear, is clearly seen.

Midland Institute, Birmingham C. J. WOODWARD

The Glacial Period

JUST one line in reply to Frank E. Nipher. I have read Tyndall's Lectures on Heat, and that some time before I addressed you on the subject of the Glacial Period. Plainly, it is against common sense to suppose that an increased outpour of solar energy would diminish the mean temperature of the air at the earth's surface to such an extent that glaciers at or near sea level should be found in Egypt, or even, I believe, in Central Hindustan, as was the case in the Glacial Period. All I can say is, that if the sun then were a hotter sun than the sun of our own age, he must have blundered at his work.

And now may I crave space for just another line on another subject? Could not our learned societies be induced to publish their mathematical contributions separately? I was compelled to take the whole of the first part of the Royal Society's Transactions of 1867, for the sake of Clerk-Maxwell's paper on Molecules. For this I paid a guinea—willingly, indeed; but had the paper been published alone, I should probably have had it for a much lower figure. Then there are Professor Stokes' and Sir W. Thomson's magnificent papers scattered up and down among the Transactions of the Royal and Cambridge Philosophical Societies; if these were gathered together and published apart, it would be a precious boon to persons like myself who are interested in physical mathematics. And pupils of the École Invariantive would, no doubt, be as much gratified by an easier access to the numerous contributions of Professor Cayley to the Theory of Determinants. Is it impossible, or even inconvenient, to afford such facilities to students and amateurs?

Hampstead, N.W., Oct 3 J. H. ROHRS

THE OWENS COLLEGE, MANCHESTER

IT is now upwards of twenty-two years since this college was opened—for the foundation of which in Manchester, John Owens, a merchant of that city, left 100,000*l.*—in a house that belonged to Mr. Cobden, in Quay Street, which was purchased and presented to the trustees by Mr. John Faulkner, the first chairman. The number of students during the first session was 64, which went on increasing year by year, until last session the day students numbered 327, and the evening students 513. A few years ago it was felt that the original house had become much too small, and that a new building ought to be erected adequate to the increased needs of the College. Accordingly, in 1866, a circular was prepared, setting forth the disadvantages of the then institution, and propounding an extension scheme which should include the additions to the College of a school of Engineering, a Medical School, and the Natural History Museum, which the Council of the Natural History Society recommended should be deposited in Owens College, "if it should appear that the scheme for enlargement was likely to be successfully carried out within a reasonable period." The trustees therefore appealed for funds which would enable them to lay the foundations of an institution which would virtually be the University of South Lancashire, and of the neighbouring parts of Cheshire and Yorkshire.

In 1867 an Extension Committee was formed for raising a fund, which "it was desirable should not be less than 100,000*l.*, and, if possible, 150,000*l.*," to carry into effect the proposed system of extension. 24,000*l.* was almost immediately subscribed. The engineers of Manchester and neighbourhood subscribed 10,000*l.* to found and endow a chair of Engineering Science, and for the provision of an apparatus and a library. An application to the Government for a grant, though never absolutely refused, was first temporarily shelved on the familiar plea that the subject was "under consideration," and on a change of Government it was ultimately forgotten. The success of the College is therefore a monument of voluntary effort. After the present site had been purchased, the sum of 12,000*l.* was subscribed towards the new Medical School. Principal Greenwood and Prof. Roscoe subsequently visited Germany, and obtained valuable information as to the schools of science in that country; and to the plans which the Professor of Chemistry especially brought home, the new College owes the perfect arrangements in its scientific lecture-rooms, and the handsomely fitted-up laboratories for chemical and physiological science; laboratories, we believe, which are not equalled by any in the kingdom, if, indeed, in Europe.

The foundation-stone of the buildings just completed was laid by the Duke of Devonshire in September 1870, and the same nobleman occupied the chair at the opening of the new building on the 7th instant.

As is well known, the "religious difficulty" has been entirely obviated, in the case of Owens College, by the will of the founder, which requires "that the students, professors, teachers, and other officers and persons connected with the said institution, shall not be required to make any declaration as to, or submit to any test whatsoever of, their religious opinions," and that "nothing shall be introduced into the matter or mode of education or instruction, in reference to any religious or theological subject, which shall be reasonably offensive to the conscience of any student, or of his relations, guardians, or friends under whose immediate care he shall be." It is no doubt partly owing to this that the Manchester

College can boast a body of teachers not surpassed in any respect by any university in the kingdom.

The college is rich in scholarships, fellowships, and prizes founded by Manchester men, and by means of these, and its admirable system of day and evening classes, affords facilities to all classes of obtaining a literary and scientific education, both general and professional, of the highest and most advanced kind. In most respects, indeed, it may be regarded as a model institution for the higher education.

Of the many excellent addresses given on the occasion, we have only space for a few extracts from those of Principal Greenwood and Sir Benjamin Brodie. We shall take another opportunity of referring to the address of Prof. Roscoe at the opening of the Chemical School.

Principal Greenwood said:—"I am addressing the assembled students of the new year; and it is because I feel that you are even more concerned in the inquiry than are my colleagues and myself that I ask you to consider some of the relations which subsist between culture and practical life, not as matters of speculative interest, but as bearing closely on the aims and the temper with which you should take up the studies of this place. This inquiry might take either of two directions, according as we consider the debt due from society to the student, or the debt due from the student to society. It is not possible altogether to separate these inquiries; but it is of the latter that I propose to speak more especially this morning, not only because in addressing students, as in addressing other men, it is more wholesome to speak of their obligations than of their claims, but also because in this place and on this day, there is little need to urge the duties of society to the student.

". . . . For us the normal principles of education, in their whole range and mutual bearings, are of infinitely greater weight than the special questions which fix attention at the moment; but our thoughts are in danger of being drawn away from these deeper truths, and our springs of action of being in that degree weakened or perverted. An illustration of this position may be seen in the history of the vigorous and successful efforts which, within a few years, have been made in favour of the claims of the natural sciences to a leading place in the curriculum of study. Men of genius and of public spirit have insisted on them with unanswerable arguments; and I shall not be suspected by those who happen to be cognizant of the part which Owens College has taken in this matter with any inclination to call these claims in question. I wish, however, to point out that arguments are urged in their support of very unequal force; and that while the able leaders of the crusade dwell most on the stronger among them, their followers are wont to recur too frequently to the weaker, and by raising them into undue prominence to run the risk of inducing—not the general public only, but what is in reality a more serious thing, of inducing you and us to hold pernicious views as to what education is and what are the appropriate motives for it. Of these arguments the weightiest is, I will venture to affirm, the most seldom heard. I mean the assertion that the natural and experimental sciences have a characteristic discipline for the mind. This position may in this place be taken for granted; and it constitutes of itself an argument at once unanswerable and sufficient. But when we hear the further argument that physical sciences should hold a prominent place in education because their promotion contributes to the material advancement of the country, or because to possess a knowledge of them will give the learner a greater command of money and what money brings, we are then offered motives of a very different order. As collateral motives they have great value, I admit, for exaggeration on one side must not be met by exaggeration on the other, but a value subordinate to that of the former consideration. It is, of course, true that all good education, through whatever medium, tends to produce good and well-furnished citizens, and therefore promotes the general, including the material, well-being of a country; and all good sound education tends to make men manly and self-reliant, and so trains their faculties as to enable them, among other things, to win with ease their share of material good. It is true too, that in choosing the subjects of study regard should be paid, in due degree, to the destination of the future life. But when the secondary and by nature inferior aim takes the first rank, the fatal consequence follows that the higher good is not even sought in the second place. The greater may include the less, but not the less the greater.

"Another instance of harm to the business of education from the passing controversies of the hour lies in the sudden development of the system of competitive examinations. To discuss the merits of this system in itself is altogether beside my object. I wish to refer only to its oblique influence on teachers and pupils, or rather (for each of these schemes would admit of long discussion) of its influence on the temper of the student. Can anything be more deplorable—if it were not deplorable it would be grotesque—than the change which this system threatens to bring about in the mutual relations of study and examination? By the old theory the business of education was—first, the discipline of the intellect by means of the arts and sciences as instruments; and, secondly, the storing of the mind with methodical knowledge gained in the process. Examinations were but the handmaid of the teaching, designed to test and measure the results of study, and so to correct its methods; and if honours and more substantial rewards were conferred on those who took the foremost places, this was partly to stimulate the flagging, and enable the more promising wits to prolong their season of study, and partly that public or academical offices might be filled by the fittest occupants. . . . Now, however, men are almost tempted to think that the public service exists for the sake of the sharp-witted or the industrious, and not they for it. 'La carrière ouverte aux talens,' once the stirring motto of an indignant people, has become a circumlocutory and more decorous version of the frank maxim of ancient Pistol—

> 'The world's mine oyster,
> Which I with sword will open.'

". . . We are now prepared to answer the question which I wish to propose: What were the conditions under which for many centuries the theory of the higher education was this—that to all who sought it a common culture was provided in the first instance, and that from this, as from a trunk, three or four types of special or professional training branched off. And again, to what influences is it due that in the present day many are found to advocate the abandonment of this principle in favour of a method by which, the common groundwork being reduced to the narrowest limits, the special training is made to begin with the first years of college life or even at a still earlier date? One answer to this question (but not the only answer) I have already indicated, viz., that according to the older theory 'a complete and generous education,' in the words of Milton, was 'that which fits a man to perform justly, skilfully, and magnanimously all the offices, both private and public, of peace and war;' whilst the other theory holds that the aims and interests of the individual are to be chiefly kept in view. Now it is no doubt true that, as is sometimes urged, these rival theories may be so handled as in appearance to lead to the same result; but in appearance only. It is true that the highest development of any community not only allows, but requires, that the best possible should be made of each of its members; and it is not less true, if less obvious, that an enlightened selfishness might discover that in the long run it can serve itself best by serving others. But 'enlightened selfishness' has been a great many centuries in learning, in this region as in others, how 'to save by losing itself.' If then, as of course no one will seriously question, the older theory be sound, it will not be safe to leave the course of study wholly to the caprice of individuals. The experience or instinct of academic bodies has aimed at giving effect to this principle by requiring that students aspiring to academic honours, and to those diplomas which are the passports to the so-called learned professions, should pursue a course of studies uniform, or nearly uniform, up to a certain defined point. In our day, when university training is no longer sought only by those who seek to enter the great professions, and when, too, the narrow list of these liberal professions is from time to time receiving one and another sister, it is a principal academic problem to show that the old principles ought still to be insisted on in their essence, and yet that modifications must be made in detail, in order that they may be applied with safety. It is when we have to meet the reluctance—the natural reluctance—of students of this new order to submit to the yoke of academic traditions that we are brought face to face with the rival claims of society and the individual. I say the rival claims; but, in fact, they are not rivals, but complementary each of the other. I mean not only that each has its rights, which must not be ignored, but that each is necessary to the perfect development of the other; that unless due play is given to the special gifts and aspirations of its members, society cannot reach its highest form; and that, unless individual men remember that they exist for the sake of society at least as much as for themselves, they too will fall short of their proper standard, and will leave some of their noblest faculties wholly unused. . . .

"... The subject matter of the studies selected is, in fact, of less importance than the discipline imparted. This only is essential—that there should be such a selection made as will (1) draw out and strengthen the several powers of the mind, and (2) afford a basis so broad that on it may afterwards be erected the structure of professional study when the career is chosen. These conditions are met if the common groundwork includes (1) letters, to cultivate the taste and judgment, to give a good style in speech or writing, and to place the student on the threshold of the best literature of home or foreign growth; (2) mathematics, to discipline the reasoning faculty, to give the habit of concentrated thought, and to place in the student's hand a weapon indispensable for the thorough mastery of the physical branches; and (3) some branch of physical study, to develop the powers of observation and inductive reasoning, and to impart the method of this study, so that, should the student afterwards take up a profession based on some physical science, as medicine, engineering, or manufacturing art, he may be able with facility and pleasure to provide himself with the technical knowledge proper to his calling. It might be added, too, in defence of the claims of this third prime element of culture, that it is singularly fitted to counteract the faults alleged, not without reason, to be inherent in the other two. But I must not proceed further on this field. I have placed the justification of the adoption of a common groundwork of culture for all students on two direct and, as I believe, sufficient pleas. But, over and above these direct uses, there are at least two others, which I can only indicate;—(1) Grace and vigour are lent to social intercourse when men feel that they can trust to the possession by all of a certain general culture—that a common atmosphere, so to say, is shared by all, and that subtle criticisms, delicate shades of thought, apt illustrations, will not fall flat on the ears of one half of those who listen. Those who are familiar with the social history of the first half of this century will agree with me that this element of social life was far more generally present with cultivated men than it is now. (2) And, again, from the want of this common elementary culture, men are without that sympathy with the pursuits of others which tends so powerfully to soften the bitterness of controversy, and even to make fruitful discussion possible."

Sir Benjamin Brodie's speech is specially remarkable as giving the impression which a long connection with one of the older Universities has made upon a distinguished man, whose sympathies would naturally be with them. We have only space for the following extract:—

"The foundation of such universities as Oxford and Cambridge is lost in almost prehistoric time; and if I say that this is the foundation of an university, I say so from what appears to me to be a very good reason, for I believe that Owens College boasts all the essential constituents of an university; and I have no doubt that before long it will go forth into the world equipped as an university in every respect. I know that some persons take a very different view of universities from that which I do. Some consider that the university is merely a sort of better grammar-school, which differs from the ordinary grammar-school by having more and older students, and a somewhat wider range of study. I don't believe that any enlargement of the curriculum of a grammar-school will ever elevate it into an university. Some persons consider that an university is a body which grants degrees. I confess that the granting of degrees is an important and responsible function; yet of all the functions of an university it appears to me the very least. To claim that function as the distinguishing characteristic of an university is equivalent to saying that the man who puts a stamp on a sovereign is the maker of the coin. An university should not only be a teaching body, but from every point of view it should represent, further, and promote the interest of knowledge, not only by teaching, but by preserving knowledge through the foundation of libraries, museums, and collections, and by the labours of its professors in furthering and increasing knowledge. I fully believe that that was the idea which was present to those who were concerned in the foundation of Owens College—namely, that it is to be not merely a grammar-school, but a great organ for furthering knowledge. . . .

"We have heard many allusions to-day to the financial condition of Owens College, and I do not doubt that there are many here who, in considering this question, look perhaps, I will not say, with some degree of envy, but with a peculiar interest, upon the statistics relating to the pecuniary affairs of Oxford and Cambridge. These great universities differ from Owens College as *plus* differs from *minus*. These institutions—Oxford and Cambridge—are in that happy position that their Chancellors of the Exchequer have no taxes to raise, and have only to consider the appropriate mode of distributing their budgets. But yet, really, any envy which might be raised from this consideration might be entirely removed by a more close intimacy and acquaintance with the subject, for though undoubtedly money is a good thing, and money well used is better than money itself, yet in many cases these endowments of universities have been so connected and linked with inappropriate objects, that they have really done more harm than good. The question of University Reform has been debated for about 30 years without the end being gained as to how to distribute these revenues properly. These revenues are also inappropriate and sometimes mischievous, doing great evil to the old universities in consequence of their application to objects which, though appropriate 300 or 400 years ago, are now useless, or worse. Unhappily these objects do not coincide with those which deserve attention at the present day, and the consequence is that a great amount of time and a large amount of energy and talent have been wasted in removing evils which have grown up in connection with these endowments. I hope that this kind of work will never be necessary in connection with the University of Owens, and I think you may congratulate yourselves that you have to begin *de novo*, and that you have only to adapt your arrangements to the purposes you desire to be served. That is a much simpler thing to do than to adapt antique arrangements to purposes which they were not intended to serve. Another point in which there are some difficulties that the old universities have had to contend with comes before us in regard to those unfortunate arrangements which for so long a period connected them with a very unpopular party in the State. It is only recently that, by a prolonged series of efforts on the part of individuals, we gained the abolition of what were commonly termed university tests. I do not think I shall offend anybody by referring to that subject, because these tests may now be regarded with a curious, though somewhat painful, interest, like the thumbscrews and other instruments of torture of which we read in history; but in reality they constituted a very atrocious evil. We must all regret that they ever existed, not only on account of the labour and difficulty which they involved to those who took an active part in sweeping them out of the way, but also on account of the far worse amount of evil, in the shape of immorality and dishonesty, which they created. However, you at Owens College are happily free from all these evils. I earnestly hope, and fully believe indeed, that Owens College will ever preserve that union between freedom and science—freedom not only to think, but freedom of research and freedom of speech—which is absolutely necessary for the progress of science. I hope that nobody will ever meddle with your professors, and try to put an extinguisher upon their researches."

ON THE APPENDIX VERMIFORMIS AND THE EVOLUTION HYPOTHESIS

TOWARDS the close of the last meeting of the British Association at Bradford, a paper was read before the Biological Section, which calls for special comment, because of the unfavourable impression which it and much of the subsequent discussion must have left on non-scientific as well as scientific hearers, as well as on account of its scientific inaccuracy.

The paper referred to was by Prof. Struthers, who endeavoured to show that the appendix vermiformis of the human intestine may be considered as a good example of a useless and detrimental addition to the vital economy, and, such being the case, it must be apparent to all that evidence of design is not exhibited in the construction of the living body, and consequently the doctrine of special creation must be supplanted by that of evolution.

The general weakness of this argument must be apparent to many at first sight, but there are some points with reference to it which call for special remark. In the first place it may be shown, if it is assumed as true that the appendix vermiformis of the human cæcum is, as stated, useless and positively injurious, that the fact militates quite as much against the doctrine of the evolutionist, as it does against those of the teleological school. For if it is positively disadvantageous, on the Darwinian hypothesis, for the individuals of a species to possess an appendix vermiformis, it is a necessary deduction, that in a very short period either the species should die out, or be replaced by another in which the detrimental organ is absent. The human race and the anthropoid apes, however, seem quite able to hold their own, without the loss of their supplementary cæcum, consequently either the appendix vermiformis causes insignificant danger, or the evolution hypothesis is incorrect.

It is not difficult to demonstrate that it is the former of these two alternatives which fails, that the danger caused by the existence of the appendix vermiformis is much exaggerated, and that its uselessness is only an expression of ignorance on the part of those who make statements to that effect.

Some people have died from perforation of the appendix vermiformis, or the peritonitis which it induces; the number of recorded cases are comparatively few, and those which follow disease of the rudiment of the vitelline duct in the small intestine are much rarer, though Prof. Struthers seems to have seen several. This shows no doubt that there are disadvantages attending the possession of a complicated cæcum, or an unobliterated vitelline duct; but it shows too much for the argument on which we are considering its bearing, for there are many other organs, avowedly indispensable to the economy, which have caused death by their simple mechanical presence. A case was lately recorded before the Zoological Society, in which a kangaroo met its death from strangulation of a loop of the small intestine by the coiling round it of the uncomplicated, but long cæcum; are we from this to infer that the cæcum is so dangerous an addition to the organism, that it would be better if it did not exist? Such can hardly be correct. Again, in man, if the testes do not descend into the scrotum, impotency is the result, can we therefore infer that the abdominal rings would be better away, because some die of strangulated inguinal hernia? It would be as logical to wish to dispense with the head, because some have been killed by wounds on the scalp.

Again, it can scarcely be said in the present state of our physiological knowledge, that the appendix vermiformis is useless, and a remnant of a fœtal structure. Leaving sexual structures out of the question, as subject to different laws, it is quite contrary to evolutionary doctrine that useless rudiments of embryonic organs should be retained in after life; for the individuals encumbered with the unnecessary remains of a former different *régime* could scarcely be expected to succeed in the struggle for existence against less trammelled and consequently more advantageously circumstanced members of its own or any other class. If also the appendix vermiformis were a rudiment of a fœtal organ, it is not easy to see how it is that it is retained in man and the anthropoid apes, whilst it is not found in the lower monkeys, the Ungulata, and other animals which possess a cæcum (the wombat excepted), and are therefore similarly situated in early life. On the other hand, the voice of the evolution hypothesis clearly states that, with the exception above mentioned, the appendix vermiformis must bring positive advantage to its possessors; for it is only developed in the most elaborated and the highest of those creatures which are the result of its unceasing and most beautiful routine, and there is no reason why its action should cease at this point where it is most called for, and where the struggle is most acute.

There is another aspect in which we think the whole subject should be regarded. Prof. Struthers' remarks all have an anti-teleological tendency; in other words, they are little more than hits at a theory which has had its day, and which, if left alone, will die a quiet and natural death. Why make this death a painful one, and attempt to develop an unpleasant party feeling between those who, from the capacities of their brains and their previous education, have been led to adopt the one or the other? Such discussions, as acknowledged by most who are competent to form a correct opinion, do very little, or nothing, towards the advancement of science, and tend to lower it very much in the estimation of the non-scientific world. The true theory will ultimately predominate, without doubt, but it will do so from its own intrinsic value, and not from attacks on the deepest feelings of its opponents, especially when they are based on a false interpretation of its deductions. To quote the words of one of the greatest of our physiologists, it can only bring ignominy on the body of scientific workers if they are supposed to countenance an argument such as that of Prof. Struthers, which assumes that because one or two individuals have died from the impactation of cherry-stones in the appendix vermiformis, therefore there is no God!

*THE COMMON FROG**

II.

BEFORE passing on to an enumeration of the subordinate groups of the sub-kingdom Vertebrata, we may first revert to our subject, the Frog, and make further acquaintance with it.

The common frog of this country belongs to the genus *Rana*, and it is the species *Temporaria*, therefore its scientific name is *Rana temporaria.* It is common in Ireland, as well as in England and Scotland, and is indeed the most widely distributed species of the frog-order, being found throughout the temperate regions of both the Old and New Worlds. It is found over nearly the whole of Europe; in Africa north of the Sahara, and in Egypt; in Northern Asia, including Japan and Chusan, and it is also spread over North America. It is not found in the northern half of Scandinavia, nor in Iceland.

Except in winter, the common frog is generally in England so familiar an object, that any description of it might seem superfluous. The purpose in view, however, renders it needful at least to recall certain external structural characters both of the adult and the immature condition.

The head and body of the frog together forms an elongated oval mass, somewhat pointed at each end, of which mass the head constitutes rather more than one-third. This mass is more or less flattened both above and below, except at the commencement of the hinder third of the back, where there is a more or less marked prominence, which indicates the junction of the haunch bones with the spine. In front of this the only marked projections are those of the eyeballs.

The short arms project outward on each side just behind the head, and each ends in a small hand with four fingers, the second of which is the shortest, and the third the longest. When the arm is turned backwards this third finger barely attains (if it can do so at all) the hinder end of the body.

The hind limbs proceed from quite the hinder end of the body, there being no vestige of a tail. The thigh is very muscular, and the leg has a good "calf." The foot is exceedingly long, and what is very remarkable, is so jointed that the toes can be sharply bent upwards on its thick and undivided part. The latter thus seems to form a third segment of the hind limb following the thigh and the leg, the limb having four segments instead of three as in ourselves, and in almost all beasts, birds, and reptiles.

The foot ends in five toes connected by a web. Of these the fourth is the longest, the first the shortest. On the inner margin of the sole of the foot, at the root of the first toe, is a small, hard prominence, called a "tarsal tubercle." When the hind limb is turned forward, the knee reaches nearly to the armpit; the ankle-joint is about on a line with the end of the snout, and both parts of the foot beyond it. These two parts of the foot together are much longer than the whole fore limb, and exceed two-thirds of the length of the whole mass of the head and body.

When the animal is viewed in profile, the point of the muzzle is seen to be very little in advance of the opening of the mouth. The latter is straight. It is also very wide, extending back even beyond the hinder margin of the eye. Just above the hinder angle of the gape, and behind the eye, is a rounded surface of smooth, tightly-stretched skin. This is called the "tympanum," and directly covers in the drum of the ear.

When the mouth is opened, if the finger be drawn along the inner margin of the upper jaw, a series of minute teeth may be detected. Towards the front of the palate are a pair of small holes (which are the inner openings of the nostrils), and between these are two juxtaposed little groups of other minute teeth. There are no teeth whatever in the lower jaw. At the hinder end of each side of the palate is another small hole. These latter two apertures are each the opening of a canal leading from the mouth to the cavity of the ear within the drum. The tongue is seen to be large, flat, and fleshy. It is tied down to the jaw in front, but free for more than its hinder half, with the processes developed from its free hinder margin.

The skin of the frog is naked and smooth, without a trace of scales, or other appendages. Its colour on the upper surface is more or less yellowish, or reddish brown, with irregular black, brown, or grey patches. Similar patches form transverse bands upon the legs. Beneath the colour is pale yellowish, often with a few spots, paler than those of the back. There is constantly a brownish black subtriangular patch placed behind the eye, and extending over the tympanum down towards the shoulder. The frog breathes partly by swallowing air (aided by a mechanism to be described hereafter), partly by the direct respiratory action of the skin. It feeds exclusively upon living animals, such as insects and slugs, which it catches by suddenly throwing forwards beyond the mouth, the free hinder half of the tongue (furnished with an adhesive secretion), and then retracting it with its prey in a most rapid manner.

In winter the frog passes into that torpid state known as *hibernation*, as is the case with our bats, hedgehogs, and some other beasts. Its season of torpidity is generally passed by it buried in mud and at the bottom of water, and great numbers of individuals may be dug up in winter all clustered together.

In spring the frogs again congregate for the purpose of oviposition in the month of March, at which period their well-known croaking makes itself heard, and though in itself unmelodious, possesses a certain charm through its association with the vernal outburst of nature.

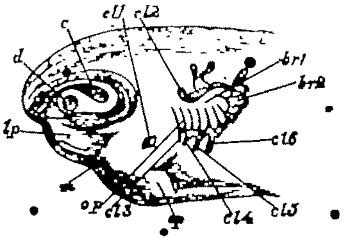

FIG. 3.—View of left side of head of Embryo Tadpole (after Parker). *br*¹ and *br*², first and second external branchiæ; *cl*¹—*cl*⁶, the six visceral clefts; *cp*, the left "holder"; *d*, the olfactory organ; *e*, the eye; *lp*, the left lip; *m*, the aperture of the mouth; *op*, the hinder margin of the rudimentary operculum.

When first laid, the frog's eggs are little round dark bodies enclosed in no solid shell or case, but in a small glutinous envelope. The latter quickly swells in the water so much that the "spawn" comes to have the appearance of a great mass of jelly through which dark specks (the yolks of the egg) are scattered. Each egg, when microscopically examined, may be seen to undergo a process of yolk sub-division and cleavage till a mulberry-like mass is formed. Upon this soon appears the "primitive groove," which forms a canal and develops beneath it a "chorda dorsalis" according to the process which has been already stated to be common to the whole of the Vertebrata.

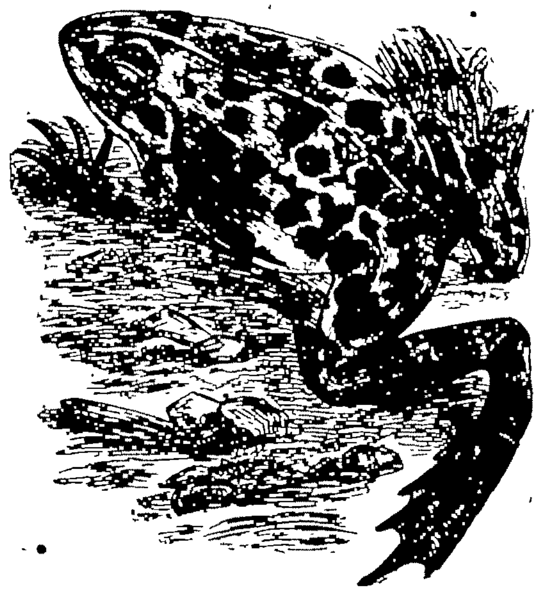

FIG. 4.—The Edible Frog (*Rana esculenta*).

Gradually the embryo assumes the form of a young tadpole, and is provided with a pair of little "holders" (or organs for adhesion) just behind the mouth, with six openings on each side of the neck (Fig. 3, cl^1–cl^6), and with a pair of rudimentary external gills (Fig. 3, br^1 and br^2). These openings are termed "visceral clefts," which lead from the exterior into the throat, as already described. The solid pillars (or intervals) between the clefts, *i.e.*, the "visceral arches," become furnished with gills,[*] or *branchiæ*, and are therefore called "branchial arches." The eggs are hatched towards the end of April, and the tadpole emerges in the stage represented at Fig. 2, 1. It has a relatively large head, a rounded body, and a long tail, by lateral undulations of which the little creature swims about. From behind the head, on each side, jut forth external branchiæ as a small plume-like structure, but no limbs are visible.

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* Continued from p. 471.

As the tadpole grows the external plumose gills at first greatly enlarge (Fig. 2, 2 and 2*a*), but afterwards become gradually absorbed, and are succeeded by short gill-filaments, which are developed along each of the branchial arches. These latter filaments do not appear externally, and indeed a membrane, termed the operculum (Fig. 3, *op*), is developed from the front of each series of branchial apertures, and which, extending backwards by degrees, ultimately covers over and conceals them.

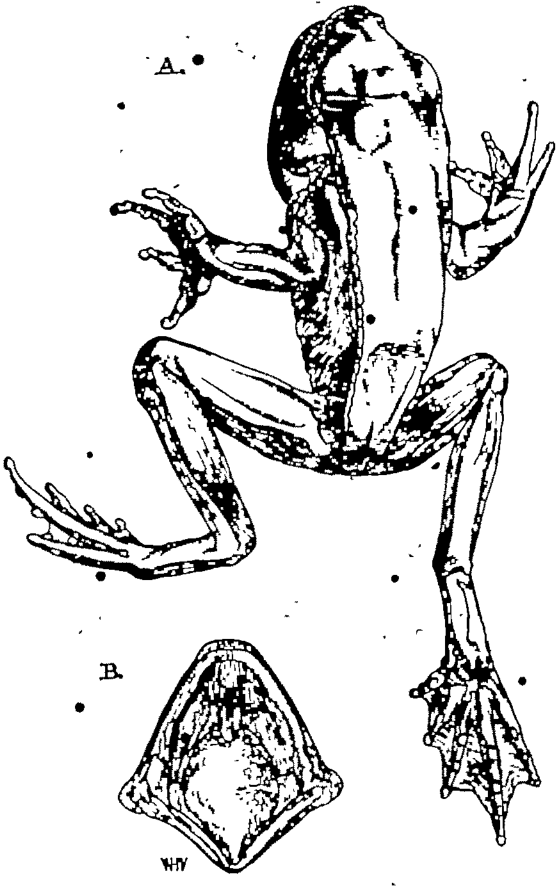

Fig. 5.—A, *Pachybatrachus robustus*, nat. size, B, interior of the mouth of ditto.

Little by little the limbs bud forth and grow, the hind ones being the first visible because the fore limbs are for a time concealed by the opercular membrane. As the legs grow, the tail becomes absorbed (Fig. 2, 7), not falling off, as some suppose. The gills also disappear, and the branchial apertures close, that on the right side first becoming obsolete by adherence of the operculum to the skin of the body.

As the gills diminish and cease to serve the purposes of respiration, lungs at the same time become developed in an inverse ratio, and the tadpoles absolutely require to come to the surface to breathe.

The process, from the hatching to the acquisition of the miniature form of the adult, may be accelerated or retarded by elevation or depression of the temperature. The frog more than doubles its bulk in its first summer.[*] The young tadpole has at first a very small mouth placed beneath the head and not at its anterior termination; it is also for a time provided with a sort of beak formed of two little horny jaws.

The food of the tadpole, quite unlike that of the adult, consists largely (especially in its earlier stages) of vegetable substances.

Having now made acquaintance with the Frog considered absolutely, or by itself, and also clearly seen that it is a member of the Vertebrate Sub-kingdom, we may enumerate the principal primary sub-divisions (Classes) of that Sub-kingdom, and enumerate such of the next smaller groups (Orders) as more or less nearly concern the subject of this work—the Frog.

The Vertebrata are divided into five great Classes:—(I), *Mammalia* (Man and Beasts); (II.), *Aves* (Birds); (III.), *Reptilia* (Reptiles, *i.e.* Crocodiles, Lizards, Serpents, and Tortoises); (IV.), *Batrachia* (Amphibians, *i.e.* Frogs, Toads, Efts, &c.); and (V.), *Pisces* (Fishes).

Of these five classes Birds and Reptiles are classed together in a larger group called *Sauropsida*, because they present so many structural resemblances. Similarly Amphibians and Fishes are grouped together, and to their united mass the common term *Ichthyopsida* is applied.

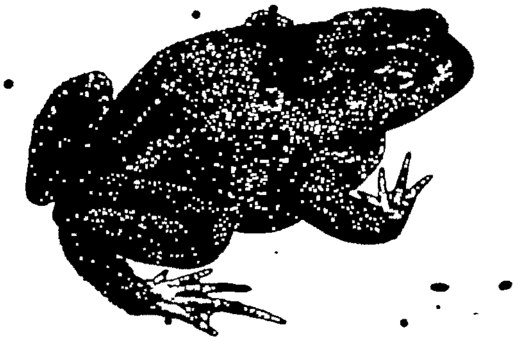

Fig. 6.—The Common Toad (*Bufo vulgaris*)

The orders into which the two classes, Mammalia and Aves (beasts and birds), are divided, may here be neglected, as we shall have little to say respecting them in the following pages. There are, however, about twelve orders of beasts, and probably some fourteen of birds.

The class of Fishes has been subdivided into five Orders.

1. Elasmobranchii (the sharks and rays, or highly organised cartilaginous fishes).
2. Ganoidei, an important order, containing many extinct forms, and a few very varied existing ones, such as the mud-fish (Lepidosiren), ceratodus, and the sturgeon.
3. Teleostei, the ordinary or bony fishes, such as the carp, sole, perch, &c., and containing a remarkable group called Siluroids, as also the curious little sea-horse—Hippocampus.
4. Marsipobranchii (the lamprey and myxine, or lowly organised cartilaginous fishes).
5. Pharyngobranchii (the amphioxus, or lancelet).

Reptiles are arranged in nine different orders, five of which are now entirely extinct. They are of living forms:—

1. Crocodilia (crocodiles).
2. Sauria (lizards, the Amphisbenæ, the little Flying-dragon, &c.)
3. Ophidia (serpents).
4. Chelonia (tortoises and turtles).

Of extinct kinds there are:—

5. Ichthyosauria; 6, Plesiosauria; 7, Dicynodontia; 8, Pterosauria; and 9, Dinosauria.

The remaining class, Batrachia, will require more lengthy consideration, both as a whole and as regards the four orders which compose it, and which are called respectively, 1, Anoura; 2, Urodela; 3, Ophiomorpha; and 4, Labyrinthodonta.

It will require such consideration, because it is the class to which the Frog itself belongs.

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[*] Gills (or branchiæ) are delicate processes of skin richly supplied with minute blood-vessels, wherein the blood becomes exposed to the purifying action of the air dissolved in the water.

[*] Parker, Phil. Trans., 1871, p. 172.

The Frog belongs to the Batrachian order *Anoura*, to the family *Ranidæ*, and to the genus *Rana*.

The order Anoura, to which all frogs and toads belong, is a remarkably homogeneous one, consisting as it does of a multitude of species, all differing from each other by comparatively trifling characters.

Altogether there are about 600 species of frogs and toads, arranged in about 130 different genera.

St. George Mivart

(*To be continued.*)

JEAN CHACORNAC*

THIS eminent French astronomer died on the 6th of last September, having been born at Lyon, June 21, 1823. Chacornac is chiefly known for his discoveries among the planetoids whose orbits are contained between those of Mars and Jupiter. In his earlier years he devoted himself to commerce, but having, in 1851, made the acquaintance of M. Valz, Director of the Marseilles Observatory, Chacornac became an enthusiastic student of astronomy, devoting himself to research in connection with the solar spots and to the assiduous exploration of the heavens. On his discovery of a new comet on May 15, 1852, he made up his mind to abandon commerce and devote himself entirely to astronomy.

In 1852, M. Valz, following the example of Mr. Hind, had drawn some charts of the region of the heavens in which the small planets were likely to be met with, and on Chacornac taking the above decision, Valz entrusted to him the construction of the "Atlas écliptique." Chacornac commenced his observations on the region of the small planets on June 1, 1852, and on September 20 he discovered Massalia, and on April 6, 1853, Phocæa, and that with an equatorial telescope of only thirteen centimetres aperture.

The poor resources which were at the disposal of the Marseilles Observatory did not permit of M. Valz's undertaking the publication of the ecliptic charts; and for this purpose he addressed the Academy of Sciences, which had appointed a commission to examine the question. M. Le Verrier, who at this time sought to reform the *personnel* of the Paris Observatory, called to his aid M. Chacornac, who, on March 4, 1854, was appointed Adjoint Astronomer.

At the Observatory of Paris, Chacornac had at his disposal an equatorial of 7 in. aperture, equal to that of Mr. Hind; he set down in his charts stars up to the 13th magnitude, and the limits which they embraced were at the same time somewhat extended. The publication commenced very soon after, and from 1854 to 1863, thirty-six charts, of which some contained not less than 3,000 stars, were put into the hands of astronomers.

During the construction of these charts, Chacornac discovered many small planets—Amphitrite (March 3, 1854), Polymnia (October 28, 1854), Circe (April 6, 1855), Lydia (January 12, 1856), Lætitia (February 8, 1856), Olympia (September 12, 1860). At the same time he observed all the comets which were then visible and defined, with the telescope of Foucault, of 80 centimetres, many spiral nebulæ, previously studied by Herschel. The drawings of M. Chacornac are among the most careful we possess, and appear to show that nebulæ of this kind undergo in time slight variations of form.

This collection of remarkable works brought to the Astronomer of the Paris Observatory many academic and honorary rewards: thus, he obtained the Lalande Prize in 1852, 53, 54, 55, 56, 60, and 1863, became titular astronomer February 22, 1857, and Chevalier of the Legion of Honour, August 15, 1857.

His labours, however, and their attendant anxieties, told upon his health. After going to Spain, where he went to observe the total eclipse of the sun of July 18, 1860, the ecliptic charts were issued less frequently, and in June, 1863, he quitted the Observatory to retire to Ville Urbanne, in the suburbs of Lyon.

In his country retirement, M. Chacornac, whose spirit had preserved all its activity, constructed with his own hands a telescope of three metres focus, by means of which, until within the last few months, he assiduously observed the solar spots and their manifold transformations. In the description of their incessant changes he sought new proofs of the gaseous nature of the sun, an idea which he was one of the first to announce.

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* From an article in *La Revue Scientifique*, by M. G. Rayet, Chief Astronomer of the Meteorological Service at the Paris Observatory.

SCIENCE LECTURES AT CAMBRIDGE

THE following Lectures in Natural Sciences will be given at Trinity, St. John's, and Sidney Sussex Colleges during Michaelmas Term, 1873:—

On General Physics and Mechanics. By Mr. Trotter, Trinity, in Lecture Room No. 11 (Monday, Wednesday, Friday, at 11, commencing Wednesday, Oct. 15).

On Elementary Organic Chemistry. By Mr. Main, St. John's (Tuesday, Thursday, Saturday, at 12, in St. John's College Laboratory, commencing Thursday, Oct. 16). Instruction in Practical Chemistry will also be given.

On Palæontology (the Protozoa and Cœlenterata). By Mr. Bonney, St. John's (Tuesday and Thursday at 9, commencing Thursday, Oct. 16).

On Geology for the Natural Sciences Tripos. Preliminary matter and Petrology. By Mr. Bonney, St. John's (Monday, Wednesday, and Friday, at 10, commencing Wednesday, Oct. 15.) A Course on Physical Geology will be given in the Lent Term, and on Stratigraphical Geology in the Easter Term.

Papers will be given to Questionists every Saturday at 11, but the first paper will be set on Wednesday, Oct. 15, at 11, when arrangements will be made for further instruction should it be required.

On Botany, for the Natural Sciences Tripos. By Mr. Hicks, Sidney (Tuesday, Thursday, Saturday, at 11, in Lecture Room No. 1, beginning on Thursday, Oct. 16). The Lectures during this term will be on the Morphology of Phanerogamia.

A Course of Practical Physiology and Histology. By the Trinity Prælector in Physiology (Dr. Michael Foster) at the New Museums. Lectures on Tuesday, Thursday, Saturday, at 12, commencing Saturday, Oct. 25.

This course is intended for those who have gone through a course of Elementary Biology similar to that given last Easter Term.

THE AMERICAN ASSOCIATION

THE Portland Meeting of the American Association for the Advancement of Science was in almost every respect an exceptional success. Its general attendance was very large, and there was an unusual number of the older members, whose presence insures consideration of the more important topics, and gives dignity and force to the discussions. An especial effort had been made to exclude all inferior communications. A regulation had been adopted, compelling the presentation of an abstract of each paper before it was read; and the examining committee in determining from abstracts what papers should be read, exercised in general a rigorous but wise discretion. It will not be the case after this, as after previous meetings, that a considerable proportion of the communications actually read will have to be ignored in the printed proceedings. But even under such restrictions, the number of papers actually read was unusually large, and there were but few instances, as compared with previous years, of the pernicious practice of reading papers by title only—a practice which, if pushed to its logical conclusion, would result in the destruction of the meetings.

The discussions were kept well in hand, wandering but little from the subject, and being, though frequently brilliant, notably brief. There was in them almost an entire absence of any display of feeling, except an occasional expression of kindly regards between opponents whose differences did not extend beyond the debate; in fact, the cordiality of the meeting was one of its prominent features.

The newspaper press sent correspondents from distant cities—New York, Boston, and Chicago being well represented. The *New York Tribune* announced that its reports would be re-published in an extra, and determined to make that extra cover, with at least a fair extract, every communication read and accepted at the meeting, and the discussions elicited. The practical difficulties in the way of such an undertaking are considerable. All the sub-sections of the Association carry on their proceedings in separate rooms simultaneously. Many of the communications are technical, abstruse, and difficult to report, and have not been reduced to writing; it being the custom of some authors to delay preparation of MSS. for the official report till some months after the close of the meeting. Notwithstanding these obstacles and the expense involved in overcoming them, the extra was brought out with all the completeness proposed; thus anticipating the usual official publication by almost a year. It is a sheet of eight pages, and gives also an illustrated series of letters upon Deep Sea Dredging, as practised by the United States Commission of Fish and Fisheries, the whole containing as much reading-matter as would make a large duodecimo volume. The extra is sold for ten cents, this price including postage.

NOTES

SIR SAMUEL and Lady Baker arrived in London on Thursday evening last. The young African, a lad of about fifteen or sixteen years of age, in whom Lady Baker is said to take much interest, accompanied the party. Both Sir Samuel and Lady Baker looked well, and seemed in excellent spirits.

FOR the Biological Fellowship examination at Magdalen College, Oxford, there are five candidates, of which we are surprised to hear that three are graduates of the University of Cambridge. The election takes place on Saturday next.

MR. EDWARD BAGNALL POULTON, from Mr. Watson's School, Reading, has been elected to an open Physical Science Scholarship of 80*l*. per annum, in Jesus College, Oxford.

MISS POGSON, daughter of the Government Astronomer at Madras, has been appointed Assistant Astronomer.

THE American aëronaut, Mr. Samuel A. King, intends during September to make an extended balloon voyage from Buffalo, New York. For this purpose he is building a large balloon to replace the "Mammoth," which was destroyed by the recent great fire in Boston. It is Mr. King's purpose to make the longest overland voyage, if circumstances favour, ever yet accomplished. It is no part of his plan to go out over the ocean, nor to explore the sea, but he expects to be able to settle something about the upper currents when he comes down. His voyage is undertaken wholly in the interest of science, and, in view of the extraordinary degree of attention now being drawn to the subject of meteorology, the results will be regarded as of much more than ordinary importance. From a communication made by Mr. King in 1871 to the Washington Philosophical Society, it appeared that out of 170 aerial voyages made by him during the past twenty-five years, about twenty-five per cent. showed that the currents of the atmosphere were moving to the north-eastward; a second twenty-five per cent. gave westerly currents; and a third gave north-westerly currents. The remaining forty voyages were about equally distributed among northerly, southerly, and easterly currents. Mr. King's experience, therefore, agrees with that of most European aeronauts, who have repeatedly testified that there is no constant westerly current of air prevailing at any altitude above the earth's surface which they have been able to reach in their balloons.

CANADA is doing its part toward the exploration of the Great West. Besides the surveying parties out on the route of the Pacific Railroad, it has special parties in the field in connection with the Geological Survey and the Boundary Commission. Mr. Selwyn, F.G.S., Director of the Survey, and Mr. R. Bell, F.G.S., are at work on the great regions watered by the North Saskatchewan, and Mr. Richardson on the other side of the Rocky Mountains in British Columbia. Mr. G. M Dawson, Associate of the School of Mines, Geologist of the Boundary Commission, has just completed a survey of the Lake of the Woods and its neighbourhood, and is now exploring the plains westward of Pembina. All these parties are provided with the means of making collections in the botany and zoology of the regions explored.

MR. J. A. HARVIE BROWN has sent us a reprint of an article by him which appeared in the *Scottish Naturalist* for July, advocating the establishment of a British Naturalist's Agency, on the model of the "American Naturalist's Agency," established at Salem, Mass. U.S. The American Agency has flourished and brought forth abundant and good fruit, and in an incredibly short space of time has become the acknowledged medium for the sale of the proceedings of all the learned societies in America, and through which advertisers on all natural history subjects make known their wants. The main purpose of the Agency is to facilitate the circulation of papers and pamphlets on Natural History, which, from the want of such an Agency, many who wish to possess them find it difficult to obtain, and which are often not even known beyond a narrow local circle. The Agency also undertake to publish new and republish old standard works in Natural History, and perform several other eminently useful offices which can only be sufficiently performed by some such central organisation. The very existence of such an Agency would create a demand for scientific knowledge. Such an Agency in this country would undoubtedly prove a great boon to naturalists, provided it were ably conducted, and fully acknowledged and supported by the leading scientific societies. Scientific circles in time, we believe, would be enlarged, and not be confined to the metropolis, or nearly so. There are plenty of good men out of London, Edinburgh, Glasgow, and the large towns who have no opportunities of reading, being removed from the principal scientific libraries. Not one individual, nor indeed any one society, could set such an undertaking afloat, but if all the leading societies would jointly discuss its merits and demerits, and at length bring it carefully and repeatedly before the notice of the British Association, there is every likelihood that it would become a complete success. To arrive at this first step it is necessary to ventilate the suggestion, and this cannot be better done than by bringing it before the notice of the local societies, and asking each to assist in bringing it finally before a higher court. Parties interested and desirous of seeing such a scheme successful may communicate with Dr. F. Buchanan White, editor of *Scottish Naturalist*, Perth, or with John Harvie Brown, Dunipace House, Falkirk.

ON Monday last a meeting was held at the Mansion House with the view of promoting technical education in the City. The meeting was immediately held in connection with the distribution of prizes by the Turners' Company, for the best specimens of workmanship in the turning of articles in ivory and stone. It is creditable to this Company that it has by this means been endeavouring to promote technical education for some years past, and if all the other City Companies took the trouble to follow the Turners' example, and encourage the introduction into the various trades and handicrafts with which they are connected of a scientific method of workmanship founded upon scientific knowledge of material and on sound scientific theory, we believe they would be doing eminent service that would be fruitful of the best results to the trade and commerce and manufactures of the kingdom.

MR. T. W. BURR writes us that he has, since 1853, been in the habit of using a sidereal dial similar to that described by Captain Mayne, in NATURE, vol. viii. p. 366.

THE death of Prof. John Lewis Russell is announced as having taken place at Salem, U.S., on the 7th of June, in the sixty-fifth year of his age. Prof. Russell was well known as an ardent student of botany, and especially in the department of New England cryptogams, in which he was a recognised authority. He took much interest in the scientific societies of Salem, having been connected more or less with their foundation and administration during the active years of his life.

PROF. AGASSIZ has recently lost one of his most valuable assistants in the death of Dr. G. A. Maack, on the 6th of August last, in the thirty-third year of his age. He was connected with the Cambridge Museum for several years, during which time he was detailed by his chief to act as geologist of the Darien Isthmus exploring party, under Commander Selfridge, and also prosecuted similar researches in Brazil and elsewhere in South America. He was specially charged with the osteological collection of the Cambridge Museum, which he managed with great ability.

THE death is announced of Mr. George Ormerod, of Sedbury Park, Gloucestershire, F.R.S., F.S.A., D.C.L., &c., a well-known antiquary. He was eighty-seven years of age.

THE *Journal of Botany* records the death of Dr. J. Lindsay Stewart, late Conservator of forests in the Punjaub, who had rendered great service to the cause of forest administration in India, by the commencement of the large and now flourishing plantations in the plains of the Punjaub, and who was also a copious writer on Indian botany.

"CONTRIBUTIONS to our knowledge of the Meteorology of the Antarctic Regions," published by the Meteorological Committee, will be of value both to meteorologists and to future Antarctic navigators. The work has been executed by Mr. R. Strachan, and the materials which form the paper have been extracted from the Meteorological Registers kept in the Antarctic Regions, on board H.M.S. *Erebus* and *Terror*, during the months December 1840—March 1841, December 1841—March 1842, December 1842—March 1843, and on board H.M. sloop *Pagoda* during January–March 1845.

As a result of the inquiry into the recent typhoid epidemic, we are glad to see that the Dairy Reform Company have secured the co-operation of Prof. Corfield, M.D., Prof. Voelcker, Ph.D., and Prof. Wanklyn, to carry out the precautions which have been adopted. A medical and veterinary examination of the employés and stock on each farm is made every week, and reports are forwarded to the Company's chief office in Orchard Street, where they are open to the inspection of customers from 10 A.M. to 4 P.M., on week days. Orders of admission to all their establishments have been given to the medical officers of health for the following districts:—St. James's, Marylebone Kensington, St. George's, Paddington, Chelsea, and St. Pancra

WITH reference to our note in last week's number concernin the *Leeds Daily News*, we are glad to be able to say that th *Leeds Mercury* and the *Yorkshire Post and Leeds Intelligencer* als report the transactions of the Leeds Naturalists' Field Club.

MESSRS. CHURCHILL have in the press and will publish durin the ensuing season the following works of interest to scientifi men :—"On Food, Physiologically, Dietetically, and Therapeu tically considered," by F. W. Pavy, M.D., F.R.S.; a third an enlarged edition of Dr. Lionel Beale's "Protoplasm, Dissentien Demonstrative, and Speculative," with 16 plates; a secon edition of "The Thanatophidia of India," by J. Fayre M.D., C.S.I.; a new illustrated work on "Medicinal Plants by Robert Bentley, F.L.S., and Henry Trimen, M.B., F.L.S This work will include full botanical descriptions and an accoun of the properties and uses of the principal plants employe in medicine, especial attention being paid to those which ar officinal in the British and United States Pharmacopœia The plants which supply food and substances required by th sick and convalescent will be also included. Each species wi be illustrated by a coloured plate drawn from nature. Th work will be published in monthly parts, of which w may expect the first very shortly. A translation by Arthu E. J. Barker, of Frey's "Manual of the Histology an Histo-Chemistry of Man," a treatise on the elements o structure and composition of the human body; the book will b largely illustrated with engravings on wood, and speciall revised by the author. "The Microscope and its Revelations," by Dr. W. B. Carpenter, F.R.S.; a new edition with upward of 500 engravings." "Experimental Investigations of th Action of Medicines;" being a handbook of Practical Pharmacology, with engravings, by T. Lander Brunton, M.D., one of th lecturers at St. Bartholomew's Hospital; "The Student's Guid to Zoology," with engravings on wood, by Andrew Wilson, Lecturer on Zoology at Edinburgh and author of "Elements o Zoology;" "On Long, Short, and Weak Sight, and the Treatment by the Scientific use of Spectacles," by J. Soelber Wells, F.R.C.S., fourth edition, with engravings.

MESSRS. BLACKWOOD will shortly publish, "Economic Geology, or Geology in its relation to the Arts and Manufactures," by David Page, LL.D.; and an "Advanced Text-Book o Botany," for the use of Students, by Dr. Robert Brown, F.R.G.S., with numerous Illustrations.

MESSRS. STRAHAN & Co. announce, as nearly ready, "The Great Ice Age and its Relation to the Antiquity of Man," b James Geikie, F.R.S.E., of H.M. Geological Survey. Thi work will be copiously illustrated.

THE third session of the Newcastle College of Science commenced on Tuesday, presided over by the Dean of Durham. Prof. Herschel delivered an address. The necessity for shortl providing more accommodation was considered, and it wa understood that an effort was about to be made to raise funds fo a new college. The very rev. chairman also mentioned that College of Agriculture was about to be founded in Central Northumberland in connection with the University of Durham.

THE annual distribution of prizes to the successful competitor in the Guildford Science and Art classes, awarded by the Government Department of Science and Art, took place on th evening of October 1, at the Town Hall. In addition to th Guildford prizes those won by the students of St. John's, Woking were also distributed, as well as the Night Art Class of th Guildfold Working Men's Institute. The number of student has continued steadily to increase upon former years, 62 havin attended the classes during the last winter session. Of these, 35 came up for examination in May, and 23 passed. Several of these obtained very advanced success in more than one subject, so that the total number of successful candidates in the seven subjects taught amounts this year to 49, including four outside candidates, leaving an increase of 13 from last year. Mr. Ethelbert Dowlen, one of the pupils, has been awarded the "Queen's Silver Medal" in botany, and besides numerous other prizes and certificates, he also obtained the "Queen's Gold Medal" for geology at St. John's College, Woking. Altogether these classes seem to have been highly successful, and we hope they will continue to be increasingly so. The classes will be re-opened for instruction on Tuesday, 27th inst., and will be continued every Monday and Wednesday evening for Physical Geography, and on Tuesdays and Fridays, from 6 to 9 P.M., in the other subjects. A class will be held on Saturdays for ladies, in Botany, at a convenient time, commencing from the 11th inst. at 11 A.M. Proposed Subjects:—1, Mathematics (1st, 2nd, and 3rd stage), or theoretic mechanics; 2, sound, light, and heat; 3, magnetism and electricity; 4, chemistry, inorganic; 5, animal physiology; 6, elementary botany; 7, biology; 8, physical geography. The fees are very moderate.

THE volume of Artizans' Reports upon the Vienna Exhibition, published by the Society for the Promotion of Scientific Industry, Manchester, will be published about the 20th of this month. There are thirty-six reports, which are said to be of a very high-class character.

WE are glad to see, from the Report of the Chester Society of Natural Science, that that Society, which has concluded its second year, continues to increase in prosperity so far as numbers are concerned—the number of members being now 454. Among these are not a few working members; and the secretary gives excellent advice in counselling each member to devote himself to a special subject, as thus only can the interest of the Society and the advance of science be best promoted. During the past year two societies of natural science have been founded in the neighbourhood of Chester—one at Wrexham, the other at Whitechurch. The Chester Society does its work by means of field excursions, general lectures, and sectional meetings.

THE forthcoming number of Petermann's *Mittheilungen* will contain a detailed account of Captain Hall's *Polaris* Arctic expedition, with its scientific results. It will be accompanied by a carefully constructed map showing the course of the *Polaris* from the 80th degree northwards, her course southward from Aug. 15 to Oct. 15, 1872, the course along which the floe containing the nineteen persons drifted after they were separated from the ship on the night of Oct. 15, 1872, until they were picked up off the coast of Labrador six months afterwards, the distance drifted each day, along with the state of the weather, and the places where seals, &c. were obtained, being indicated; and lastly, the course taken by the men who were picked up in Melville Bay last June.

SHORTLY before his death the late Colonel J. W. Foster completed the manuscript of a work upon the prehistoric races of the United States, which has just made its appearance from the press of S. C. Griggs and Co., of Chicago. This contains an excellent summary of the present state of our knowledge of the aborigines of North America, as illustrated by the remains found in mounds, shell heaps, and ancient mines, as well as by their crania.

THE City of London College, Leadenhall Street, to judge from the programme we have received, offers excellent opportunities to young men engaged during the day for obtaining a good education, literary and scientific, and for intellectual improvement in various ways.

THE *Times of India* says that a scientific geographical survey of native Sikkim is in contemplation by the authorities.

THE *Geological Magazine* announces the death of Prof. Dr. Kemp of Darmstadt, a distinguished zoologist and palæontologist, whose name is well known in connection with the discovery of the *Dinotherium*.

HERR SCHLOENBACH, proprietor of certain salt works at Lieberhall, in Hanover, has instituted a foundation of 12,000 florins, the interest of which is to be devoted to assist geologists who may undertake journeys of exploration beyond the Austro-Hungarian empire. This is intended as a memorial tribute to his son, a young German geologist of much promise, recently deceased.

THE additions to the Zoological Society's Gardens during the last week include an Arctic Fox (*Canis lagopus*) and an Iceland Gull (*Larus leucopterus*), European, presented by Mr. B. L. Smith; a Black-handed Spider-monkey (*Ateles melanochir*) from South America, presented by Mr. B. Went; an African Civet Cat (*Viverra civetta*), presented by Lady Cust; a Macaque Monkey (*Macacus cynomolgus*) from Africa, presented by Capt. Denison; a Raccoon (*Procyon lotor*) from North America, and a Vulpine Phalanger (*Phalangista vulpina*) from Australia, presented by Miss Breach.

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICS

On the Introduction of the Decimal Point into Arithmetic, by J. W. L. Glaisher, B.A.

The following is an extract from Peacock's excellent History of Arithmetic, in the "Encyclopædia Metropolitana," which forms the stanndard (not to say the only) work on the subject. Speaking of Stevinus's "Arithmétique," Peacock writes: "We find no traces, however, of decimal arithmetic in this work, and the first notice of *decimal*, properly so called, is to be found in a short tract, which is put at the end of his 'Arithmétique,' in the collection of his works by Albert Girard, entitled 'La Disme.' It was first published in Flemish, about the year 1590, and afterwards translated into barbarous French by Simon of Bruges. . . . Whatever advantages, however, this admirable invention, combined as it still was with the addition of the exponents, possessed above the ordinary methods of calculation in the case of abstract or concrete fractions, it does not appear that they were readily perceived or adopted by his contemporaries. . . . The last and final improvement in this *Decimal Arithmetic*, of assimilating the notation of integers and *decimal* fractions, by placing a *point* or *comma* between them, and omitting the exponents altogether, is unquestionably due to the illustrious Napier, and is not one of the least of the many precious benefits which he conferred upon the science of calculation. No notice whatever is taken of them in the 'Mirifici Logarithmorum Canonis Descriptio,' nor in its accompanying tables, which was published in 1614. In a short abstract, however, of the theory of these logarithms, with a short table of the logarithms of natural numbers, which was published by Wright, 1616, we find a few examples of decimals expressed with reference to the decimal point; but they are first distinctly noticed in the 'Rabdologia,' which was published in 1617 In an 'Admonitio pro decimali Arithmetica,' he mentions in terms of the highest praise the invention of Stevinus, and explains his notation; and without noticing his own simplification of it, he exhibits it in the following example, in which it is required to divide 861094 by 432. . . . The quotient is 1993,273, or $1993,2'7''3'''$, the form under which he afterwards writes it, in partial conformity with the practice of Stevinus. The same form is adopted in an example of abbreviated multiplication, which subsequently occurs. . . . The preceding statement will sufficiently explain the reason why no notice is taken of *decimals* in the elaborate explanations which are given by Napier, Briggs, and Kepler, of the theory and construction of logarithms; and indeed we find no mention of them in any English author between 1619 and 1631. In that year the 'Logarithmicall Arithmetike,' was published by Gellibrand, and other friends of Briggs, who died the year before, with a much more detailed and popular explanation of the doctrine of logarithms than was to be found in the 'Arithmetica Logarithmica.' It is there said . . . From this period we may consider the decimal arithmetic as fully established, inasmuch as the explanation of it began to form an essential part of all books of practical Arithmetic. The simple method of marking the separation of the decimals and integers by a comma, of which Napier has given a solitary example, was not however generally adopted." . . .

De Morgan ("Arithmetical Books," 1847, p. xxiii.) writes: "Dr. Peacock mentions Napier as being the person to whom the introduction [of the decimal point] is unquestionably due; a position which I must dispute upon additional evidence. The inventor of the single decimal distinction, be it point or line, as in 123·456, or 123 | 456, is the person who first made this distinction a permanent language; not using it *merely as a rest in the process*, to be useful in pointing out afterwards how another process is to come on, or language is to be applied, but making it his final and permanent indication as well of the way of pointing out where thei ntegers end and the fractions begin, as of the manner in which that distinction modifies operations. Now first I submit that Napier did not do this; secondly, that if he did do this, Richard Witt did it before him."

De Morgan then states that he has not seen Wright's translation of 1616, but he proceeds to examine Napier's claim as resting on the two examples in the "Rabdologia," in the first of which a comma is used, but only in one place. After this examination he proceeds, "I cannot trace the decimal point in this: but if required to do so, I can see it more distinctly in Witt, who published four years before Napier. But I can hardly admit him to have arrived at the notation of the decimal point. . . ."[*]

I agree with De Morgan in all that he has stated in the above extracts, and do not think that the single instance of the comma used in the course of work, and replaced immediately afterwards by exponential marks, is a sufficient ground for assigning to Napier the invention of the decimal point, or even affords a presumption that he made use of it at all in the expression of results.

Still one of the objects of this paper is to claim (provisionally of course, till evidence of any earlier use is produced, if such there be) the invention of the decimal point for Napier, but not on account of anything contained in the "Rabdologia." The mathematical works published by Napier in his life-time (he died in 1617) were his "Mirifici Logarithmorum Canonis Descriptio," 1614, containing the first announcement of the invention of logarithms; and the "Rabdologia," 1617, giving an account of his almost equally remarkable (as it was thought at the time) invention of numbering rods or "bones." In 1619, two years after his death, the "Mirifici Logarithmorum Canonis Constructio," containing the method of construction of the canon of logarithms was published, edited by his son, and in this work the decimal point is systematically used in a manner identical with that in which we employ it at the present day. I can find no traces of the decimal point in Wright's translation of the "Descriptio," 1616, and, as De Morgan says, the use of the decimal separator is not apparent in Witt. The earliest work, therefore, in which a decimal separator was employed seems to be Napier's posthumous work, the "Constructio" (1619), where the following definition of the point occurs on p. 6. "In numeris periodo sic in se distinctis, quicquid post periodum notatur fractio est, cujus denominator est unitas cum tot cyphris post se, quot sunt figuræ post periodum. Ut 10000000·04 valet idem, quod $10000000\frac{4}{100}$. Item 25·803, idem quod $25\frac{803}{1000}$. Item 9999998·0005021, idem valet quod $9999998\frac{5021}{10000000}$, et sic de cæteris." On p. 8 we have 10·502 multiplied by 3·216, and the result found to be 33·774432; and on pp. 23 and 24 occur decimals not attached to integers, viz. ·4999712 and ·0004950. These show that Napier was in possession of all the conventions and attributes that enable the decimal point to complete so symmetrically our system of notation, viz. (1), he saw that a point or separatrix was quite enough to separate integers from decimals, and that no signs to indicate primes, seconds, &c., were required; (2), he used ciphers after the decimal point and preceding the first significant figure, and (3), he had no objection to a decimal standing by itself without any integer. Napier thus had complete command over decimal fractions and understood perfectly the nature of the decimal point, and I believe (except perhaps Briggs) he is the first person of whom this can be said. When I first read the "Constructio," I felt some doubt as to whether Napier really appreciated the value of the decimal point in all its bearings, as he seemed to have regarded it to some extent as a mark to separate figures that were to be rejected from those that were to be retained; but a careful examination has led me to believe that his views on the subject were pretty nearly identical with those of a modern arithmetician. There are perhaps 200 decimal points in the book, affording abundant evidence on the subject.

The claim of Napier to the invention of the decimal point is not here noticed for the first time, as both Delambre ("Hist. de l'Astron. mod. t. i. p. 497) and Hutton allude to the decimal fractions in the "Constructio" (though the latter claims priority for Pitiscus), and Mr. Mark Napier ("Memoirs of John Napier," p. 454) devotes a good deal of space to it.

Briggs also used decimals, but in a form not quite so convenient as Napier; thus, he writes 63·0957379 as $63\underline{0957379}$, viz., he prints a bar under the decimals: this notation first appears without any explanation, in his "Lucubrationes," appended to the "Constructio."[*] Briggs used this notation all his life (he died in 1631), and he explains it in the "Arithmetica Logarithmica" of 1624. Oughtred's symbol first used (as far as I know), in his "Arithmeticæ in numeris"...Clavis, 1631, differed only from Briggs's in the insertion of a vertical bar to separate the decimals from the integers more completely, thus: $63\,|\underline{0957379}$. Oughtred's and Briggs's notation are essentially the same, the improvement of the former being no doubt due to the uncertainty that sometimes might be felt as to which was the first figure above Briggs's line.

From an inspection of MSS. of Briggs and Oughtred (the Birch MSS. contain a letter of Briggs's to Pell, and the Royal Society has a Peter Ramus with many of his MS. notes, while the Cambridge University copy of the "Constructio" is annotated in MS. by Oughtred), it is apparent that in writing, Briggs and Oughtred both made the separating rectangle in exactly the same way, viz., they wrote it $63\,\lfloor\underline{0957379}$, the upright mark usually being just high enough to fix distinctly what two figures it was intended to separate, and rarely took the trouble to continue the horizontal bar to the end of the decimals, if there were many. Thus Oughtred was a follower of Briggs, and only made an improvement in the *printed* notation. It is clear that in writing Briggs's rectangle was pretty nearly as convenient as Napier's point, and there is every probability that Briggs appreciated all the properties of the "separatrix" as clearly as Napier; but in his 8 pp. of "Lucubrationes" he has left much less to judge by than has Napier. In 1624, as we can see from his "Arithmetica Logarithmetica," he had full command over decimal arithmetic in its present form (except that he used the rectangular "separatrix" instead of the point). Gunter was a follower of Napier, and employed the point (but see De Morgan). In his "Description and Use of the Sector" (1623), he uses the point throughout pretty much as we do at present (*e.g.* p. 41 of the "First Booke of the Crosse-staffe": "As 4·50 unto 1·00 : so 1·000 unto 0·222"), except that he calls the decimals *parts* in the text. In Roe's "Tabulæ Logarithmicæ, or Two Tables of Logarithme" (1633), the explanatory portion of which was written by Wingate, decimal points are used everywhere; thus we have (p. 29): "As 1 is to ·079578 : so is the square of the circumference to the superficial content," and he takes the case of circumference 88·75, and obtains by multiplication (performed by logarithms) 626·8 for the result. Wingate refers for explanation on the decimal point to his arithmetic, but I have not seen any edition of this work that was published previously to Roe's tables (Watt gives one, 1630). In his "Construction and Use of the Line of Proportion" (1628), Wingate also uses decimals and decimal points.

On the whole, therefore, it appears that both Napier and Briggs saw that a mere separator to distinguish integers from decimals was quite sufficient, without any exponential marks being attached to the latter; but that Napier used a simple point for the purpose, while Briggs employed a bent or curved line, for which in print he substituted merely a horizontal bar subscript to the decimals; that Gunter and Wingate followed Napier, while Oughtred adopted Briggs's method and made an improvement in the mode of *printing* it. Napier has left so many instances of the decimal point as to render it pretty certain that he thoroughly appreciated its use; and there is every reason to believe that Briggs had, in 1619, an equal command over his separator, although there are not enough printed instances of that date to prove it so conclusively as in Napier's case (there is no instance in the "Lucubrationes" in which a quantity begins with a decimal point, and there could not well be one). Napier did not use the decimal point in the "Descriptio" (1614), nor in his book of arithmetic first printed under the editorship of Mr. Mark Napier in 1839, and there is only the single doubtful case in the "Rabdologia," 1617, so that there is reason to believe that he did not regard it as generally applicable in ordinary arithmetic. The only previous publication of Briggs's that I have seen was his "Chilias," 1617, which contains no letterpress at all. The fact that Napier and Briggs use different separating notations is an argument against either having been indebted to the other, as whoever adopted the other's views would probably have accepted his separator too. It is doubtful whether, if Napier had written an ordinary arithmetic at the close of his life he would have used his decimal point. Wingate employed the decimal point with much more boldness, and regarded it much more in the light of a permanent symbol of arithmetic than did (or could) Napier. The Napierian point and the Briggian separator differ but little in writing, and as far as MS. work is concerned it is quite easy to see why many should have considered the latter preferable, for it was clear and interfered with no existing mark. A point is the simplest separator possible, but it had already another use in language. In all the editions of Oughtred's "Clavis" (which work held its ground till the beginning of the last century) the rectangular separator was used, and it is not unlikely that it was ultimately given up for the same reason as that which I believe will lead to the abandonment of the similar sign now used in certain English books to denote factorials, viz., because it was troublesome to *print*. But be this as it may, it is not a little remarkable that the first separator used (or more strictly, one of the first two) should have been that which was finally adopted after a long period of disuse. All through the seventeenth century exponential works seem to have been common, on which see the accounts in Sir Jonas Moore's "Moor's Arithmetick," London, 1660, p. 10; and Samuel Jeake's "Compleat Body of Arithmetick," London, 1701 (written in 1674), p. 208, which are unfortunately too long to quote in this abstract. In his account Peacock is inaccurate in saying that the "Logarithmicall Arithmetike" was published by Gellibrand and others, the mistake having arisen, no doubt, from a confusion with the "Trigonometria Britannica," 1633; and in any case the reference is not a good one, as the "Arithmetike" of 1631 shows (for reasons which must be passed over here) a less knowledge of decimal arithmetic than do any of the chief logarithmic works of this period. Also Briggs died in 1631, not 1630.

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[*] In an essay "On some points in the History of Arithmetic" (Companion to the Almanac for 1851), De Morgan has further discussed the invention of the decimal point, but in the same spirit as regards Napier. He seems never to have seen Napier's "Constructio" of 1619, and the work is very rare. The only copy I have been able to see is that in the Cambridge University Library.

[*] A curious blunder is made in Bartholomew Vincent's reprint of the "Constructio," Lyons, 1620 (of which there is a copy in the Royal Society's library). The printer, unaware that the position of Briggs's subscript bars had any meaning, has disposed them symmetrically under all the figures.

There is no doubt, whatever, that decimal fractions were first introduced by Stevinus in his tract, "La Disme." De Morgan ("Arithmetical Books," p. 27) is quite right in his inference that it appeared in French in 1585, attached to the "Pratique d'Arithmétique." A copy of this work (1585) with "La Disme" appended, is now in the British Museum. On the title-page of the "Disme" are the words "Premierement descripte en Flameng, et maintenant conuertie en François, par Simon Stevin de Bruges." These words appearing also in Albert Girard's collected edition of Stevinus's works (1634) no doubt gave rise to De Morgan's inference that "the method of decimal fractions was announced before 1585 in Dutch." The Cambridge University Library possesses a 1585 copy, entitled "De Thiende . Beschreven door Simon Stevin van Brugghe. ..Tot Leyden. By Christoffel Plantijn, M.D. LXXXV." (privilege, dated December 20, 1584), and there seems every reason to believe, in the absenc of any evidence to the contrary, that this was the first edition of this celebrated tract. Peacock's statement that "it was first published in Flemish about the year 1590, and afterwards translated into barbarous French by Simon of Bruges" is also, I suspect, founded on no other evidence than the sentence on the title-page of the "Disme," which appears also in Girard. De Morgan rightly remarks that Simon of Bruges is Stevinus himself, but he cannot tell whence Peacock derived the date 1590. It is probable that it was merely a rough estimate obtained by considering the dates of the other works of Stevinus.

Stevinus's method involved the use of his cumbrous exponents. Thus he wrote 27·847 as 27(0)8(1)4(2)7(3)[*] and read it 27 commencements, 8 primes, 4 seconds, 7 thirds; and the question chiefly noticed in this abstract is the consideration of who first saw that by a simple notation the exponents might be omitted, and introduced this abbreviation into arithmetic.

Napier's "Rabdologia" was translated into several languages soon after its appearance, and I have taken some pains to examine the different ways in which the translators treated the example which Peacock regarded as the first use of the decimal point, as we can thereby infer something with regard to the state of decimal arithmetic in the different countries. Napier (1617) wrote 1993,273 in the work, and $1993,2'7''3'''$ in the text. In Locatello's translation (Verona, 1623) this is just reversed, viz. there is $1993{\cdot}2'7''3'''$ in the work, and 1993,273 in the text. The Lyons edition (1626) has 1993,273 in the work, and 1993,2(1)7(2)3(3)[†] in the text, while De Decker's edition (Gouda, 1626) has 1993,273 in the work, and in the text 1993(0)2(1)7(2)3(3), the last being exactly as Stevinus would have written it. Ursinus's "Rhabdologia," Berlin, 1623, is not an exact translation, and the example in question does not occur there.

SANITARY PROGRESS[‡]

SANITARY science is a thing of yesterday, comparatively speaking; but sanitary art, the art of preserving the health, whether of individuals or of communities, has been studied and practised for ages. Sanitary science is the latest and highest development of medicine. I say it is the highest branch of medical science because of the extreme importance of its objects, and I may also add of its results. It is the study of the causes of diseases, and it points out the means of preventing them; and I am sure you are all agreed that "prevention is better than cure;" as Rollet of Lyons well said, "Medicine cures individuals, hygiene saves the masses." But while we contrast hygiene (another name for sanitary science) with curative medicine, we must not forget that it is altogether a medical science, and that its great lights have been all medical men (mind, I am not speaking of the art now, but of the science), and this is necessarily so, and always must be so. I have said that sanitary science is the study of the causes of diseases, of the modes in which they originate, and in which they spread from one person or place to another. It is therefore only those who are acquainted with disease, that are competent to deal with it all, and these are those who have made medical science generally their special subject. You sometimes hear it said that medical men don't know much about diseases. Just think what this means; disease has been studied by earnest men in all its various forms for thousands of years; experiences have been recorded, comparisons made; the effects of remedies noted from generation to generation, and yet we are asked to believe that medical men don't know anything about diseases; the thing is absurd on the face of it.

Sanitary science is, then, a medical science, and the most intimate acquaintance with diseases is necessary for its prosecution—I mean for its advancement as a science. Sanitary investigations can only be scientifically conducted by medical men, just as pianos can only be played by musicians. This science is also the latest development of medical science. We must understand simple things before we can study complex ones. It is little use for a boy to study higher algebra until he has mastered the rule of three; and so pathology, or the study of diseased actions, becomes more and more advanced as physiology—the study of normal healthy actions—is more scientifically pursued; while the study of sanitary matters in a scientific way has only become possible of later years from the great advances made in the study of pathology, physiology, and chemistry; but being possible, it has made such rapid strides, and evolved such startling facts with regard to the causes of diseases, that it has become the popular subject of the day. Everyone thinks that he is competent to speak about it, and everyone who wants to make an effective discourse must needs take upon himself to expound some, to him, new view of sanitary matters; this is very mischievous. A man may do more harm by giving the weight of his authority to erroneous views respecting the method to be employed for the prevention of diseases than he has done good during the whole of his life in any other way. None but those who have made a special study of this subject have a right to speak on it, or at any rate have a right to influence the public mind with regard to it. The amount of good which may be done by the exposition of correct views on sanitary matters is incalculable; the amount of evil done by the enunciation of erroneous views, backed by apparent authority, fearful.

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[*] Stevinus enclosed the exponent-numbers in complete circles, which have been replaced above, for convenience of printing, by parentheses.

[†] These parentheses are printed instead of the circles which appear in these works as in Stevinus.

[‡] Abstract of the Inaugural Lecture delivered at the Town Hall, Birmingham, Thursday evening, Oct. 9, 1873, by Prof. Corfield, M.D. Oxon

But if sanitary science is a thing of yesterday, such is not the case with the observation of sanitary facts, nor with the practice of sanitary art; and, while it is true that sanitary science is essentially and entirely a medical study, and is necessarily so, it is equally true that the practice of the art of preserving the health is not only possible to all, but is a duty which devolves upon all. In all ages we have had writers on this subject. From all countries we may learn useful lessons about it. From the times of Hippocrates, Galen, and Celsus, we have had records of the results of observations on the methods of preserving the health; from the time of Moses we have had lawgivers imposing salutary conditions of existence upon unwilling, because ignorant populations. We look upon the immense engineering works undertaken and carried out by the Romans to supply their towns with pure water with astonishment, when we turn round and see our own towns supplied from polluted rivers, or, worse still, from shallow wells dug in the soil upon which they themselves stand, wells supplied in most cases chiefly by the foul water which has percolated from the surface of the ground. We have found out in later times that one of the main conditions of the health of communities depends on the purity of the drinking water, and we see that the Roman engineers, by having to go to a considerable distance for water in order to get it to a sufficient height in their cities, accidentally, as it were, fulfilled one of the most important of sanitary requirements.

"Knowledge is power," and as we come to know more of the conditions which favour the spread of diseases, as we do daily, it is our own fault if we neglect to use the power which that knowledge gives us. There are two conditions of insalubrity which are pre-eminent. I hardly know which to place first. The one is overcrowding, and the other the accumulation of refuse matters in and about dwellings. These conditions were those which especially favoured the spread of the fearful plagues of the middle ages; as a result of over-crowding we have a deteriorated condition of the air, from the diminution of the amount of its most essential constituent, oxygen; and, worse still, we have it rendered foul by the exhalation of decomposing organic matters from the bodies of the persons breathing it. Such a state of air is especially favourable to the multiplication of the poisons of diseases; such a state of the air is also brought about by the non-removal of refuse matters from the vicinity of habitations. Dr. Laycock tells us that the plague in York in each of its visitations, and also the cholera, broke out in the same abominably filthy places; and in cholera epidemics it has been repeatedly noticed that those parts of towns which are most filthy and most over-crowded, always suffer worst.

But the danger is not only from special epidemic diseases. Such insanitary conditions induce a lowered vitality of the inhabitants, who become prone to attacks of diseases of all sorts; and then we have sickness, inability to work, and consequent inability to earn bread and to pay rents, and so the evil recoils from the tenants upon the landlords. One witness says, "Rent is the best got from healthy houses." Another, "Sickness at all times forms an excuse for the poorer part not paying their rent, and a reasonable excuse."

I consider that one of the most important conclusions that the study of sanitary science has forced upon us lately is the conclusion that the immediate removal of refuse matters is one of the first necessities of the healthy existence of a community. There are those who would have you believe that refuse matters may be rendered innocuous in one way or another, so that they may be kept with safety in and near to houses. Don't listen to them; the principle is wrong—radically wrong. Depend upon it that the true method is to get rid of such matters at once, and in the simplest possible way, and that is the cheapest plan in the end. Show me a town where refuse matters are kept—no matter how they are treated—and I will show you a town where the standard of vitality is low; I will show you a town with a high death-rate, especially among children.

To take the other side of the question, look at London. There you have a population of 3¼ millions, with the lowest death-rate of any very large collected population in the world, with one of the lowest death-rates among the large towns of even our own country. Why is this? I say unhesitatingly, and without fear of contradiction, that with all allowances made for the excellent position of London, it is mainly due to the fact that the principle there, however incompletely it may be carried out, is the immediate removal of all refuse matters; in London, the water-carriage system, by which the foul water containing a very large proportion of the refuse matters of the population, is removed by gravitation in sewers, is carried out far more perfectly than in any other large town, and this system is daily being rendered more perfect there; it is the right system based upon a true principle, and its results are most salutary. When you have got rid of refuse matters, then see what you can do with them; and here arises a very curious consideration. Sewers, in most instances, were not originally built as sewers, but as drains; a sewer is a conduit for the removal of fouled water; a drain is a channel for the removal of mere superfluous water, the object being to dry the soil. The pattern of all our old sewers, the Cloaca Maxima at Rome, was originally a drain; it was constructed by Tarquinius Priscus, the fifth King of Rome, 600 years B.C., to drain the marshy ground between the Palatine and Capitoline hills, and it was so well constructed that it drains that ground at this moment. Pliny wondered that it had endured 700 years unaffected by earthquakes, by inundations of the Tiber, by masses which had rolled into its channel, and by the weight of the ruins which had fallen over it. What would he say could he see it now, as any of you may who choose to go to Rome, still discharging, after more than 2,400 years, its dirty water into the Tiber? But the convenience of the great drain for the disposal of refuse matters soon became apparent, and so it was turned into a sewer, and has been one ever since.

Well, what are we to do with the refuse sewer water, when we have got it out of our towns? This is one of the greatest questions of the day. Drains, of course, were naturally made to discharge into rivers, their proper place, so long as they were only drains; but when they come to be used as sewers, this will not do; in the first place the rivers are fouled, and in the next the manure is lost. I shall be able to show you in the course of the lecture that the only way known by which sewer water can be either purified or utilised, is by turning it, with suitable precautions, on to land, that this may be done, not only without injury to the health of the neighburhood, but with great benefit in many ways.

We have spoken of drains to dry the soil; what is the necessity of this? Every farmer knows that crops will not flourish on undrained land; neither can human beings; a damp house is a synonym for an unhealthy house, you all know that; but it is only within the last few years, as the result of a most important sanitary research, made by Dr. Buchanan, that we have come to know as a scientific fact, beyond all dispute, that the drying of the soil of a town reduces the number of deaths from consumption in a most extraordinary manner; in some towns the number of deaths under this head has been reduced by one-third or even by one-half, in this way.

To mention some other special diseases which have been successfully combatted of late years, look at scurvy, that terrible malady which formerly decimated our navies! We know now that that disease may be prevented by the use of limejuice as part of the daily food, and we are no longer afraid of it. (Some illustrations of the ravages of this disease were given.)

Look at small-pox, beyond all exception the most fearful epidemic disease with which the world was ever afflicted! We know how to prevent it, and we have recently had a very severe lesson from not applying that knowledge. It is to the immortal credit of England that Jenner, the discoverer of vaccination, was an Englishman; there are certain people, and they have actually formed a society, who are trying to get compulsory vaccination done away with in this country. Let me tell you that if there is one fact established in preventive medicine it is that vaccination affords a protection from small-pox; let me tell you that this statement is founded upon an induction such as has been brought to bear upon no other subject in medical science; and, let me add, that those persons who bring isolated facts as arguments against a statement so supported, show that they have no idea of the nature of an inductive argument at all. An unvaccinated person is a danger to the community, and ought not to be allowed to go at large, and so far from persons being merely fined for not allowing their children to be vaccinated, and then permitted to keep them unvaccinated, the children ought to be vaccinated by the public vaccinator, even in spite of their parents, who should not be allowed to risk their children's lives through their own obstinacy and ignorance; and not only their children's lives, but those of the persons around them. The recent epidemic of small-pox showed us several important things—it showed us what we knew before, that small-pox is far more fatal to unvaccinated than to vaccinated persons; it showed us that while small-pox is especially fatal to unvaccinated children, it is less fatal to vaccinated children than to other persons; thus demonstrating the necessity of re-vaccination, and it showed us that re-vaccination once performed is actually a better protection against small-pox than a previous attack of small-pox is. You know that it is not common for a person to have small-pox twice. Well, it is much less common for a person to have small-pox after he has been successfully re-vaccinated, and if he has it is almost certain to be a very mild attack. Out of nearly 15,000 cases of small-pox admitted into various London hospitals during the late epidemic, only four presented proof of having been re-vaccinated.

Let us pass on to typhoid fever. Here is a disease of the very existence of which, as distinct from certain other diseases, we have only known in recent times, but yet a disease about which, thanks to the researches of men now among us, one of whom it especially becomes me, as his pupil to mention, Sir William Jenner, we really seem to know more than about almost any other disease; a disease which we deliberately hunt down to its source, and stop just as we could stop the supply of stone from a quarry or of rifles from an armoury; a disease, the haunts and habits of which we know with such accuracy that we are able to go into a house and say, "Alter this, and alter that, or you will very likely get typhoid fever here," a disease the ways of which we know so well, that, when there has been a case of it caused by local defects in a house, we can almost predict what alterations are required without going to the place. Surely the results obtained from the study of this disease are some of the most striking results of sanitary progress in our day. I find that the idea has become widely spread that the recent epidemic of typhoid fever in London was due to the distribution of milk from a sewage farm; this was not so, and I regard it most in the light of a special providence that none of the milk sent out from that establishment came from a sewage farm: had it been so, such a fact, combined with the prejudice and ignorance which exists upon the matter, would have dealt a severe blow to the progress of one of the greatest sanitary improvements of the day. The cause of that epidemic is known with absolute certainty, the very channel by which the poison got into the dairy well having been recently unearthed.

I must allude, for an instant, to the recent sanitary legislation; it has been found fault with by many on account of matters of detail; but consider the fact that the result of it is that the country has spent a large sum of money in the employment of medical officers of health and sanitary inspectors, and that such men now exist, and you will see that in it we may find great cause for rejoicing when looking to the future of sanitary progress. In a lecture on the "History of Hygiene," which I delivered some three or four years ago at University College, London, I said, "From its very nature, hygiene interests all classes of society; but it is to those who are worst off—the poorest and most wretched—that it must direct its first attention. Civilisation has its evils as well as its advantages, as Bouchardat has well remarked; and one of the greatest of them is the over-crowding of people in the great centres of population, with the misery and disease which are the results of it. It is to better constructed houses for the working classes, to a free supply of good water, and to satisfactory sewerage arrangements, that we must look for an amelioration in these respects; and I would hasten to add, to a wider spread among those classes of such an education as shall lead them to appreciate the means used for the improvement of their condition, and to lend a helping hand for the furtherance of those means."

I feel that I cannot do better in conclusion than congratulate this town on having, through the munificence of one of its citizens, been the first to appreciate the importance of the education of the people in these subjects, and on having such an institution as this in which so much useful knowledge is imparted to the people, and congratulate myself on having the privilege of such an opportunity of spreading broadcast the great truths of sanitary science. The time is fast coming which was looked forward to by Dr. Parkes when he wrote:—"Let us hope that matters of such great moment may not always be considered as of less importance than the languages of extinct nations, or the unimportant facts of a dead history."

SCIENTIFIC SERIALS

THE current *Ibis* commences with the latter part of Mr. Brooke's notes on the ornithology of Sardinia, special attention being drawn to *Otis tetrax*, which is moderately common; *Phœnicopterus roseus*, which occurs in large flocks during the winter and even up to June; the presence of *P. erithacus* is doubtful. *Fulica nigrosa* was not seen, though included in both Cara's and Salvadori's lists. In the museum there are several specimens o *Phalacrocorax desmarestii*, and *P. carbo* is extremely common. *Larus audouini* is found, though very rarely.—Captain F. W. Hutton, in a note on *Rallus modestus* of New Zealand, gives evidence to show that Dr. Buller is in error when he considers *R. modestus* to be *R. dieffenbachii*, in an immature state of plumage, as the proportions of the chicks are different, and the bill of the latter more slender.—Messrs. Salvin and Elliot in continuation of their notes on the *Trochilidæ*, discuss the genus *Thalurania*, which is exclusively tropical, and consists of eleven species and five sections.—In notes on Chinese ornithology, Mr. R. Swinhoe draws special attention to *Ceryle rudis* at Ningpo, *Gallinago solitaria*, *Eudromias veredus*, and other land as well as water-birds found at Shanghai.—Mr. Sclater supplements Mr. Salvin's list of the birds of Nicaragua, with additions from a recent small collection made by Mr. Belt, adding seventeen species, mostly well known through Central America.—Mr. E. L. Layard gives notes of the birds observed in Para; and Mr. Sclater describes and figures two new species named by him *Picolaptes layardi*, and *Thamnophilus simplex*.—Captain J. H. Lloyd on the birds in the province of Kattiawar in West India, commences the detailed account with an interesting comment on the general ornithological description of the region.

THE *Monthly Microscopical Journal* for October, commences with a description, by Mr. F. H. Welch, of the thread-worm *Filaria immitis*, occasionally infesting the vascular system of the dog, with remarks on the same, relative to Haematozoa in general, and the Filaria in the human blood. The specimens described were obtained from the right ventricle and pulmonary artery of a dog, from Shanghai, the male, female, and young being described. The left ventricle also contained some of the young.—Dr. Royston-Pigott fully illustrates a paper entitled "Researches in Solar Spectra, applied to test residuary aberration in microscopes and telescopes; and the construction of a compensating eye-piece, being a sequel to the paper on a searcher for aplanatic images."—Dr. Rutherford describes a new freezing microtome in which the freezing box and escape tube are much larger than in his older instrument, and the indicator is improved.—Mr. Ch. Stodder, in a letter, points out that it is inaccurate to suppose that the nominal price of American objectives is directly comparable with that of English makers, as the value of money in the two countries is so different, and duty has to be paid on entering the former.

Annali di Chimica applicata alla Medicina, July number, 1873.—We notice in this journal, besides a number of formulæ for pharmaceutical preparations and other details interesting to the druggist, a paper by M. Gubler, on experiments with new and old opium alkaloids, which deals, amongst others, with apomorphia.—There is also a translation of Mr. Simon's memorandum on the diffusion of cholera, and other papers from native and foreign sources. In the *Rendiconto delle sessioni dell' Accademia delle scienze dell' Istituto di Bologna*, 1872–1873," are given briefly (in about 189 pages) abstracts of the papers read before the Society, together with other matter of the usual nature.

Reale Istituto Lombardo di scienze e Lettere Rendiconti, Fascicolo xiii.," July 1873.—This number contains several critical literary, historical, and philosophical papers, including one on Kant's philosophy, by C. Cantoni.—In the scientific section there is a paper by Prof. Cavalleri on improvements in the helioscope, and a portion of a paper by P. Cantoni on electrical adherence, which is illustrated with several tables of data.—Fascicolo xiv. contains a paper on the capacity of the nasal fossa, by P. Mantegazza, and one on cholera by G. Strambio.—C. Lombroso details some experiments on the tonic action of maize (*guasto*) affected with the *Penicillium glaucum*. The author maintains that the maize in this state acts injuriously. G. Sangalli, who replies to the paper, maintains that the effects are due to another cause.—New comet discovered at the Royal Observatory of Milan, by G. Tempel; communicated by G. V. Schiaparelli.—The continuation of P. Cantoni's paper on electrical adherence is given.—The other papers are on the propagation of the corpuscle cornalia, by C. Gibell, and a letter on a purulent disease of one hemisphere of the brain, by L. Porta.

SOCIETIES AND ACADEMIES

PHILADELPHIA

Academy of Natural Sciences, June 3.—Dr. Ruschenberger in the chair.—"Fertilisation of *Pedicularis canadensis*." Mr. Thomas Meehan drew attention to the structure of the flower of *Pedicularis canadensis*, in which it was evident self-impregnation was impossible, and there seemed to be no special arrangements for fertilisation by distinct agency, as there were in so many allied plants. In this case the stamens were included in the closely compressed arch of the corolla, and, with the anthers, were directed retrorsely to the pistil, which at an early stage, and long before the maturity of the pollen, was protruded beyond the corolla, rendering self-fertilisation almost impossible in this flower. But the flowers were always abundantly fertile, and though the arrangements were such as seemingly to afford no chance even for insects to aid in the fertilisation, it was also probable that in some way it was accomplished by them. Both last season and this he had devoted some time to watching the plant, but failed to find any clue to the process. A species of *Bombus* seemed to have the plant especially under its charge, visiting the flowers in great numbers; but they bored through the corolla on the outside of the tube for the saccharine matter, and the anthers or pollen did not seem to be in the least disturbed by this. Still it was so highly probable that in some way some insect aided in the cross-fertilisation of these flowers, that it might serve a useful purpose to direct attention to it, as others with time and opportunity might discover what he had failed to find.

RIGA

Society of Naturalists, April 16—M. Tank communicated some observations on honeydew, which he thinks is an immediate excretion of the leaves due to cooling.—M. Behrmann gave reasons for doubting the supposition that certain fires which occurred almost daily from October to December last year, in a village of the Orel Government, arose from phosphuretted hydrogen out of the marshy ground.

April 23.—M. Petzholdt read a paper on the composition and formation of Imatra stones. Various hypotheses of formation have been given—the gyratory, the stalactitic, the geological, the vegetable, the animal, &c. Parrot supposed the stones to be petrified, shell-less molluscs. M. Petzholdt formulates his view thus:—In a slimy layer of fine sand, mud, and carbonate of lime, are formed, through mutual attraction of particles of the latter, several ball-heaps of lime. Next, dry deposition of the whole at a later epoch. Disturbance of the stratum by water, setting free the hard spherical masses (Imatra stones).

April 30.—M. Pfeiffer showed a small headless chick with large legs, found dead with another, which was alive in the same egg. The two were connected by a fibre. After separation the living chick throve normally.

May 21.—M. Glasenapp gave a note on blackened wood in certain trees blown down in a storm. The blackening is attributed to a kind of fungus which formed on the north side of the trees while yet standing.—M. Gottfriedt read a paper on enclosure of diamonds in xanthophyllite; the supposed diamonds he finds to be merely hollow spaces, erosion figures.—M. Teich gave an account of an excursion to North-West of Kurland.—The *Correspondenz Blatt*, No. 9, contains a description of the snakes of the Baltic Provinces, of which there are three species—*Vipera berus*, *Tropidonotus natrix*, and *Coronella laevis*.

GOTTINGEN

Royal Academy of Sciences, Aug. 6.—Dr. Paul du Bois-Reymond communicated a paper on the representation of functions by Fourier's series.

Aug. 13.—M. Waitz compared some points in the Annales Sithienses, relative to Pippin and Charlemagne, with other annals of the time.—M. Ewald gave a paper on the passage, Ezek. xlv. 12: "Twenty shekels, five-and-twenty shekels, ten-and-five shekels shall be your maneh." The maneh, it is known, originally contained 60 shekels (which these numbers make up), and this enumeration, he thinks, was in order to exactness and certainty, not because there were coins of these several values. The Septuagint version (rightly read) makes the maneh 50 shekels, and it is known there was such a maneh. The author advances a theory, on which the passage affords evidence of both manehs having been known in the first half of the sixth century B.C.—Dr. Voss communicated a note on the geometry of focal surfaces of congruences.

Aug. 20.—M. Minnigerode gave a long paper on a new method of solving Pell's Equation $t^2 D\mu = 1$.

PARIS

Academy of Sciences, October 6.—M. Bertrand in the chair.—The following papers were read:—Note on the means used to obtain a constant temperature in rooms and on the methods of moderating it during the heat of summer, by General Morin.—On new propyl compounds, by M. A. Cahours. The author described several ethers of the propyl series.—Certain considerations on the yellow elastic tissue and its immediate organic analysis, by M. Chevreul.—Treatment of carbuncle and malignant pustule by carbolic acid and ammonic carbolate, by M. Déclat.—Statistical tables of the losses of German armies in France during the war of 1870–1, by Capt. D. H. Leclerc.—The subcutaneous infarctus of cholera, by M. Bouchut.—On the improvement in healthfulness caused by the growth of *Eucalyptus globulus* in marshes, by M. Gimbert.—Studies on the *Phylloxera*, by M. Max Cornu.—On the action on the vine of the carbonic disulphide used to destroy the *Phylloxera*, by M. Lecocq de Boisbaudran.—On the size and variations of the sun's diameter, by S. Respighi. The author in his letter criticised Secchi's statements as to the difference between the nautical almanac diameter and his own observations by monochromatic light. He regarded Secchi's observations as erroneous.—On the theory of the thrust of earthworks, by M. J. Curie.—On the condensation of gases and liquids by carbon, by M. Melsens. The author noticed the thermal phenomena produced by the contact of the liquids with carbon, &c.—On the production of certain borates in the dry way, by M. Ditte.—Researches on tribromacetic acid, by M. H. Gal.—On the development of *Batrachians*. This was a note on the embryos of *Hylodes martinensis*, by M. Bavay.

PAMPHLETS RECEIVED

ENGLISH.—Synopsis of all the Mosses known to inhabit Ireland: David Moore, Ph.D.—Lobley's Geologist's Excursion to the Malvern District.—Proceedings of the Belfast Natural History Society for 1871–2.—Layton Astronomical Observations.—Report, Chester Society of Natural Science.—Law of Elliptic Motion deduced from the Laws of Gravitation and Compound Rotation: G. Hamilton.—Milk, Typhoid Fever and Sewage: Alfred Smee.—Contributions to the Knowledge of the Meteorology of the Antarctic Regions.—A new Method of obtaining the Differentials of Functions; Profs. Rice and Johnson.—Count Rumford, How he Banished Beggary from Bavaria: T. L. Nichols, M.D.—A Scamper across Europe: T. L. Nichols, M.D.

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THURSDAY, OCTOBER 23, 1873

LIST OF SCIENTIFIC SOCIETIES AND FIELD CLUBS

IN GREAT BRITAIN AND IRELAND.

The following list has been compiled mainly from information recently sent us by the Secretaries of the various Societies named. For obvious reasons the Chartered London Societies have been omitted, and in the meantime we have omitted the scientific societies connected with the public schools, a list of which we hope to be able to give in an early number. Corrections of the following list and any additional information are requested by the Editor. The letter (E) denotes that the number of members has been taken from the list appended to Sir Walter Elliot's Address to the Edinburgh Botanical Society (November 1872); *a* denotes that the Society is also a Field Club; and *b* that it issues regular or occasional publications.

County and Title of Society.	When founded.	No. of members.
ENGLAND AND WALES		
Berkshire		
Reading Microscopical Society	1860	72
*ab*Newbury District Field Club	1870	98 (E)
Buckinghamshire		
High Wycombe Nat. Hist. Soc.	1865	70 (E)
Cambridgeshire		
*b*Cambridge Philosophical Society	1819	557 (E)
a ,, Field Naturalists' Club and Entomological Society	1852	40 (E)
Cambridge Natural Science Club	1872	12
Cheshire (*See* Lancashire)		
*a*Chester Society of Nat. Sci.	1871	454
Cornwall		
Cornwall Royal Geological Society (Penzance)	1814	163 (E)
*ab*Royal Institution of Cornwall (Truro)	1818	214 (E)
*b*Cornwall Royal Polytechnic Soc. (Falmouth)	1833	400
*b*Penzance Nat. Hist. and Antiquarian Soc.	1839	60 (E)
Cumberland		
*a*Keswick Literary Society	1869	70
Devonshire		
*ab*Plymouth Institution and Devon and Cornwall Statistical Society	1812	153 (E)
Torquay Natural History Society	1844	104
*ab*Teign Naturalists' Field Club	1858	123
Association for Advancement of Science, Literature, and Art	1862	174 (E)
*ab*Exeter Naturalists' Club and Archæol. Assoc.	1862	165 (E)
Dorsetshire		
*b*Purbeck Society	1855	30 (?)
Durham (*See* Newcastle)		
*a*Seaham Nat. Hist. Club	1861	50 (E)
Glamorganshire		
*b*Royal Institution of S. Wales (Swansea)	1835	255 (E)
*ab*Cardiff Naturalists' Society	1867	289
Gloucestershire		
Bristol Microscopical Society	1843	33 (E)
*ab*Cotteswold Naturalists' Field Club (Stroud)	1846	100 (E)
*ab*Bristol Naturalists' Society	1862	204
*ab*Cheltenham Naturalists' Association	1867	32 (E)
Hampshire		
*b*Isle of Wight Philosophical and Scientific Soc.	1850	103
*ab*Winchester and Hampshire Scien. & Lit. Soc	1869	125
Bournemouth Nat. Hist. & Antiquarian Soc.	1870	
S. of England Lit. & Phil. Soc. (Southampton)		115 (E)
Herefordshire		
*ab*Woolhope Naturalists' Field Club	1851	174 (E)
Kent		
*ab*East Kent Natural History Soc. (Canterbury)	1859	109
*ab*Folkestone Natural History Society	1868	150
*ab*Maidstone and Mid-Kent Natural History and Philosophical Society	1869	80
*ab*West Kent Natural History, Microscopical and Photographic Society		135
London, Counties included in		
Geological Association	1858	297
*ab*Quekett Microscopical Club	1865	570
Old Change Microscopical Society	1865	
*ab*Croydon Microscopical Club	1870	135
South London Micros. and Nat. Hist. Club	1870	200
,, Entomological Society	1872	
*a*New Cross Micros. and Nat. Hist. Soc.	1872	
Sydenham and Forest Hill Micros. Club		
Bethnal Green Clubs		
Lancashire		
*b*Manchester Literary and Philosophical Society (including Microscopical section)	1781	222
*b*Liverpool Literary and Philosophical Society	1812	198
*b*Lancashire and Cheshire Historical Society	1831	257 (E
*ab*Liverpool Naturalists' Field Club	1860	500
*ab*Manchester Field Naturalists' Society	1860	180
a ,, Scientific Students' Association	1860	140
a ,, Lower Mosely Street School Nat. Hist. Soc. about	1863	
*a*Lunesdale Naturalists' Field Club (Lancaster)	1868	80 (E)
,, Entomological Society		
ab ,, Geological Society		
Warrington Literary and Philosophical Society		96
Leicestershire		
*b*Leicester Literary and Philosophical Society	1835	238 (E)
Norfolk		
*a*Norwich Geological Society	1864	
*ab*Norfolk and Norwich Naturalists' Society	1869	118
,, Microscopical Society		
Northamptonshire		
*a*Northamptonshire Field Club	1866	50 (E)
Northumberlandshire		
Newcastle-on-Tyne Lit. and Phil. Soc.	1793	1500
b ,, ,, Antiquarian Soc.	1813	
,, ,, Entomological Soc.	1870	35 (E)
*b*Northumberland, Durham, and Newcastle Nat. Hist. Soc. (Newcastle-on-Tyne)	1829	130
*ab*Tyneside Naturalists' Field Club	1846	600
Nottinghamshire		
*a*Nottingham Naturalists' Society	1852	45
,, Literary and Philosophical Soc.	1864	271
Oxfordshire		
Ashmolean Society (Oxford)	1828	
Shropshire		
*b*Ludlow Natural History Society	1833	70 (E)
*b*Shropshire and N. Wales Natural History and Antiquarian Society	1835	86 (E)

County and Title of Society.	When founded.	No. of members.
*ab*Oswestry and Welshpool Naturalists' Field Club and Archæological Society	1857	45
*a*Severn Valley Naturalists' Field Club (Bridgenorth)	1863	243 (E
*a*Caradoc Field Club (Shrewsbury)	1863	71 (E
a ,, Field Club	1868	60 (E
Somersetshire		
Royal Literary and Scientific Institution (Bath)	1823	215
*b*Somersetshire Archæological & Nat. Hist. Soc.	1849	365 (E)
*ab*Bath Nat. Hist. and Antiquarian Field Club	1855	94
Staffordshire		
*ab*North Staffordshire Naturalists' Field Club	1865	280
*a*Tamworth Natural History, Geological, and Antiquarian Society	1871	100
Suffolk		
*ab*Suffolk Institute of Archæology and Nat. Hist. (Bury St. Edmunds)	1848	147
Surrey (*See* London)		
*ab*Holmesdale Nat. Hist. Club (Reigate)	1857	105 (E)
Sussex		
*ab*Brighton and Sussex Nat. Hist. Soc.	1854	180
*ab*Lewes and East Surrey Nat. Hist. Soc.	1864	105
*b*Eastbourne Nat. Hist. Soc.	1868	95
Warwickshire		
*b*Warwickshire Nat. Hist. and Archæological Soc.	1836	97 (E)
ab ,, Naturalists' and Archæologists' Field Club	1854	50 (E)
Birmingham Nat. Hist. and Microscopical Soc.	1858	
Leamington Philosophical Society	1866	
Wiltshire		
Wiltshire Archæological and Nat. Hist. Soc.	1853	320
Worcestershire		
*b*Worcestershire Nat. Hist. Soc.	1833	200 (E)
a ,, Naturalists' Field Club	1846	120
*ab*Malvern Field Club	1853	60
*ab*Dudley and Midland Geological and Scientific Society and Field Club	1862	172 (E)
Yorkshire		
*b*Leeds Philosophical and Literary Society	1820	650
*b*Hull Literary and Philosophical Society	1821	371
Sheffield Literary and Philosophical Society	1822	241 (E)
Whitby Literary and Philosophical Society	1822	58
*b*Yorkshire Philosophical Society	1822	459 (E)
*b*Scarborough Philosophical Society	1829	66 (E)
*b*W. Riding Geological and Polytechnic Society	1838	
*ab*Yorkshire Naturalists' Club	1849	98 (E)
*a*Halifax Naturalists' Society	1857	41 (E)
*a*Leeds Natural History Society	1862	61 (E)
*ab*West Riding Consolidated Naturalists' Society including:—	1862	
*a*Huddersfield Naturalists' Society	1847	125
*a*Heckmondwike ,, ,,	1861	36
*a*West Clayton ,, ,,	1862	24 (E)
*a*Ovenden ,, ,,	1865	36 (E)
*a*Barnsley ,, ,,	1867	47
*a*Stainland ,, ,,	1868	
*a*Rippenden ,, ,,	1871	
*a*Holmforth ,, ,,	1871	
*a*Wakefield ,, ,,	1871	
*a*Liversedge ,, ,,	1872	
*a*Rostrick ,, ,,	1873	
*a*Birkby ,, ,,	1873	
*b*Ripon Scientific Society	1862	50 (E)
*a*Richmond and N. Riding Naturalists' Field Club	1863	147
*a*Norland Naturalists' Society	1863	17 (E)
*ab*Cleveland Literary and Philosophical Society	1863	160 (E)
*b*Bradford Philosophical Society	1865	
*a*Bolton Scientific Students' Society	1865	
*a*Leeds Naturalists' Field Club and Scientific Association	1870	66
Bolton Literary and Scientific Society	1871	100
*a*The Denny Club (Leeds)		
*a*Morley Naturalists' Society		13 (E)
SCOTLAND		
Aberdeenshire		
Aberdeen Philosophical Society	1840	83 (E)
ab ,, Nat. Hist. Soc.	1863	80 (E)
Berwickshire		
*ab*Berwickshire Naturalists' Club	1831	249 (E)
Clackmannanshire		
*ab*Alloa Nat. Science and Archæology Soc.	1862	110
Dumfriesshire		
*ab*Dumfriesshire Nat. Hist. and Antiquarian Soc.	1862	100 (E)
Fifeshire		
*a*Largo Field Naturalists' Soc.	1863	60
Forfarshire		
*b*Montrose Nat. Hist. and Antiquarian Society	1836	190 (E)
*a*Dundee Naturalists' Field Club	1869	12
Inverness-shire		
Inverness Literary Institute		60
Lanarkshire		
*b*Glasgow Philosophical Society	1802	570
ab ,, Nat. Hist. Society	1851	221
ab ,, Geological Society	1858	202
Morayshire		
Elgin and Morayshire Lit. and Scien. Assoc.	1836	100
Nairn		
Nairn Naturalists' Club		
Orkney		
Orkney Nat. Hist. Soc.	1837	17
Perthshire		
Perth Literary and Antiquarian Society	1784	
*ab*Perthshire Society of Natural Science	1867	150
Renfrewshire		
Paisley Field Club (recently formed)		
Roxburgh		
Tweedside Physical and Antiquarian Society	1834	60 (E)
*b*Hawick Archæological Society	1856	158
Selkirk		
Galashiels Society (recently formed)		
IRELAND		
Antrim		
*b*Belfast Nat. Hist. and Philosophical Society	1821	
ac ,, Naturalists' Field Club	1863	242
Cork		
Royal Institution (Cork)	1807	
Cork Literary and Scientific Society	1819	
Cuvierian and Archæological Society (Cork)	1835	
Londonderry		
*ab*Derry Nat. Hist. and Philosophical Society	1870	80

LOCAL SCIENTIFIC SOCIETIES

I.

WE have devoted part of our space this week to a kind of Census of our Local Scientific Societies. It will be seen that in these Islands we already muster a goodly number, but no friend of Science would consider the number satisfactory; it does not, we are sure—seeing that there are twenty counties in England and Wales, and a much larger proportion in Scotland and Ireland, which appear not to boast of any such society—represent the true activity of the different regions from which, so to speak, the societies are fed. We do not suppose that our list is accurate; indeed our present purpose in printing it is to gather information. We hope that many societies exist which are not in our list; we fear that some have already ceased to exist since the time that Sir Walter Elliot, with infinite pains, compiled some of the data on which we have had to rely in the absence of information forwarded by the officers of the societies themselves.

On the whole, however, all lovers of Science and advocates for the spread of scientific education among all classes, ought to feel greatly gratified at the rapid increase during recent years, of local scientific societies and field-clubs indicated by the dates of foundation to be found in our list. No more unmistakeable sign of a general elevation of taste, of the spread of the scientific influence and of a desire for scientific knowledge, can, we think, be obtained, than this starting-up, in all parts of the country, of societies for the express purpose of scientific work in one form or another, and that generally as a means of recreation. By far the greater number of the societies have had their birth within recent years. With one or two exceptions, the older societies are not very prominently scientific, while as a rule the recently founded ones bear on their very front the declaration that they have been established solely for the pursuit of Science.

This is indeed very encouraging, more especially when we reflect that this result is no outcome of any temporary burst of enthusiasm, of any exciting scientific "revival" agitation, but is simply the natural fruit of the slow but sure development of the scientific spirit in our country.

From the information which has been kindly sent us by the secretaries of the various societies many interesting facts might be presented, and many curious and valuable inferences drawn. It will be seen from the list, that the societies are very unequally distributed over the country, quite a busy hive of them being clustered around the border counties of England and Scotland, while not a few counties in both countries, as well as in Ireland, are quite unrepresented, and many large counties by but a single society. Why should this be? Is it to be attributed to the backward state of intelligence and education in the unrepresented districts? We do not think so; we believe that in every county in the three kingdoms, men and women will be found with an intelligent love of Science, a desire for scientific knowledge, and a wish for the spread of scientific education. Such people only require to be roused to perceive the advantages of the establishment of scientific societies and field-clubs in their midst; if only some one would take the initiative and start such societies where they do not at present exist, we have no fear, if judicious means be used, that ample success will follow. From the large number of members belonging to many of the societies, members belonging to all classes of society, it will be seen that it is now considered honourable to be connected with such an association; and although in most societies there is only a small nucleus of working members, still while efforts should be made to engage all in the work, the non-working majority should be considered as, at least by their subscriptions and good-will, they help on the good cause.

Into these and other details we hope to enter in one or more future articles, founded partly on the statistics we possess. At the present time, when a Committee of the British Association is considering the whole question of our local Societies, we think it useful to point out the extreme importance of an increased activity in this direction. The recent action of the Government in aiding the establishment of Science Schools has enormously increased the advantages which such local associations may confer on outsiders, while at the same time it has greatly widened the recruiting ground. And it is in this double capacity that the formation, encouragement, and extension of such societies should be the care of all, whether scientific in their tastes or not; while, to friends of Science it is crucial, for Government aid, under existing arrangements, can only come where there are Science Classes; and without Government aid, in nine cases out of ten, the thing will fall to the ground altogether, or drag on an existence of second-rate utility.

If there then be any Scientific Societies without Science Classes attached to them, let them be assured that their museums are comparatively valueless; and further, that their museum must always remain as it is, for though it is clear to many that the Government must soon supply typical collections to museums which are available for teaching purposes, it is equally clear that there is no reason why they should do so to museums the utility of which is limited merely to members of a society.

Again: If there be any Scientific Societies without Science Classes attached to them let them be assured that their courses of lectures will prove of the least possible value; for mere lectures to those anxious to learn, but who are debarred from more serious study, are more than disappointing, they are hurtful.

In the ordinary course of things the Lecture should be the precursor of the Science Class. The Science Class should drive the student to the Museum, and from the zealous students the society should be recruited.

There is one point in which all will acknowledge our local societies have of late made considerable progress, and here again the British Association has been helpful to them—we refer to the more general establishment of courses of lectures, and the more general engagement of competent men of science, to place things new and strange before their members. Let not such lecturers forget that their duty is almost a sacred one; though he may not be a Davy, there may yet be a Faraday among the audience, one who may be gained or lost to Science according as the lecturer does his allotted work well or ill.

This brings us to another point. Why should not physical and chemical apparatus available for high-class experimental lectures be occasionally seen in our museums or in rooms adjoining them? Why should the stuffed crocodile and curious weapon of some southern race of savages have it all their own way to the extent that they do? Here, no doubt, our Government has been greatly at fault, for after all, humble local museums, *parvis componere magna*, are little British Museums, and there is no help provided by the government for any physical, or chemical, or astronomical students in the British Museum. But though our government is behind the age in London, the South Kensington authorities are alive to the weak point in the armour, as regards the provinces, and if a local society will only establish a Science Class, travelling collections of the most important modern scientific instruments are to be had for the asking; and we may hope that ere long there may be a model museum at South Kensington, doing for physical science what is done for it in Paris by the magnificent *Conservatoire des Arts et Métiers*, a museum in which the applications of Science, and the implements for the teaching of Science hold the first place.

FARADAY ON SCIENTIFIC LECTURING

At a time when the lecture season is commencing, we believe we shall be doing good service by placing before those of our readers who are not already acquainted with them in Dr. Bence Jones' "Life of Faraday," the opinions of that great man on many points connected with lectures on Science.

They were written to a friend when Faraday was but 21 years of age, but we believe he would have changed little though he might have added much if he had revised them in his later years. He commences by explaining that:—

"The subject upon which I shall dwell more particularly at present has been in my head for some considerable time, and it now bursts forth in all its confusion. The opportunities that I have latterly had of attending and obtaining instructions from various lecturers in their performance of the duty attached to that office, has enabled me to observe the various habits, peculiarities, excellencies, and defects of each of them as they were evident to me during the delivery. I did not wholly let this part of the things occurrent escape my notice, but when I found myself pleased, endeavoured to ascertain the particular circumstance that had affected me; also, whilst attending Mr. Brand and Mr. Powell in their lectures, I observed how the audience were affected, and by what their pleasure and their censure were drawn forth.

"On going to a lecture I generally get there before it begins; indeed, I consider it as an impropriety of no small magnitude to disturb the attention of an audience by entering amongst them in the midst of a lecture, and, indeed, bordering on an insult to the lecturer. By arriving there before the commencement, I have avoided this error, and have had time to observe the lecture-room."

He dwells on the form of the lecture-room, and then indicates how important a matter ventilation is.

"There is another circumstance to be considered with respect to a lecture-room of as much importance almost as light itself, and that is ventilation. How often have I felt oppression in the highest degree when surrounded by a number of other persons, and confined in one portion of air! How have I wished the lecture finished, the lights extinguished, and myself away merely to obtain a fresh supply of that element! The want of it caused the want of attention, of pleasure, and even of comfort, and not to be regained without its previous admission. Attention to this is more particularly necessary in a lecture-room intended for night delivery, as the lights burning add considerably to the oppression produced on the body."

He then goes on:—

"Having thus thrown off, in a cursory manner, such thoughts as spontaneously entered my mind on this part of the subject, it appears proper next to consider the subject fit for the purposes of a lecture. Science is undeniably the most eminent in its fitness for this purpose. There is no part of it that may not be treated of, illustrated, and explained with profit and pleasure to the hearers in this manner. The facility, too, with which it allows of manual and experimental illustration, places it foremost in this class of subjects. After it come (as I conceive) arts and manufactures, the polite arts, belles lettres, and a list which may be extended until it includes almost every thought and idea in the mind of man, politics excepted. I was going to add religion to the exception, but remembered that it is explained and laid forth in the most popular and eminent manner in this way. The fitness of subjects, however, is connected in an inseparable manner with the kind of audience that is to be present, since excellent lectures in themselves would appear absurd if delivered before an audience that did not understand them. Anatomy would not do for the generality of audiences at the R. I. (Royal Institution), neither would metaphysics engage the attention of a company of schoolboys. Let the subject fit the audience, or otherwise success may be despaired of."

Now for the lecturer:—

"A lecturer may consider his audience as being polite or vulgar (terms I wish you to understand according to Shuffleton's new dictionary), learned or unlearned (with respect to the subject), listeners or gazers. Polite company expect to be entertained not only by the subject of the lecture, but by the manner of the lecturer; they look for respect, for language consonant to their dignity, and ideas on a level with their own. The vulgar—that is to say in general, those who will take the trouble of thinking, and the bees of business—wish for something that they can comprehend. This may be deep and elaborate for the learned, but for those who are as yet tyros and unacquainted with the subject must be simple and plain. Lastly, listeners expect reason and sense, whilst gazers only require a succession of words.

"These considerations should all of them engage the attention of the lecturer whilst preparing for his occupation, each particular having an influence on his arrangements proportionate to the nature of the company he expects. He should consider them connectedly, so as to keep engaged completely during the whole of the lecture the attention of his audience.

"I need not point out to the active mind of my friend the astonishing disproportion, or rather difference, in the perceptive powers of the eye and the ear, and the facility and clearness with which the first of these organs conveys ideas to the mind—ideas which, being thus gained, are held far more retentively and firmly in the memory than when introduced by the ear. 'Tis true the ear here labours under a disadvantage, which is that the lecturer may not always be qualified to state a fact with the utmost precision and clearness that language allows him and that the ear cannot understand, and thus the complete action of the organ, or rather of its assigned portion of the sensorium, is not called forth; but this evidently points out to us the necessity of aiding it by using the eye also as a medium for the attainment of knowledge, and strikingly shows the necessity of apparatus.

"Apparatus, therefore, is an essential part of every lecture in which it can be introduced; but to apparatus should be added, at every convenient opportunity, illustrations that may not perhaps deserve the name of apparatus and of experiments, and yet may be introduced with considerable force and effect in proper places. Diagrams, and tables too, are necessary, or at least add in an eminent degree to the illustration and perfection of a lecture. When an experimental lecture is to be delivered, and apparatus is to be exhibited, some kind of order should be observed in the arrangement of them on the lecture table. Every particular part illustrative of the lecture should be in view, no one thing should hide another from the audience, nor should anything stand in the way of or obstruct the lecturer. They should be so placed, too, as to produce a kind of uniformity in appearance. No one part should appear naked and another crowded, unless some particular reason exists and makes it necessary to be so. At the same time, the whole should be so arranged as to keep one operation from interfering with another. If the lecture-table appears crowded, if the lecturer (hid by his apparatus) is invisible, if things appear crooked, or aside, or unequal, or if some are out of sight, and this without any particular reason, the lecturer is considered (and with reason too) as an awkward contriver and a bungler.

"The most prominent requisite to a lecturer, though perhaps not really the most important, is a good delivery; for though to all true philosophers science and nature will have charms innumerable in every dress, yet I am sorry to say that the generality of mankind cannot accompany us one short hour unless the path is strewed with flowers. In order, therefore, to gain the attention of an audience (and what can be more disagreeable to a lecturer than the want of it?), it is necessary to pay some attention to the manner of expression. The utterance should not be rapid and hurried, and consequently unintelligible, but slow and deliberate, conveying ideas with ease from the lecturer, and infusing them with clearness and readiness into the minds of the audience. A lecturer should endeavour by all means to obtain a facility of utterance, and the power of clothing his thoughts and ideas in language smooth and harmonious, and at the same time simple and easy. His periods should be round, not too long or unequal; they should be complete and expressive, conveying clearly the whole of the ideas intended to be conveyed. If they are long, or obscure, or incomplete, they give rise to a degree of labour in the minds of the hearers which quickly causes lassitude, indifference, and even disgust.

"With respect to the action of the lecturer, it is requisite that he should have some, though it does not here bear the importance that it does in other branches of oratory; for though I know of no species of delivery (divinity excepted) that requires less motion, yet I would by no means have a lecturer glued to the table or screwed on the floor. He must by all means appear as a body distinct and separate from the things around him, and must have some motion apart from that which they possess.

"A lecturer should appear easy and collected, undaunted and unconcerned, his thoughts about him, and his mind clear and free for the contemplation and description of his subject. His action should not be hasty and violent, but slow, easy, and natural, consisting principally in changes of the posture of the body, in order to avoid the air of stiffness or sameness that would otherwise be unavoidable. *His whole behaviour should evince respect for his audience, and he should in no case forget that he is in their presence.* No accident that does not interfere with their convenience should disturb his serenity, or cause variation in his behaviour; he should never, if possible, turn his back on them, but should give them full reason to believe that all his powers have been exerted for their pleasure and instruction.

"Some lecturers choose to express their thoughts extemporaneously immediately as they occur to the mind, whilst others previously arrange them, and draw them forth on paper. Those who are of the first description are certainly more unengaged, and more at liberty to attend to other points of delivery than their pages; but as every person on whom the duty falls is not equally competent for the prompt clothing and utterance of his matter, it becomes necessary that the second method should be resorted to. This mode, too, has its advantages, inasmuch as more time is allowed for the arrangement of the subject, and more attention can be paid to the neatness of expression.

"But although I allow a lecturer to write out his matter, I do not approve of his reading it; at least, not as he would a quotation or extract. He should deliver it in a ready and free manner, referring to his book merely as he would to copious notes, and not confining his tongue to the exact path there delineated, but digress as circumstances may demand or localities allow.

"A lecturer should exert his utmost effort to gain completely the mind and attention of his audience, and irresistibly to make them join in his ideas to the end of the subject. He should endeavour to raise their interest at the commencement of the lecture, and by a series of imperceptible gradations, unnoticed by the company, keep it alive as long as the subject demands it. No breaks or digressions foreign to the purpose should have a place in the circumstances of the evening; no opportunity should be allowed to the audience in which their minds could wander from the subject, or return to inattention and carelessness. A flame should be lighted at the commencement, and kept alive with unremitting splendour to the end. For this reason I very much disapprove of breaks in a lecture, and where they can by any means be avoided, they should on no account find place. If it is unavoidably necessary, to complete the arrangement of some experiment, or for other reasons, leave some experiments in a state of progression, or state some peculiar circumstance, to employ as much as possible the minds of the audience during the unoccupied space—but, if possible, avoid it.

"Digressions and wanderings produce more or less the bad effects of a complete break or delay in a lecture, and should therefore never be allowed except in very peculiar circumstances; they take the audience from the main subject, and you then have the labour of bringing them back again (if possible).

"For the same reason (namely that the audience should not grow tired), I disapprove of long lectures; one hour is long enough for anyone, nor should they be allowed to exceed that time.

"A lecturer falls deeply beneath the dignity of his character when he descends so low as to *angle for claps*, and *asks for commendation*. Yet have I seen a lecturer even at this point. I have heard him causelessly condemn his own powers. I have heard him dwell for a length of time on the extreme care and niceness that the experiment he will make requires. I have heard him hope for indulgence when no indulgence was wanted, and I have even heard him declare that the experiment now made cannot fail from its beauty, its correctness, and its application, to gain the approbation of all. Yet surely such an error in the character of a lecturer cannot require pointing out, even to those who resort to it; its impropriety must be evident, and I should perhaps have done well to pass it.

"Before, however, I quite leave this part of my subject, I would wish to notice a point in some manner connected with it. In lectures, and more particularly experimental ones, it will at times happen that accidents or other incommoding circumstances take place. On these occasions an apology is sometimes necessary but not always. I would wish apologies to be made as seldom as possible, and generally, only when the inconvenience extends to the company. I have several times seen the attention of by far the greater part of the audience called to an error by the apology that followed it.

"An experimental lecturer should attend very carefully to the choice he may make of experiments for the illustration of his subject. They should be important, as they respect the science they are applied to, yet clear, and such as may easily and generally be understood. They should rather approach to simplicity, and explain the established principles of the subject, than be elaborate and apply to minute phenomena only. I speak here (be it understood) of those lectures which are delivered before a mixed audience, and the nature of which will not admit of their being applied to the explanation of any but the principal parts of a science. If to a particular audience you dwell on a particular subject, still adhere to the same principle, though perhaps not exactly to the same rule. Let your experiments apply to the subject you elucidate, do not introduce those which are not to the point.

"Though this last part of my letter may appear superfluous, seeing that the principle is so evident to every capacity, yet I assure you, dear A., I have seen it broken through in the most violent manner—a mere alehouse trick has more than once been introduced in a lecture, delivered not far from Pall Mall, as an elucidation of the laws of motion.

"Neither should too much stress be laid upon what I would call small experiments, or rather illustrations. It pleases me well to observe a neat idea enter the head of a lecturer, the which he will immediately and aptly illustrate or explain by a few motions of his hand—a card, a lamp, a glass of water, or any other thing that may be by him; but when he calls your attention in a particular way to a decisive experiment that has entered his mind, clear and important in its application to the subject, and then lets fall a card, I turn with disgust from the lecturer and his experiments. 'Tis well, too, when the lecturer has the ready wit and the presence of mind to turn any casual circumstance to an illustration of his subject. Any particular circumstance that has become table-talk for the town, any local advantages or disadvantages, any trivial circumstance that may arise in company, give great force to illustrations aptly drawn from them, and please the audience highly, as they conceive they perfectly understand them.

"Apt experiments (to which I have before referred) ought to be explained by satisfactory theory, or otherwise we merely patch an old coat with new cloth, and the whole (hole) becomes worse. If a satisfactory theory can be given, it ought to be given. If we doubt a received opinion, let us not leave the doubt unnoticed, and affirm our own ideas, but state it clearly, and lay down also our objections. If the scientific world is divided in opinion, state both sides of the question, and let each one judge for himself, by noticing the most striking and forcible circumstances on each side. Then, and then only, shall we do justice to the subject, please the audience, and satisfy our honour, the honour of a philosopher."

We trust that during the ensuing session, these opinions of Faraday may be in the minds of every lecturer on Science.

ECKER'S "CONVOLUTIONS OF THEB RAIN"

On the Convolutions of the Human Brain. By Dr. Alexander Ecker, Professor of Anatomy and Comparative Anatomy in the University of Freiburg, Baden. Translated, by permission of the author, by John C. Galton, M.A., Oxon., M.R.C.S., F.L.S., &c., &c. Translator of Prof. Roser's "Manual of Surgical Anatomy," &c. (London: Smith, Elder, & Co., 1873.)

OF late years the topographical anatomy of the surface of the brain has deservedly attracted considerable attention; and the recent able investigations of Hughlings Jackson and Ferrier have shown the importance, in fact the absolute necessity of a correct and generally recognised description and enumeration of the cerebral convolutions. Mr. Galton therefore deserves the thanks of all interested in the subject, for having introduced to us in English dress this valuable monograph by Prof. Ecker of Freiburg.

There are two methods by which the complex human brain may be analysed and reduced to its simpler elements, two paths that lead to the same goal; the one is by a careful examination and comparison of the brains of the lower animals, and especially of apes, which latter in their higher groups present a "sketch map" as it were, which is filled in and completed in man only. This has been carried out with great success by Gratiolet primarily, and in England it has been followed amongst others by Huxley, Marshall, Flower and Rolleston. The other method is by tracing the development of the fœtal brain, and observing which fissures, and therefore which convolutions, are the first to make their appearance, and so are of primary importance, and how these subsequently undergo farther evolution and complication. Tiedemann and Reichert have hitherto been our authorities on this point, and it is by this method chiefly that Prof. Ecker arrives at his conclusions.

In this country the admirable little treatise of Prof. Turner has been welcomed and the classification therein adopted is now generally accepted, and taught in several of our anatomical schools. Prof. Ecker in the main follows Prof. Turner, although the nomenclature, of course, is that of the German school, and so differs occasionally from ours, which follows rather Gratiolet and the French school. The synonyms are, however, in all cases faithfully given.

The author insists upon the essential difference between the Sylvian fissure and the other sulci, these being mere indentations of the cortex, whilst that is formed by the folding of the temporo-sphenoidal lobe on the fore part of the brain during its development. The anterior or ascending branch of this fissure is here correctly described as being short and arrested by the hinder end of the lower frontal convolution, whilst that described as such by Prof. Turner is a distinct sulcus (præcentral) terminating close behind the ascending ramus. The gyrus connecting the inferior and ascending frontal (anterior central) convolutions is always present, although it is not always superficial, being occasionally concealed by the over-lapping of those convolutions. Instead of the orbital lobule usually described on the under surface of the frontal lobe, the three frontal convolutions are traced round the apex to the orbital surface. The narrow ridge internal to the olfactory sulcus (gyrus rectus) is regarded as the continuation of the first, the gyrus between that and the orbital sulcus as the second, and outside the last as the third. We should rather consider all internal to the orbital sulcus as first frontal, which is grooved by a special olfactory sulcus, and the second as ending posteriorly between the anterior branches of the triradiate orbital sulcus. The marginal convolution is regarded as simply the inner surface of the superior frontal.

In the parietal lobe the supra-marginal and angular convolutions are amongst the most difficult in the brain to indicate and circumscribe. Prof. Ecker describes the supra-marginal convolution as arching over the end of the fissure of Sylvius and joining the upper temporo-sphenoidal convolution, and the angular as folding over the hinder end of the parallel fissure and joining the middle temporo-sphenoidal convolution. This description, and it is supported by our experience, is not quite in accordance with that of some other anatomists; for instance, in Mr. Marshall's well-known essay on the brain of the bushwoman, the supra-marginal convolution is correctly defined thus, whilst the angular would require the anterior enlarged portion of the third annectent gyrus, as marked in the figure, to complete its bend and unite it to the second temporal gyrus. Similarly, in the idiot boy's brain, the angular gyrus would be a large folded convolution, there indicated as the bifurcated anterior extremity of the second annectent convolution; and in the idiot woman the parallel fissure extends so far back that it quite cuts off the angular gyrus from the temporo-sphenoidal, and the convolution is represented by the straight, also bifurcated fore part of the second annectent gyrus in the figure. The intra-parietal fissure of Turner is here called less correctly inter-parietal.

In the occipital lobe, a tolerably constant transverse depression, into which the intra-parietal fissure often debouches is appropriately named "occipital sulcus." Prof. Ecker regards the bridging, or annectent convolutions, as unworthily distinguished by special names in the human brain, since they do not bridge over any fissure as in the lower apes. He carefully points out their homology with those gyri in the ape, yet deprecates the transference of the names from the Simian to the human brain. But this comparison and correspondence of nomenclature is precisely what we require for the satisfactory determination of the cerebral functions, and the homological significance of a part is quite sufficient to justify the application of the same name to it. So also, on the inner surface, the lower annectent gyrus is described as the "gyrus cunei," and the occasional presence of the upper annectent gyrus is alluded to, of which we have now seen several examples. The operculum of the ape's brain is discussed, but the same term is unfortunately here applied to quite a different part of the human brain, viz. the united lower ends of the ascending frontal and parietal convolutions which overhang the island of Reil.

The middle convolution on the under surface of the occipito-temporal lobes is regarded, not without precedent, as the direct continuation of the gyrus fornicatus, and the uncinate gyrus of Huxley thus comes to be divided into three parts, the "lingual lobule" behind the union of the two gyri, the "convolution of the Hippocampus" immediately below the dentate fissure, and the recurved hook or "uncinate lobule"; but the connection between the gyrus fornicatus and this convolution is small and narrow, whilst that between it and the lingual lobule is large and direct; further, the author points out, after Gratiolet, that in many apes the calcarine is prolonged into the dentate fissure and cuts off the arched from the uncinate gyrus; surely this shows the essential unity of the uncinate convolution, and that the junction with the gyrus fornicatus is a superadded and secondary element in the human and certain Simian brains.

The translator has generally performed his work well; there are, however, one or two slips; for instance, the dentate fissure is said to produce an eminence in the floor of the *posterior* corner of the lateral ventricle; the parieto-occipital fissure also is described correctly as being concave forwards, whilst in the diagram it is represented as convex: the figures are exceedingly clear. Prefixed is an exhaustive bibliography by the translator, which adds materially to the value of the work; and finally, we can cordially recommend it as an accurate and lucid guide to a somewhat difficult study.

G. D. T.

OUR BOOK SHELF

The Zoological Record for 1871. Edited by Prof. Newton. (J. Van Voorst, 1873.)

THE birth of true biological science is of so recent origin, and its development has been so rapid that until lately many of the necessary steps in the furtherance of its proper progress have remained beyond the cognizance of its most enthusiastic followers. The difficulties connected with, and the unmanageableness of the large number of facts accumulated day by day on all branches of zoology, and recorded by observers in all parts of the civilised world, have until lately been scarcely realised. Only by those who, from the disappointment which they have experienced on finding that observations which have cost them incalculable time and labour, have been previously undertaken and exhausted by others before them, either in their own or some other country, appreciate fully the necessity for an easily accessible, accurate, and not over ponderous account of the labours of previous workers.

It is only the full appreciation of the advantage to future science students which stimulates the authors of the several parts of the work before us to continue and commence their contributions to this, what may be truly termed, labour of love. The labour involved in obtaining a complete and condensed account of the gist of each zoological paper published here or elsewhere throughout a year, is so great, and the smallness of the class who are disposed to purchase the work when produced, so necessarily restricted, that at first sight it is evident that it is only with the assistance of donations from scientific bodies, or from contributions of one kind or another on the part of amateurs in the subject, that the necessary expenses can be covered and the staff maintained.

These considerations will recommend this valuable work to the consideration of all interested in zoological progress.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

On the Equilibrium of Temperature of a Gaseous Column subjected to Gravity

SINCE reading Principal Guthrie's first letter on this subject (vol. viii. p. 67), I have thought of several ways of investigating the equilibrium of temperature in a gas acted on by gravity. One of these is to investigate the condition of the column as to density when the temperature is constant, and to show that when this is fulfilled the column also fulfils the condition that there shall be no upward or downward transmission of energy; or, in fact, of any other function of the masses and velocities of the molecules. But a far more direct and general method was suggested to me by the investigation of Dr. Ludwig Boltzmann* on the final distribution of energy in a finite system of elastic bodies. A sketch of this method as applied to the simpler case of a number of molecules so great that it may be treated as infinite, will be found on p. 535. Principal Guthrie's second letter (vol. viii. p. 486) is especially valuable as stating his case in the form of distinct propositions, every one of which, except the fifth, is incontrovertible. He has himself pointed out that it is here that we differ, and that this difference may ultimately be traced to a difference in our doctrine as to the distribution of velocity among the molecules in any given portion of the gas. He assumes, as Clausius, at least in his earlier investigations, did, that the velocities of all the molecules are equal, whereas I hold, as I first stated in the *Phil. Mag.* for Jan. 1860, that they are distributed according to the same law as errors of observation are distributed according to the received theory of such errors.

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* Studien über das gleichgewicht der lebendigen Kraft zwischen bewegten materiellen Punkten. Von Dr. Ludwig Boltzmann. Sitzb. d. Akad d. Wissensch, October 8, 1868 (Vienna).

It is easy to show that if the velocities are all equal at any instant they will become unequal as soon as encounters of any kind, whether collisions or "perihelion passages" take place. The demonstration of the actual law of distribution was given by me in an improved form in my paper on the Dynamical Theory of Gases, "Phil. Trans." 1866, and *Phil. Mag.* 1867, and the far more elaborate investigation of Boltzmann has led him to the same result. I am greatly indebted to Boltzmann for the method used in the latter part of the sketch of the general investigation (see p 535) which was communicated in a condensed form to the British Association on Sept. 20, 1873.

J. CLERK-MAXWELL

Mallet-Palmieri's Vesuvius

As I am assured that it would be most undesirable as well as unbecoming of me to continue a scientific controversy in the tone of Mr. Mallet's letter which appeared in NATURE of October 9, I would only beg those who have perused it to remember that my remarks were altogether directed to the assertions contained in Mr. Mallet's introductory sketch, and not comments upon his theory of volcanic energy of which, as he himself admits, we as yet know little or nothing. I would then ask them to compare its contents with the substance of my letter in NATURE, Sept. 4, and judge for themselves whether so far from its being any answer to my arguments, it does not, on the contrary, furnish additional "evidence of his confounding chemical constitution with percentage composition, &c.," the very keynote of this discussion.

Mr. Mallet writes—"Mr. Forbes appears to think that chemists, mineralogists, and geologists are the sole arbiters" of such questions; a remark he could not have made had he read some of my publications; yet I am quite willing to admit that I do place more faith in them collectively, than in any one physicist or mere mechanician whether theoretical or practical; and I believe I am correct in asserting that no theory of volcanoes will be accepted by the scientific world until its doctrines are proved to be fully in accordance with the facts brought forward by these sciences.

When the reasons for my delay in answering Mr. Mallet's criticisms were fully stated, is it not, to say the least, most unjust of him to harp on this string, after having already taken more than a month to produce a rejoinder the reverse of an answer, and the style of which, peculiar to himself, is in complete harmony with that of his introductory sketch, of which one of his favourable reviewers writes—"We do not cordially approve of his method of dealing with other writers. There is, if we may be excused the expression, a tone of bitterness all through his writing which gives the reader a most uncomfortable sensation, and leads a person altogether unbiassed to imagine a feeling of jealousy on the part of so distinguished a writer as Mr. Mallet which we are sure cannot exist in reality. After giving a sketch of the various authors who have ventured to give different and erroneous opinions on the subject of vulcanicity," &c. Another reviewer remarks that—"While objecting to most of the views of geologists, which, however, he frequently distorts, Mr. Mallet claims the character of physical truth for his own ideas," and adds, "what we chiefly object to in this portion of the volume is the assumption on Mr. Mallet's part of a conscious superiority to others, and a freely expressed contempt for all previous observers, especially for geologists." Need I add more?

DAVID FORBES

11, York Place, W. Oct. 20

Oxford Science Fellowships

As Mr. Perry's letter, in the last number of NATURE, contains assertions calculated to impede the progress of science here by deterring persons, not graduates of Oxford, from competing for appointments in colleges, and also involves charges of, to say the least, discourtesy to himself, I trust you will find space in your next number for the following explanation.

First, as to Mr. Perry's general assertion respecting fellowships.

From the fact that a graduate of Belfast is ineligible for a Fellowship in *Merton College*, Mr. Perry infers that "outsiders are ineligible for *Oxford* Fellowships in Physical Science." This is clearly illogical, and it is also untrue.

Secondly, as to the special case of Mr. Perry.

The ordinances of Merton College state that "no person shall be eligible" for a fellowship "who shall not have passed all the examinations required by the University for the degree of Bachelor of Arts." It appears a possible interpretation that Cambridge and Dublin B.A.'s, who can at any time incorporate in this University, may be candidates. If this be so, the reply of the Warden of Merton, as Mr. Perry gives it (of the actual correspondence I know nothing), may be correct, though perhaps not sufficiently explicit. This, however, is a legal question, and the college is taking steps to obtain the opinion of an eminent counsel.

Mr. Perry was not left, as his letter would naturally lead readers to infer, without warning as to this difficulty; for in July I wrote to Mr. Perry strongly expressing my doubt as to his eligibility, but as I was away from Oxford I could not quote the words of the ordinance; I advised him to consult the sub-warden, but I believe he did not follow my advice.

Mr. Perry received my letter, and replied to it on July 27.

The great difficulties which Mr. Perry asserts to have been thrown in his way, simply arose from the fact that he only proposed to come to Oxford during the vacation. Now it is not to be expected that I should allow any person who chooses to apply to overhaul the physical apparatus of the University in my absence, and it is unreasonable to suppose that, to suit the convenience of such a person, I must give up engagements made long before, in order to assist him in a candidature for an office of emolument in a college.

It must be borne in mind that there are nineteen colleges, any one of which may at any time offer a fellowship for proficiency in physics, and consequently to have to be at the service of outsiders, who may wish to be candidates, during the long vacation (the only time I have for real study) might become a serious matter, and to ask for such assistance seems to me to make a most unreasonable request.

I must add that if Mr. Perry imagines he would have been at any appreciable disadvantage by not knowing the particular instruments in the University cabinet (which it is by no means certain would be used for a college examination), either he assumes that the examiners would be guilty of the absurdity and unfairness of puzzling candidates by new or peculiar apparatus, or he feels very uncertain about his own practical knowledge.

A Cambridge B.A. is a candidate for the Merton Fellowship, and I have every reason to think that he found the Oxford candidates on exactly equal terms with himself in the practical examination.

H. B. CLIFTON

Oxford, Oct. 18

P.S.—Since writing the above I have been informed that a Cambridge graduate has been elected to a Science Fellowship in Magdalen College, Oxford. This is a proof of the inaccuracy of Mr. Perry's statement as to the ineligibility of outsiders for Oxford Fellowships.

Harmonic Echoes

I BELIEVE the echo observed by W. J. M. is of a different nature from mine and more analogous to one described by Oppel (Pogg. Ann. xciv. 357, 530). Each bar of the railing, when struck by the aerial pulse, diverts a small portion, which is scattered in all directions, much as if the bar were itself the source of sound. These derived pulses reach the ear of the observer at approximately equal intervals, and accordingly blend into a musical note, whose pitch, however, may not be quite constant. Oppel discusses the effect of different positions of the original source and the observer with respect to the grating, on which alone the pitch and its variations depend. It is evident that an echo formed in this way is in no sense *selective*.

I have been asked several times how the Bedgebury echo would be affected by the *character* of the original sound. Of course, if my theory is correct, the octave could not be returned, unless it were originally present; but the intensity of the echo was too feeble to give any promise of a successful observation with such an instrument as the clarinet. The experiment would be most interesting if a more powerful echo of the same class can be found.

RAYLEIGH

Terling Place, Witham, Oct.

Deep-sea Soundings and Deep-sea Thermometers

We feel sure you will not deny us space in your valuable periodical, when we tell you that, however unconsciously on your part, you, as well as other scientific authorities, are the means of doing us injustice and much professional injury, by the frequent allusions to the so-called Casella-Miller Thermometer, now used in deep-sea investigations. We are certain that we have only to call your attention to the real facts of the case for you to set the matter right before your readers.

1. We beg to state that in the year 1857 we invented, made, and supplied the Meteorological Department of the Board of Trade with upwards of fifty instruments of this description.

2. This thermometer we called the Double Bulb Deep Sea Thermometer, and a notice of it was published in the first number of the Meteorological Papers for the year 1857.

3. This thermometer, identical in every respect (except in its size), has been, after a lapse of some twelve years, *re-invented* and ushered before the scientific world with all the prestige of having a paper read upon it by the Vice-President of the Royal Society, Dr. Miller, who declared that he had just invented the instrument, in which task (of inventing an instrument well-known to all leading instrument makers, and Mr. Casella among the number) the learned doctor says he was assisted by Mr. Casella. (See Proceedings of the Royal Society, No. 113, page 482).

4. Annexed is an extract from Dr. Miller's paper describing the instrument, and by its side we give an extract from a treatise published by us in the year 1864, called "A Treatise on Meteorological Instruments."

Extract from "The Proceedings of the Royal Society," vol. xvii. page 483. Paper read June 3, 1869, by Dr. Miller.	Extract from "Negretti and Zambra's Treatise on Meteorological Instruments," published 1864, page 90:—
"The expedient adopted for protecting the thermometer from the effects of pressure consisted simply in enclosing the bulb of such a Six's thermometer in a second or outer glass tube, which was fused upon the stem of the instrument. "This outer glass tube was nearly filled with alcohol, leaving a little space to allow of variation in bulk due to expansion. "The spirit was heated to displace part of the air by means of its vapour, and the outer tube and its contents were sealed hermetically."	"The usual Six's thermometers have a central reservoir or cylinder containing alcohol. This reservoir, which is the only portion of the instrument likely to be affected by pressure, has been in Negretti and Zambra's new instrument superseded by a strong outer cylinder of glass containing mercury and rarefied air; by this means the portion of the instrument susceptible of compression has been so strengthened that no amount of pressure can possibly make the instrument vary."

5. We leave your readers to draw their own conclusions as to the similarity of the two instruments. Dr. Miller, when we called his attention to the fact of our prior claim, stated that he was not aware of the existence of our instrument, and we freely acquit Dr. Miller of conscious plagiarism, but we cannot omit to state, at the same time, that at the date at which Dr. Miller's paper was read, any scientific instrument maker worthy of the name was fully acquainted with our arrangement.

6. In order to prove what we thought of our instruments and as to their fitness for the purpose they were intended, when we were written to by the Meteorological Committee, three or four years ago, to produce a thermometer to be submitted to them for approval, we replied that we had already produced the only thermometer which in our opinion would answer the purpose, and that the thermometer was well known to them; we also said we were ready to make that instrument smaller, or larger, but that we could not possibly produce a better one.

Holborn Viaduct, E.C. HY NEGRETTI & ZAMBRA
October 14

Settle Caves Report

IN your abstract of the "Report of the Committee for exploring the Victoria Cave at Settle, by W. Boyd Dwakins, F.R.S." vol. viii. p. 476, are the following sentences. "The exact age of the Cave-earth is a matter of dispute. Mr. Tiddeman from the *physical evidence alone* regards it as preglacial, or rather as older than the great ice-sheet of that district."

Now it is true that in the spring of 1871, at a meeting of the Settle Caves Committee, I suggested the probability of the beds of lower Cave-earth in the Victoria Cave being of preglacial age from the physical evidence in the cave alone; but at a committee meeting at Settle soon after I laid much stress upon the impossibility of any animals, existing before the time of the Ice-sheet, having their remains preserved in the open country, although it was very likely that they might be found sealed up in sheltered caves. Acting on this idea the committee, notwithstanding some opposition, fortunately determined upon continuing their researches, and the result was the interesting discovery of the older mammals

May I be permitted to cite the following paragraph from the *Geological Magazine* of Jan. 1873, to show that I do not rely upon the physical evidence in the cave alone as determining the age of the lower cave-earth, although I confess that evidence, to my mind, is almost conclusive. "Perhaps one of the strongest pieces of evidence that the older cave mammals mentioned lived in this district only at a time previous to the great ice-sheet is, that so far as we know the remains of none of them (except of *Cervus elaphus*, which ranges from the Forest-bed to the present day) have been found in any of the Post-glacial deposits in this district. Though so common in the river-gravels in the Midland and Southern counties, they are never found except in caves until we get much farther south or east. Leeds, I believe, is the nearest locality where they occur. This would seem to imply that their remains were wiped off the area by the great ice-sheet which occupied what is now the Irish Sea and its tributary river-systems, and only left in the shelter of caves to which it could have no direct access. Brown bear, horse, red deer, reindeer, megaceros, the more modern Bovidæ, and other more recent forms are not uncommon in the Post-glacial beds; but the older cave mammals seem conspicuous only by their absence."

Clapham, Lancaster, Oct. 6 R. H. TIDDEMAN

Carbon Battery Plates

MR. T. W. FLETCHER will obtain what he requires from the India Rubber, Gutta Percha, and Telegraph Works Co., No. 100, Cannon Street, E.C.

I have 12,000 Carbons, or as we call them Graphite Plates, at work at this moment, and for some years past have obtained them solely from the above Company.

Tunbridge, Oct. 14 CHARLES V. WALKER

ASTRONOMICAL ALMANACS[*]

III.—*Foundation of the Nautical Almanac*

DURING his voyage of 1761 to the island of Saint Helena, for the purpose of observing the transit of Venus, Maskelyne, like La Caille, investigated the methods for determining longitudes at sea, and on his return, in "The British Mariner's Guide" (1763), proposed to adopt the plan of an almanac sketched by the French astronomer. There existed at this time in England a commission instituted by George III. for the discovery of longitudes at sea;[†] it was a body almost analogous to the present French "Bureau des Longitudes." Maskelyne took many steps to induce this Commission to approve of his proposal; and, at the same time, he commissioned several ship-captains to put it to the test. Their reports confirmed his assertions, and on February 9, 1765, Maskelyne presented to the Commissioner of Longitude a detailed report,[‡] in which, besides a complete exposition of the method and plan of a nautical almanac, he gave from the entries in the log-books the result of this new method. The proposition of the wise abbé was adopted, and Maskelyne was entrusted with the calculation and publication of the "Nautical Almanac and Astronomical Ephemeris." The Commissioners did more; they ordered the printing of the Tables of the Moon, left by Tobias Mayer, according to which the lunar distances were to be calculated. At the same time parliament voted a sum of 3,000*l.* to the widow of the astronomer of Göttingen, and a sum of 300*l.* to Euler, for having furnished to Mayer the theorems which he used to construct his theory.*

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[*] Continued from p. 352.

[†] "The Commissioners appointed by Act of Parliament or the discovery of longitude at Sea, and for examining, trying, and judging of all Proposals, Experiments, and Imperiments (*sic*) relative to the same, and encouraging attempts to find a Northern Passage between the Atlantic and Pacific Oceans, and to approach the Northern Pole."

[‡] It is found *in extenso* in the "New and Correct Tables of the Motions of the Sun and Moon," by Tobias Mayer: London, 1770. Published by order of the Commissioner of Longitude.

The first volume of the "Nautical Almanac" is concerned with the year 1767, and appeared in 1766. Although infinitely superior to the "Connaissance des Temps" for 1767, this publication is far from the perfection which it has since attained. Its object is two-fold, but not well-defined; it contains much information useless to the astronomer, and many things besides which the mariner could dispense with. There is first a calendar with the aspects of the planets; then a solar table giving for each day the longitude of the sun at noon, calculated to $\frac{1}{100}$ of a second; the right ascension of the sun in time to $\frac{1}{10}$ of a second, his declination to a second, and the equation of the time; next follow the eclipses of the four first satellites of Jupiter; then tables of the planets, giving the longitude (to a second) and the latitude (to a minute), heliocentric and geocentric, the declination (to a second), the hour of the passage of the meridian (to a minute), every third day for Mercury, and every sixth day for the other planets. The table following gives, for every day from noon to midnight, the longitude (to the $\frac{1}{100}$ of a second) and the latitude (to a second) of the moon, her right ascension and declination from noon to midnight, as well, as her apparent semi-diameter and horizontal parallax. Then follow the distances calculated for every three hours, of the moon from the sun and from a certain number of stars of the first magnitude, and lastly the configuration of the satellites of Jupiter for every day in the year, at 5.30 P.M. The work is completed by detailed and well-written instructions, telling the signification and use of the various tables contained in the volume.

The calculations are, moreover, made with an amount of care far greater, according to Lalande, than was ever bestowed on the "Ephémérides." Each article was calculated separately by two persons and verified by a third calculator. In the case of the longitudes, latitudes, right ascension, declination, semi-diameter, and parallax of the moon, these were calculated by one person for noon and another for midnight, and afterwards verified by the mean of the differences which were carried as far as the fourth order.

Some years later, in 1772, three English astronomers, Lyons, Parkinson, and Williams, published some exceedingly convenient tables, entitled, "Tables for correcting the apparent Distance of the Moon and a Star from the Effects of Refraction and Parallax" (Cambridge, 1772), by the aid of which ten minutes sufficed to calculate an observation of distance between the moon and a star, and therefrom to deduce the longitude. The use of the lunar distances became from that time a great convenience. It was in the same year, 1772, that Lalande transferred into the "Connaissance des Temps" for 1774 the calculations of the lunar distances copied in the "Nautical Almanac," "not having," said he, "either the leisure to do it myself, nor the means which the Commission of Longitude of London furnished to the Astronomer-Royal Maskelyne, for maintaining calculators, whose work he had only to superintend and verify." The introduction of these lunar distances doubled the value of the "Connaissance des Temps," which became a work useful at once to astronomers and mariners.

IV. *Foundation of the Berlin "Astronomisches Jahrbuch"*

This same year, 1774, witnessed the appearance of a great number of publications analogous to the *Connaissance des Temps* and the *Nautical Almanac*, all intended to regulate the publication of the Ephemerides, which in nearly all countries astronomers published at different times. Of these we shall mention the "Jahrbuch" of Berlin, the "Ephemerides" of Vienna, and those of Milan.

The idea of the "Berliner Astronomisches Jahrbuch" originated with Lambert. Born August 29, 1728, at Mulhouse, then a free town of Alsace, of parents who kept a small tailor's shop, Lambert received a very incomplete elementary education, which he afterwards supplemented by assiduous labour and persevering determination. In 1748 Count Pierre de Solis entrusted Lambert with the education of his children; this was an opportunity of which he knew how to take advantage. He found in the Chateau of Coire, the abode of this nobleman, an exceedingly rich library, by means of which he not only completed his imperfect education, but from which he drew the elements of one of his finest works, the "Dissertation on the remarkable Properties of Light." Shortly after, in 1763, the restraints to which Protestants were subjected in France, and in particular the law which prohibited them from exercising any public functions, induced him to yield to the invitations of Frederick the Great; Lambert went to live at Berlin, and became, in 1764, a *pensionnaire* of the Royal Academy of Prussia. France thus lost one of her scientific glories; for, not only was Lambert a distinguished astronomer, but pre-eminently remarkable for the universality and extent of his attainments.*

Long before the time to which we refer there had appeared at Berlin Astronomical Ephemerides; the first, due to the astronomer Grischow, date from 1749; it is the "Calendarium ad annum 1749 pro meridianum Berolinense cum approbatione Academicæ regiæ Scientiarum et elegantiarum litterarum Borussiæ." They were carried on by Grischow until 1754, and suffered afterwards many interruptions. It was these Ephemerides which Lambert undertook to revive. According to the plan which he proposed to the Academy of Berlin, each volume of the "Jahrbuch" would appear two years in advance and consist of two parts. One part was devoted to the astronomical ephemerides (Prussia not then having any marine, Lambert had not to trouble himself with nautical ephemerides) and so disposed that it could easily serve for a place of different latitude; the other forming a collection of all the news concerning the astronomical sciences (observations, remarks, and problems). Lambert also proposed to collect, in another work, all the tables serving either for the calculation of the ephemerides or for other astronomical calculations.

The proposal of Lambert having been adopted, an astronomer who was afterwards director of the Berlin observatory, and whose reputation became universal, J. El. Bode, was entrusted, under the direction of Lambert and the nominal superintendence of the Academy, with the numerous calculations which the publication of these Ephemerides necessitated. The first volume appeared in 1774, under the title of "Berliner astonomisches Jahrbuch für 1776, unter aufsicht und mit Genehmhaltung der koniglichen Academie der Wischenschaften verfertigt und zum Drucke befördert."

Lambert had the direction of the "Jahrbuch" for only a very short time; death came soon after to deprive Science of one of her most ardent worshippers. Nevertheless his initiative, though of short duration, was successful, and from its first appearance, the work which he founded progressed more notably than those which preceded it.

At the same time also appeared the Ephemerides of Milan,—"Effemeridi artronomiche per l'anno 1775, calculate pol meridiano di Milano, del abbe Angelo de

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* Fifty years later, another parliament authorised the printing of the new lunar tables of Hansen, his compatriot, and awarded to that illustrious astronomer a sum of 1,000*l.* by way of national recompense.

* His most important astronomical work is entitled "Insigniores Orbitæ Cometarum Proprietates."

Cesaris." It was also the first volume of a series of ephemerides which have been since continued without interruption.

In 1799 the publication of the Portuguese ephemerides commenced—"Ephemerides astronomicas calculadas para o meridiano Observatorio nacional de universidade de Coimbra, para uso do mesmo Observatorio, e para o da navegaçao Portugueza."

Lastly, in 1756, appeared the ephemerides of Vienna:—"Ephemerides astronomicæ anni 1757, ad meridianum Vindobonensem Jussu Augustorum calculis a Maximiliano Hell. Cæsario regio astronomo et Mechanicus experimentalis professore publico et ordinis," which were continued by Triesnecker. The Ephemerides of Vienna were constructed upon the model of the Abbé de la Caille, much more than upon that of the *Connaissance des Temps*. Moreover, at this period, the Ephemerides of La Caille were almost exclusively employed by French astronomers.

(*To be continued*)

THE BRIGHTON AQUARIUM

IN accordance with an intention entertained previous to resigning the tenure of my office as Curator to the Brighton Aquarium, I propose to give a brief outline of the plan of construction and general system of arrangements obtaining in that institution.

The Brighton Aquarium, while emulated by several buildings of a similar nature, in different parts of the kingdom and on the Continent, still holds its own in being on a scale of magnitude hitherto unsurpassed, more than one of its tanks, in illustration of this, being of sufficient size to accommodate the evolutions of porpoises and other small Cetaceæ. The architect and originator of the undertaking, Mr. Edward Birch, well known as the engineer of the new pier at Hastings, entertained the idea of constructing this Aquarium as long ago as the year 1866 when visiting the one on a small scale then existing at Boulogne; Brighton was selected as a site on account of its proximity to the sea-coast and its great popularity as a place of resort. The works were commenced in the autumn of the year 1869, but owing to various interruptions the building was not formally thrown open to the public until August 1872, the ceremony taking place during the week in which the members of the British Association honoured Brighton as their place of meeting.

The area occupied by the Brighton Aquarium averages 715 feet in length by 100 feet in width, running east and west along the shore line between the sea and the Marine Parade; the principal entrance is at the west end facing the eastern angle of the Royal Albion Hotel. The building internally is divided into two corridors separated from one another by a fernery and considerable interspace. The approach to the first or Western corridor is gained through a spacious entrance-hall supplied with reading-tables, and containing between the pillars which support the roof portable receptacles of sea-water for the display of small marine specimens that would be lost to sight in the larger tanks.

The tanks for ordinary exhibition commence with No. 1 on the left side of the western corridor, and, as shown in the ground-plan, follow in consecutive order round the two corridors, the last, No. 41, immediately facing No. 1. The smallest of these tanks measures 11 feet long by 10 feet broad, and is capable of holding some 4,000 gallons of water, while the largest, No. 6, in the western corridor, and the subject of the accompanying engraving, presents a total frontage, including the two angles of 130 feet, with a greatest width of 30 feet, and contains no less than 110,000 gallons. Every gradation of size occurs between these two extremes, the depth of the water in all ranging from 5 to 6 feet. Supplementary to the foregoing, a series of half-a-dozen shallow octagonal table-tanks occupies a portion of the interspace between the two corridors, these being especially adapted for the exhibition of animals such as starfish, anemones, and others seen to best advantage when viewed perpendicularly through the water. Flanking one side of this same interspace are several ponds fenced off for the reception of seals and other amphibious mammalia and larger Reptilia, while at its further or eastern extremity artistic rock-work runs to a height of 40 feet, thickly planted with choice ferns and suitable exotic plants, and broken in its course by a picturesque waterfall and stream. Tanks 12 to 17 in the eastern corridor, in addition to the stream and basin beneath the waterfall, are set apart for the exclusive exhibition of fresh-water fish, the remaining tanks being devoted to marine species. The bulk of water thus utilised in the fresh and sea-water tanks collectively amounts to 500,000 gallons, and in addition to this several smaller store tanks in the Naturalists' Room, adjoining the eastern corridor, afford accommodation for reserve stock, or for new arrivals before their display to public view.

The style of architecture dominant throughout the building is Italian and highly ornate, the arched roof of the corridors being groined and constructed of variegated bricks, supported on columns of Bath stone, polished serpentine marble, and Aberdeen granite; the capital of each column is elaborately carved in some appropriate marine device, while the floor in correspondence is laid out in acrostic tiles. The divisions constituting the fronts of the tanks are composed each of three sheets of plate glass, each plate having a thickness of one inch, and measuring six feet high by three feet wide, separated from one another and supported centrally by upright massive iron mullions; in the smallest tanks the front is represented by but one of these divisions, while that of the largest, No. 6, consists of as many as eleven. Among other conspicuous structural features of the aquarium demanding notice are the huge masses of rock entering into the composition of the tanks and fernery. Part of these are composed of porous tufa brought from Derbyshire, while the remaining and greater portion presents the appearance at first sight of old Red Sandstone of the Devonian epoch. This latter, however, is entirely artificial, being built up of smaller nondescript fragments, faced with cement and coloured sand, though so true to Nature have the boulders been fashioned and stratigraphically arranged, that more than one eminent geologist has been deceived by their aspect, and it is difficult in looking into the larger tanks to get rid of the impression that some of the miniature picturesque coves characteristic of the Devonshire coast have been transported bodily to Brighton.

The system adopted at the Brighton Aquarium for continually renewing the supply of oxygen necessary for the well-being of the animals agrees with that followed at Berlin, streams of compressed air being constantly forced into the tanks through vulcanite tubes carried to the bottom of the water, and each tank being fitted with a greater or less number of these tubes according to its size. Following the same principle there is no true circulation, each tank being distinctly independent and the same water remaining in it perpetually unless required to be changed on account of turbidity, an accident such as the cracking of a front glass, or for altering the arrangement of the inhabitants. In such cases the tanks are refilled from four large reservoirs situated beneath the corridors, holding in aggregate a quantity approximating but not exceeding that contained in the tanks above, and into which the water is first pumped by a six-horse power centrifugal engine direct from the sea, and thence conveyed by the same force to the tanks, through a main extending round the building.

The system above described, while practical in aquaria at the seaside, where the supply of water is unlimited, does not answer inland, as exemplified in the decadence, from a scientific point, of the one from which that at Brighton is copied, and even in the former case is associated with serious drawbacks and disadvantages, which forbid it from yielding in compensation for the outlay and labour expended the results realised by those constructed on later and more approved principles. It is impossible, for instance, to keep in health at the Brighton Aquarium the number of fish in comparison to the size of any given tank as will be found in the aquarium at the Crystal Palace or that of Hamburgh, or Copenhagen, or any other constructed on the same principle, though at the same time it is essential to remark, that lately the capabilities of the Brighton tanks have not been turned to their greatest advantage, as instanced in No. 6, holding 110,000 gallons of water, which for many weeks past has been occupied by but three dogfish, a ray, and a few turtle; No. 11, with 9,000 gallons, by two mackerel, and so on. A remaining still greater source of dissatisfaction associated with the non-circulatory system, and yet one capable, perhaps, of full appreciation by those only who have held practical aquarium responsibility, arises from the difficulty, verging upon the impossibility, of maintaining the tanks uniformly bright and clear throughout the building. Some fish foul the water to a much greater extent than others, notably the Flat-fish or Pleuronectidæ, who in a few weeks will render a clear isolated tank too opaque for the opposite side some twenty feet distant to be discerned. The only existing remedy for such a case is to run off the water and supply fresh from the reservoirs beneath; but this water being drawn from the shore-line, the feeding pipe remaining exposed at half tide, is necessarily loaded with impurities, which re-agitated by the action of pumping involves the lapse of several more days before the tank is in a fit state for exhibition. At the suggestion of my predecessor, the late Mr. J. K. Lord, oysters and other bivalve mollusca were introduced into the tanks for the purpose of removing the organic particles which rendered the water turbid, but though these have proved of great service, the root of the evil remains undisturbed, and it is only by the application of the circulatory system, securing with it the more thorough oxygenisation of the water, that the problem is to be effectually solved.

This system, initiated by Mr. W. A. Lloyd at the Hamburgh Aquarium, and now maintained under his personal superintendence at the Crystal Palace, consists in having, in the first place, a bulk of water in the reservoirs beneath exceeding by four or five times the total amount contained in the tanks above, and which, being pumped up by steam power and circulated through the building, takes up in its course by exposure to the atmosphere an amount of oxygen, permitting the preservation in health not only of a much larger number of inhabitants to each tank, but at the same time communicates to the water a degree of clearness and brilliancy unattainable by other means, and which brilliancy is increased or diminished in exact proportion to the uniformity and force of the current so maintained. One theoretical objection urged by the architect of the Brighton Aquarium against the circulatory system, is that in the event of paint or other deleterious substance falling into any one tank the water of the neighbouring tanks would suffer equally. Practically, such mishaps have no business to occur, and though in such a case, on the "siphon" mode of circulation first attempted but abandoned as impractical at Brighton, some mischief might be done, it would be impossible under that to be presently suggested as still feasible at the institution here under consideration, and until the adoption of which the Brighton Aquarium cannot be expected to fully realise the highest anticipations of its promoters, while the greater or less turbidity of its tanks must continue as hitherto a constant source of dissatisfaction to the directors, and of anxiety and mortification to the officers held responsible.

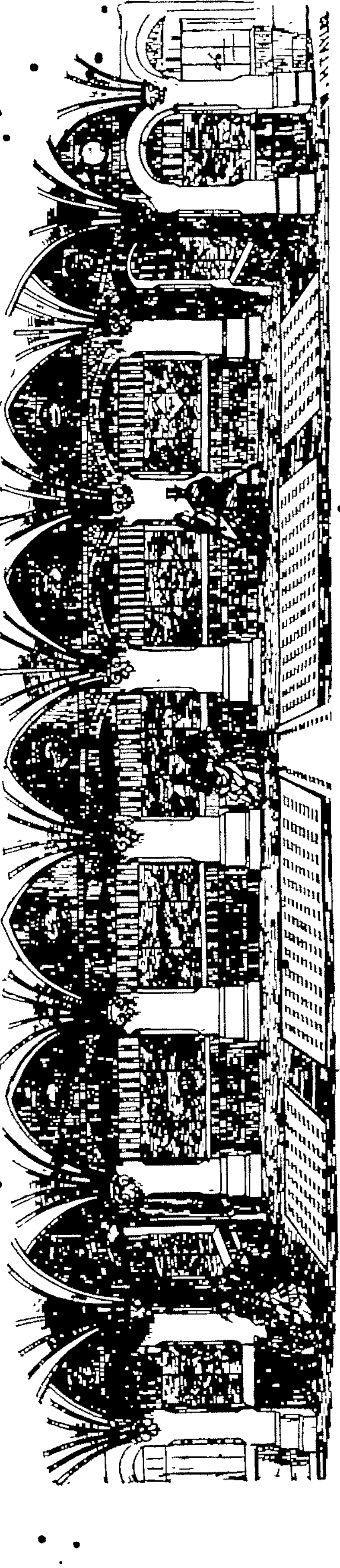
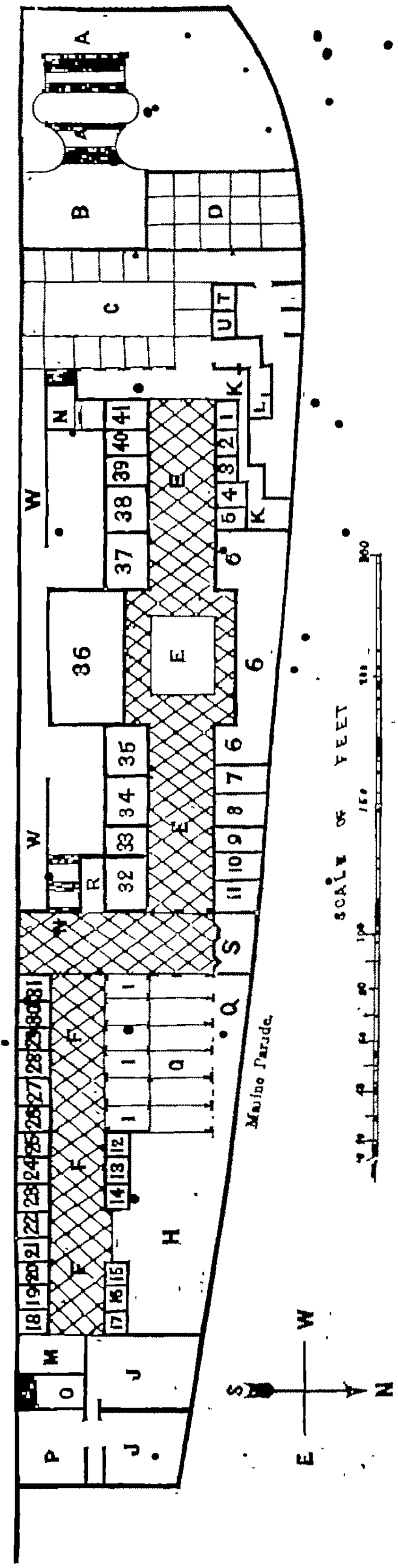


Fig. 1.—Front View of Tank No 6 (110ft long), Western Corridor, Brighton Aquarium.



A Steps descending from West End. B Entrance Court C Entrance Hall and Reading Room. D Restaurant and Dining Hall EEE Western Corridor with Tanks on each side. FFF Eastern Corridor G Interspace. H Rockwork, Fernery, and Cascade III Table Tanks JJ Engine Room, Stores, &c KK Ladies' Cloak Room. LL Gentlemen's do. M Naturalist's Room. N Steps to Corridors with Promenade over O Business and Private Entrance. P Curator's Office. Q Rockwork, with Seal-Ponds, &c. R Grotto. S Heating Apparatus. T Clerk's Office. U Board Room. W Inclines for Hand Chairs.

At the eastern extremity of the building there is still a considerable plot of ground unutilised belonging to the company; admirably adapted for the construction of a supplementary reservoir, holding, say, one million and a half gallons, and whose contents, added to those of the existing ones, would yield a body of water sufficient for the purpose. This ground, in fact, being considerably above the level of the tanks, would permit of simplifying matters by pumping the water up from the existing reservoirs to the proposed new one, whence it would circulate through the tanks and return to its original source by the mere force of gravitation. As now the individuality of each tank might be maintained, the water flowing from the main through the existing cocks, and escaping to the reservoirs beneath, through the same overflows, two or more of which are supplied to each tank, which might be enlarged or further multiplied, if requisite to carry off the accelerated stream. Should any tank now become unduly turbid through unforseen circumstances, the accident could be immediately remedied by emptying it into the large reservoirs beneath where so small a quantity in proportion would effect no appreciable alteration, the tank being refilled through the main; while in the event of paint or other poisonous ingredient being upset the water would be run off to waste as under existing circumstances. An important and essential preliminary step to these proposed alterations will, however, be to render the existing reservoirs watertight, their present defective condition in this respect being one of the chief obstacles to the storage of clear water, which in an inland aquarium would have simply proved the ruin of the undertaking. These difficulties surmounted, and the reservoirs filled with water drawn from some little distance off shore, say at the head of the pier close by, and so free from the chalky wash and innumerable organic impurities inseparable from the present supply, the aquarium will be entirely independent of the sea, and much waste of labour now occupied in pumping from it saved. The still more important results accruing to the institution through the uniform clearness of the water, and the capability of each tank to support a number of inhabitants compatible to its size, cannot be overrated.*

Under any circumstances, if the remedy here proposed is not adopted, it is to be trusted that the weak points of the Brighton Aquarium here noticed will prove sufficient to prevent the repetition of the same errors of construction in any of the Aquaria now in contemplation or being built in this country. On the Continent, the type initiated by our own countryman at Hamburgh, and at the Crystal Palace, with such improvements as practical experience dictates, is almost invariably adopted, and it is incumbent that England, as the initiator of the movement, should maintain her lead. So far, from its size and its proximity to the sea, the Brighton Aquarium has been able to achieve results unrealised by any other institution of its description, as instanced in the recent preservation in its tanks of creatures so large as porpoises, and fish so delicately constituted as herrings and mackerel; but these results are by no means commensurate with the expenditure involved in its establishment, and we hope steps will be forthwith taken to remedy the defects indicated.

The Isle of Wight and the Devonshire coast, especially Torquay, are localities offering far greater advantages than Brighton, as zoological stations for the acquisition of specimens, and now that the financial success of large Aquaria under judicious management in centres of sufficient population is well established, the temptation these places offer to an enterprising company cannot be long resisted.

W. SAVILLE KENT

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* Defects of construction in the Brighton Aquarium likely to interfere with the future efficiency of the establishment were alluded to without specification, while the building was in the hands of the contractor.—See NATURE, vol. iv. p. 394.

THE RAPIDITY OF DETONATION

A CIRCUMSTANCE of singular interest has recently been revealed in connection with the investigations still being carried on with gun-cotton at Woolwich Arsenal. The experiments made with this powerful explosive have now extended over a period of ten years, and although many discoveries of vital interest have been made by Professor Abel and by Mr. E. O. Brown, who is aiding in the research, the results teach us, before everything, how much more we have yet to learn of the properties of pyroxilin. First, of all, the violence of its explosion had to be tamed, then a compressed form of the material was devised, and after that it was shown that, like its sister-explosive, nitro-glycerine, gun-cotton could be violently detonated, if ignited by a charge of fulminate. Gun-cotton, in fact, turns out to be sympathetic, for, according to the energy with which it is inflamed, so it responds in its behaviour: thus, if gently ignited by a spark, the cotton, in the form of yarn, smouldered slowly away; when set fire to by a flame, it burnt up rapidly; if in the form of a charge it was exploded in a mine or a fire-arm, it at once resented the shock and replied with corresponding energy, behaving like gunpowder under similar circumstances; while, lastly, if fired with great violence with a few grains of fulminate, it is detonated with as much force and with the same terrible effect as its instigator.

More recently, as many may have heard, our investigators have succeeded in detonating, or, in other words, exploding to the best advantage, gun-cotton when in a damp condition; and in this state the explosion is every bit as violent as when the material is dry. This grand discovery is naturally of the utmost importance, because, although many objections may be advanced as to the danger of storing and using gun-cotton when dry, the most nervous of us would scarcely hesitate to employ it sopping wet. In this latter condition the material is, strange to say, not only non-explosive, but positively non-inflammable; so much so, indeed, that it would be probably as serviceable in putting out a fire as a wet blanket or a damp towel would be. It can neither be inflamed nor exploded when wet; and further, unless one has the key to its detonation—a little fulminate of mercury—it is of no more value as an explosive than so much wet paper pulp. When placed in contact, however, with a fuse of the proper construction and a cake of dry gun-cotton, to start the action, the wet pyroxiline, as we have said before, detonates as readily as when the moisture amounts to but a fraction of a per cent. Moreover, the quantity of water in the material is really of no importance, for it has been found that for submarine mines, compressed cakes enclosed in a fishing-net and thrown overboard with a dry primer and a fulminate fuse, will explode with just as much energy as when confined in a water-tight steel case.

It is in respect to this detonation, and more particularly to the rapidity of its action, that we desire to speak at the present moment. Recent experiment has shown that the rapidity with which gun-cotton detonates is altogether unprecedented, the swiftness of the action being truly marvellous. Indeed, with the exception of light and electricity, the detonation of gun-cotton travels faster than anything else we are cognizant of. Thus, detonation will run along a line of gun-cotton cakes, placed so as to touch one another. with a rapidity only inferior to that of electricity, setting fire to a charge or conveying a signal, if desired, almost instantaneously. Twenty thousand feet, or nearly three miles per second, is calculated to be its rate of travelling according to Noble's electric chronoscope. In one experiment forty-two feet of the material was fired, and records secured at every six feet; and in this case the results given were most uniform, for the velocity only varied from nineteen to twenty thousand feet per second, the ratio of transit being in no instance less than this.

To form an approximate idea of this extraordinary rapidity, it is necessary to call to mind the rates of travelling of other mediums. Light and electricity we may leave out of the question, as these are immaterial bodies. A bullet usually flies at the rate of 1,300 feet per second, although rifled barrels have been known to project a shot with a velocity of 1,900 feet. Sound is much slower in travelling, for a second of time is required in getting through some 1,100 feet. A quick match of the most delicate construction would probably be longer still in making way, and a train of gunpowder would be left far behind. So it may be safely affirmed, we think, that the detonation of gun-cotton travels more rapidly than any other known medium, with the exception, we repeat, of light and electricity.

It is curious to note that not every detonating or fulminating substance will induce the detonation of gun-cotton. It seems as if a certain number of vibrations require to be set up—a certain key-note to be struck—in order to secure the decomposition of the material. Thus it is found that fulminate of mercury detonates guncotton readily, while again it is also capable of being detonated by itself; so that if a line of compressed cakes is detonated at one end by a charge of fulminate of mercury, the detonation is communicated rapidly from one cake to another, until they are all consumed. Nor does the force diminish at all as it runs along the line, as might perhaps be imagined; if this were the case, the detonation set up at the beginning of a line would only run up to a certain distance, and there come to a full stop, as soon, that is, as the vibrations are insufficient to explode the gun-cotton. This, however, does not happen in actual experiment; and, on reflection, it stands to reason that if the first cake of pyroxilin is capable of firing the second one, the ninety-ninth is just as ready to detonate the hundredth. Thus the detonation can be carried along a line of any length, and the force is as powerful and violent at the end as it was at the beginning.

This property of gun-cotton may obviously be put to valuable use both in industrial and military operations. In any case where it is of importance that a series of blasting or mining charges should be fired simultaneously, their connection together by means of gun-cotton would ensure such a result. True, the same effect could be obtained by means of an arrangement of insulated wires, the charges being detonated simultaneously with the aid of a battery, but such a plan is not always convenient nor practicable. For cutting down palisades, or stout wooden walls, a line of gun-cotton discs exploded in this way would be most efficacious; and a more ready plan of felling timber does not probably exist than that of placing around the stem of a tree a chain or necklace of the explosive in the form of compressed cakes, the detonation of these dividing the trunk as sharply as the keenest axe.

NOTES

WE read in the *Daily News*:—"Mr. Henry Cole, C.B., presided at the annual meeting of the Hanley School of Art, on Monday evening, and after speaking of the results of the South Kensington Museum, said it was his painful duty to announce that this organisation, which had borne such great fruits, and which was so highly prized by the nation, and was so indispensable to the commercial and social progress of this country, was in jeopardy. The Government contemplated changes which were directly opposed to the further development of the Science and Art Department. It had hitherto flourished under a management which ensured individual Parliamentary responsibility, but it was now proposed to hand over South Kensington to the Trustees of the British Museum, who were already fully occupied in their own departments. The management of the British Museum was not such as to make them desirous of seeing it extended to South Kensington, nor were fifty trustees the proper administrators of public money to the amount of hundreds of thousands a year granted to science and art. He appealed to art students throughout the country not to allow the work of the Prince Consort to be destroyed, and the means of their own instruction to be taken away or muddled with old decaying notions. He urged them to call upon their representatives in Parliament—and an election was not far off—to protect their interests and rights from unprincipled invasion; and he offered his humble services, if he could assist them, to preserve the institution which the Prince Consort founded from the hands of the ignorant spoiler. Mr. Melly, M.P., in proposing a vote of thanks to Mr. Cole, spoke in terms of praise of his efforts to spread art and science, and said it would be far more sensible to transfer the British Museum to South Kensington than to place the latter under the management of the British Museum. It was not by following antiquated notions that the work of education was to be carried on, but by adopting the free-trade principle which Mr. Cole had carried out at South Kensington. He was the Cobden and Bright in the education of art and science. He had been in this matter a true free-trader, and in following the public he had served it. Mr. Cole, in responding, offered 50*l.* towards the establishment of a local museum." Surely it is monstrous that while a Royal Commission is sitting to inquire into these matters the Government should thus attempt to make the Commissioners look ridiculous by taking such a step without waiting for their report. This is another instance of the ignorant action of Government in all matters appertaining to Science.

THE *Challenger* reached the coast of Brazil on September 15 last, after a successful but rather stormy voyage across the Atlantic. She was to have left Bahia on September 25 for the Cape of Good Hope.

MR. SCLATER has received a letter from Dr. A. B. Meyer, announcing his return to Vienna after a most successful expedition to New Guinea. Dr. Meyer landed in Mac Cleur's inlet on the west coast, and crossed the main land to the Bay of Geelvink. He has obtained fresh specimens of nearly all the known Paradise-birds, and of one which he bellves to be new to science.

THE examination for Natural Science Scholarships, held in common at the same time and with the same papers for Magdalen, Merton, and Jesus Colleges, Oxford, has terminated in the following elections:—At Magdalen College, to the Demyship, Mr. W. W. Jones, of Clifton College; to the Exhibition, Mr. F. J. Bell, of Christ's Hospital. At Merton College, to the Postmasterships, Mr. W. Carter, of Blackburn Science School, and Mr. F. J. Bell, of Christ's Hospital. At Jesus College, to the Scholarship, Mr. E. W. Poulter. It will be seen that Mr. Bell was elected by two colleges and has decided to accept the election to Magdalen. There were fourteen candidates. The election to the Biological Fellowship at Magdalen College took place on Saturday last, when Mr. C. J. F. Yule, of St. John's College, Cambridge, was announced as the successful candidate. The election to the Physics Fellowship at Merton College will not take place until Oct. 30.

WE regret to have to announce the death of M. Jules Pierre Verreaux, Aide-Naturaliste au Musée d'Histoire Naturelle du Jardin des Plantes. M. Verreaux was a great traveller in early life, and enriched the French National Museum by large collections from the Cape and Australia. On his return to Europe, he was for many years scientific assistant to his brother, the late Edward Verreaux, at the Maison Verreaux in the Place Royale at Paris, so well known to naturalists of all countries. After his brother's death, M. Verreaux accepted the office in the Jardin des Plantes, which he held until his decease. M. Verreaux had a very complete and extensive knowledge of the class of birds, and was the author of numerous ornithological memoirs and papers. His loss will be severely felt by ornithologists who have occasion to consult the rich collection in the Jardin des Plantes, and by many friends and correspondents in this country and elsewhere.

WE have also to record the death of Dr. Otto Wacherer, a German physician, resident at Bahia, who made large collections in various branches of Natural History, and was the author of an excellent memoir on the Ophidians of that district of South America, published in the Zoological Society's "Proceedings."

DR. BESSELS, of the *Polaris* expedition, has given evidence that the death of Captain Hall was solely due to natural causes.

SIR C. B. ADDERLEY, M.P., speaking at the annual meeting of Saltley Reformatory yesterday, expressed his satisfaction at the undoubted diminution of crime in this country. He did not attribute the decrease to any change in our system of secondary punishments but to the gradual spread of education and enlightenment, more especially among the lower classes.

ON November 18 there will be an election at Balliol College, Oxford, to a scholarship on the foundation of Miss Hannah Brackenbury, "for the encouragement of the Study of Natural Science;" worth 80*l.* a year (and tuition free) for four years: open to all such candidates as shall not have exceeded eight terms from Matriculation. At ten o'clock, A.M., papers will be set in the following subjects:—(1) Mechanical Philosophy and Physics; (2) Chemistry; (3) Physiology; but candidates will not be expected to offer themselves in more than two of these. There will also be a practical examination in one or more of the above subjects, if the examiners think it expedient. Candidates are requested to communicate their intention to the Master of Balliol by letter, on or before Monday, November 10, enclosing testimonials from their colleges or schools, and (if members of the University) certificates of their Matriculation; and stating the subjects in which they offer themselves for examination.

WE have received the List of the Candidates who took Honours at the May Examination of Science Schools and Classes in connection with the Science and Art Department. We are sorry that our space does not permit us to publish the list of names, which we are glad to see is very large, it is, moreover, very gratifying to notice that in nearly every department a considerable proportion of the successful Candidates have been "self-taught."

THE following science-teachers, who attended the special course of instruction in magnetism and electricity to science-teachers, in connection with the Science and Art Department, having passed first class, are registered as qualified to earn payments in magnetism and electricity:—T. N. Andrews, G. Armstrong, T. Bayley, J. Brealand, R. Brown, W. Cook, S. Cooke, J. Hamilton, H. Harris, J. Harte, D. Low, S G. Maunder, A. J. Rider, A. Robinson. J. Sayle, J. Simpson, C. Symons, P. H. Trachy, J. Webb, J. W Woods. The following for the same reason are registered as qualified to earn payment in acoustics, light, and heat:—J. Alexander, T. J. Baker, S. Barbour, J. Beavis, G. R. Begley, P. Doyle, J. B. Duckett, T. Elliott, T. Isherwood, G. Jeffrey, L. M. Leader, E. Leech, E. Magennis, J. Marshall, J. Moylan, W. Patterson, E. Reynolds, L. J. Ryan, J. Schofield, G. Severs, W. J. Snowdon, W. Sturgess, C. Symons.

THE name of Dr. Kaup, whose death we noted last week, was inadvertently misspelled "Kemp." Jean Jacques Kaup was Grand-ducal Inspector of the Natural History Museum of Darmstadt.

SIR SAMUEL AND LADY BAKER, it is said, have accepted an invitation from the Geographical Society of New York to visit that city during the summer months of next year.

THE inaugural lectures in connection with the scheme of education adopted by the University of Cambridge for the town of Nottingham, were delivered on the 9th inst. in the Lecture Hall of the Mechanics' Institution of that town, and were largely attended. Mr. E. B. Birks, M.A., Fellow of Trinity, who has been appointed to conduct classes and to lecture on English Literature, gave his inaugural lecture in the afternoon to a large audience, composed principally of ladies, for whom this subject has been specially selected; and in the evening Mr. V. H. Stanton, M.A., Fellow of Trinity, who had been appointed to teach Political Economy, opened his course. On Friday week Mr. T. O. Harding, B.A., B.Sc., Fellow of Trinity, commenced his instruction in "Force and Motion," the introduction to Physical Science. The Session will continue to next April, and will be divided into two terms. For the second term, which will commence after Christmas, arrangements have been made for the study of Astronomy, Physical Geography, and English Constitutional History. Examinations will be held at the conclusion of each Term in the work done, and University Certificates will be granted to those who succeed in them.

WE learn from the *Bulletin International* of the Paris Observatory, that Lieutenant Parent and Dr. Wykander, while passing the winter of 1872–3 on the coast of Spitzbergen, made a series of spectrum observations on the Aurora, and determined seven different spectral lines, which, according to Wykander, are identical with the spectrum at the bottom of the flame of a candle or petroleum lamp.

MESSRS. ROUTLEDGE & SONS, have in the press, a "New Illustrated Natural History," by the Rev. J. G. Wood, M.A., with 500 Illustrations; and "The Book of African Travel," by W. H. G. Kingston. This work is intended to give records of the journeys of all the celebrated travellers in Africa down to the present time. It will be profusely illustrated.

MESSRS. HODDER and STOUGHTON will shortly publish "Life, Wanderings, and Labours in Eastern Africa," with an account of the first successful ascent of the equatorial snow mountain Kilima Njaro, and remarks on the East African slave trade, by the Rev. Chas. New, of the Livingstone Search and Relief Expedition, illustrated.

THE annual migration of the butterfly from east to west across the isthmus of Panama in August and September was, according to the *Star*, proceeding. The butterfly has golden green stripes on a black ground, and is very beautiful. It has been recognised by Mr. O. Salvin, of London and Guatemala, as the *Urania fulgens*.

WE have received the diminutive prospectus of what is likely to be, at least, an ingenious and curious work; it is entitled "Chemistianity," and will contain "2,000 chemical facts, relating to inorganic chemistry, explained within 5,000 lines of oratorical verse, compiled by permission from the works of leading chemists of the day; together with the views of the author (expressed in verse) as to the advantages of a general knowledge of chemistry." If the book is readable it will certainly be a triumph of ingenuity, if not of genius, on the part of the author, Mr. J. C. Sellars, manufacturing chemist, Birkenhead, who is also publisher.

IN the *Chemical News* for Oct. 17 will be found a long list of subjects for prizes to be awarded in May 1874, by the Société Industrielle de Mulhouse.

THE first three parts are published (price 6*d.* each) of "British Marine Algæ; being a popular account of the Seaweeds of Great Britain, their collection and preservation," by W. H. Grattann. It is intended as a cheap and popular rather than scientific handbook to our marine flora, and will apparently serve a very useful purpose as such. The illustrations, though on a small scale, are sufficient to recognise the more striking forms.

THE last two parts, xi. and xii., of the new edition of Griffith and Henfrey's Micrographic Dictionary, bring down the work as far as Hydra. The botanical articles have been written up to the present state of science by the Rev. M. J. Berkeley.

MR. A. ELLEY FINCH has published the lecture he delivered last March before the Sunday Lecture Society, "On the Pursuit of Truth." We think he has done well in so doing, as he shows clearly and shortly the only principles of evidence upon which permanent and satisfactory belief can be founded, showing the distinction between the evidence which satisfies the theologian, the lawyer, and the man of science. Mr. Finch has added many footnotes and appendices, which, though often irrelevant, are in most cases valuable and interesting, the appendices being mostly abstracts of passages from the works of well-known authors bearing more or less on the subject alluded to in the lecture. We wish the lecture a large circulation among the general public, whom it would tend to enlighten.

THE *Gazette de Vos* publishes some statistics with regard to education in Germany, which appear in *La Nature*. According to the latest official information, the German Empire numbers 380 gymnasiums, pro-gymnasiums, and academies (*lycées*); 156 Latin schools (in Bavaria and Wurtemberg); 270 "real-schulen," 12 high schools, technical and polytechnic. Prussia possesses besides, 26 provincial schools of arts and industry; Saxony, 5 commercial schools and 4 schools of arts, industry, and architecture; Saxe-Coburg-Gotha, 3 schools of the kind last mentioned; the City of Hamburg possesses a school of art for boys and another for girls. Bavaria has 33 schools of arts, commerce, and agriculture; Prussia, 26 agricultural schools, with 41 winter schools of rural economy. The rest of the German Empire possesses 56 other schools belonging to one or other of these categories. Prussia numbers 260 superior public schools for girls, and the rest of Germany, 54. 143 seminaries for the training of teachers are in full activity in the German Empire during the present year; primary instruction is given in 60,000 schools. All the German States have schools for deaf-mutes and for the blind; Prussia possesses 35 for the former and 14 for the latter. With regard to schools for the artistic professions, Bavaria occupies the first rank, but Wurtemberg and Prussia have latterly made great progress in this direction.

"THE Pearl of the Antilles; or, An Artist in Cuba," by Walter Goodman, is the title of a volume just published by Messrs. King & Co. Since Mr Goodman calls himself an artist, we should have expected a few illustrations of Cuban scenery in his work, but there are none. The work makes no pretensions to be a contribution to the natural history of Cuba, but in a very entertaining manner the author gives a series of sketches of social life on the lovely island.

THE additions to the Zoological Society's collection during the past week include two Weka Rails (*Ocydromus australis*) from New Zealand, presented by the Acclimatisation Society of Otago; an Alligator (*Alligator mississippiensis*) from New Orleans, presented by Capt. M. Cowper; two Patagonian Conures (*Conurus patagonus*) from Chili, two Solitary Tinamous (*Tinamus solitarius*) from Brazil, received in exchange; a Macaque Monkey (*Macacus cynomolgus*) and a Bonnet Monkey (*M. radiatus*) from India, presented by Mr. G. Veitch, and deposited; a Cape Petrel (*Daption capensis*), purchased, from Manilla, which is the first specimen of this bird obtained by the Society.

ON THE FINAL STATE OF A SYSTEM OF MOLECULES IN MOTION SUBJECT TO FORCES OF ANY KIND

LET perfectly elastic molecules of different kinds be in motion within a vessel with perfectly elastic sides, and let each kind of molecules be acted on by forces which have a potential, the form of which may be different for different kinds of molecules.

Let x, y, z, be the the coordinates of a molecule, M, and ξ, η, ζ the components of its velocity, and let it be required to determine the number of molecules of a given kind which, on an average, have their coordinates between x and $x + dx$, y and $y + dy$, z and $z + dz$, and also their component velocities between ξ and $\xi + d\xi$, η and $\eta + d\eta$ and ζ and $\zeta + d\zeta$. This number must depend on the coordinates and the components of velocities and on the limits of these quantities. We may therefore write it

$$dN = f(x, y, z, \xi, \eta, \zeta)\,dx\,dy\,dz\,d\xi\,d\eta\,d\zeta \qquad (1)$$

We shall begin by investigating the manner in which this quantity depends on the components of velocity, before we proceed to determine in what way it depends on the coordinates.

If we distinguish by suffixes the quantities corresponding to different kinds of molecules, the whole number of molecules of the first and second kind within a given space which have velocities within given limits may be written

$$f_1(\xi_1, \eta_1, \zeta_1)\,d\xi_1, d\eta_1, d\zeta_1 = n_1 \qquad (2)$$

and

$$f_2(\xi_2, \eta_2, \zeta_2)\,d\xi_2\,d\eta_2\,d\zeta_2 = n_2 \qquad (3)$$

The number of pairs which can be formed by taking one molecule of each kind is $n_1 n_2$.

Let a pair of molecules encounter each other, and after the encounter let their component velocities be ξ_1', η_1', ζ_1' and ξ_2', η_2', ζ_2'. The nature of the encounter is completely defined when we know $\xi_2 - \xi_1$, $\eta_2 - \eta_1$, $\zeta_2 - \zeta_1$ the velocity of the second molecule relative to the first before the encounter, and $x_2 - x_1$, $y_2 - y_1$, $z_2 - z_1$ the position of the centre of the second molecule relative to the first at the instant of the encounter. When these quantities are given, $\xi_2' - \xi_1'$, $\eta_2' - \eta_1'$ and $\zeta_2' - \zeta_1'$, the components of the relative velocity after the encounter, are determinable.

Hence, putting α, β, γ for these relative velocities, and a, b, c for the relative positions, we find for the number of molecules of the first kind having velocities between the limits ξ_1 and $\xi_1 + d\xi$, &c., which encounter molecules of the second kind having velocities between the limits ξ_2 and $\xi_2 + d\xi$, &c., in such a way that the relative velocities lie between α and $\alpha + d\alpha$, &c., and the relative positions between a and $a + da$, &c

$$f_1(\xi_1, \eta_1, \zeta_1)\,d\xi\,d\eta\,d\zeta\; f_2(\xi_2, \eta_2, \zeta_2)\,d\xi\,d\eta\,d\zeta\; \phi(abc\,\alpha\beta\gamma)\,da\,db\,dc\,d\alpha\,d\beta\,d\gamma \quad (4.$$

and after the encounter the velocity of M_1 will be between the limits ξ_1' and $\xi_1' + d\xi$, &c., and that of M_2 between the limits ξ_2' and $\xi_2' + d\xi$, &c.

The differences of the limits of velocity are equal for both kinds of molecules, and both before and after the encounter.

When the state of motion of the system is in its permanent condition, as many pairs of molecules change their velocities from V_1, V_2 to V_1', V_2' as from V_1', V_2' to V_1, V_2, and the circumstances of the encounter in the one case are precisely similar to those in the second. Hence, omitting for the sake of brevity the quantities $d\xi$, &c., and ϕ, which are of the same value in the two cases, we find—

$$f_1(\xi_1, \eta_1, \zeta_1)\, f_2(\xi_2, \eta_2, \zeta_2) = f_1(\xi_1', \eta_1', \zeta_1')\, f_2(\xi_2', \eta_2', \zeta_2') \qquad (5)$$

writing—

$$\log f(\xi, \eta, \zeta) = F(MV^2, l, m, n) \qquad (6)$$

where l, m, n are the direction cosines of the velocity, V, of the molecule M.

Taking the logarithm of both sides of equation (5)—

$$F^1(M_1V_1^2 l_1 m_1 n_1) + F_2(M_2V_2^2 l_2 m_2 n_2) = F_1(M_1V_1'^2 l_1' m_1' n_1') + F_2(M_2V_2'^2 l_2' m_2' n_2') \qquad (7)$$

The only necessary relation between the variables before and after the encounter is—

$$M_1 V_1^2 + M_2 V_2^2 = M_1 V_1'^2 + M_2 V_2'^2 \qquad (8)$$

If the righthand side of the equations (7) and (8) are constant, the lefthand sides will also be constant; and since l_1 m_1 n_1 are independent of l_2 m_2 n_2 we must have—

$$F_1 = A M_1 V_1^2 \text{ and } F_2 = A M_2 V_2^2 \qquad (9)$$

where A is a quantity independent of the components of velocity, or—

$$f_1(\xi_1, \eta_1, \zeta_1) = C_1 e^{A M_1 V_1^2} \qquad (10)$$

$$f_2(\xi_2, \eta_2, \zeta_2) = C_2 e^{A M_2 V_2^2} \qquad (11)$$

This result as to the distribution of the velocities of the molecules at a given place is independent of the action of finite forces on the molecules during their encounter, for such forces do not affect the velocities during the infinitely short time of the encounter.

We may therefore write equation (1)

$$dN = C e^{AM(\xi^2 + \eta^2 + \zeta^2)}\,d\xi\,d\eta\,d\zeta\,dx\,dy\,dz \qquad (12)$$

where C is a function of $x\,y\,z$ which may be different for different kinds of molecules, while A is the same for every kind of molecule, though it may, for aught we know as yet, vary from one place to another.

Let us now suppose that the kind of molecules under consideration are acted on by a force whose potential is ψ. The variations of x, y, z arising from the motion of the molecules during a time δt are

$$\delta x = \xi\,\delta t,\ \delta y = \eta\,\delta t,\ \delta z = \zeta\,\delta t \qquad (13)$$

and those of ξ, η, ζ in the same time due to the action of the force, are

$$\delta\xi = -\frac{d\psi}{dx}\delta t,\ \delta\eta = -\frac{d\psi}{dy}\delta t,\ \delta\zeta = -\frac{d\psi}{dz}\delta t \qquad (14)$$

If we make

$$c = \log C \qquad (15$$

$$\log \frac{dN}{d\xi\,d\eta\,d\zeta\,dx\,dy\,dz} = c + AM(\xi^2 + \eta^2 + \zeta^2) \qquad (16)$$

The variation of this quantity due to the variations δx_1 δy_1 δz_1 $\delta\xi_1$ $\delta\eta_1$ $\delta\zeta_1$ is

$$\left.\begin{aligned} &\left(\xi\frac{dc}{dx} + \eta\frac{dc}{dy} + \zeta\frac{dc}{dz}\right)\delta t \\ -\,2AM&\left(\xi\frac{d\psi}{dx} + \eta\frac{d\psi}{dy} + \zeta\frac{d\psi}{dz}\right)\delta t \\ +\,M(\xi^2 + \eta^2 + \zeta^2)&\left(\xi\frac{dA}{dx} + \eta\frac{dA}{dy} + \zeta\frac{dA}{dz}\right)\delta t \end{aligned}\right\} \qquad (17)$$

Since the number of the molecules does not vary during their motion, this quantity is zero, whatever the values of ξ, η, ζ. Hence we have in virtue of the last term—

$$\frac{dA}{d} = 0 \quad \frac{dA}{dy} = 0 \quad \frac{dA}{dz} = 0 \qquad (18)$$

or A is constant throughout the whole region traversed by the molecules.

Next, comparing the first and second terms, we find

$$c = 2AM(\psi + B) \qquad (19)$$

We thus obtain as the complete form of dN

$$dN_1 = e^{(AM_1(\xi_1^2 + \eta_1^2 + \zeta_1^2 + 2\psi_1 + B_1)}\,dx\,dy\,dz\,d\xi\,d\eta\,d\zeta \qquad (20)$$

when M is an absolute constant, the same for every kind of molecule in the vessel, but B_1 belongs to the first kind only. To determine these constants, we must integrate this quantity with respect to the six variables, and equate the result to the number of molecules of the first kind. We must then, by integrating $dN_1\,\tfrac{1}{2}M_1(\xi_1^2 + \eta_1^2 + \zeta_1^2 + 2\psi_1)$ determine the whole energy of the system, and equate it to the original energy. We shall thus obtain a sufficient number of equations to determine the constant A, common to all the molecules, and B_1, B_2, &c. those belonging to each kind.

The quantity A is essentially negative. Its value determines that of the mean kinetic energy of all the molecules in a given place, which is $-\frac{3}{2}\frac{1}{A}$, and therefore, according to the kinetic theory, it also determines the temperature of the medium at that place. Hence, since A_1, in the permanent state of the system, is the same for every part of the system, it follows that the temperature is everywhere the same, whatever forces act upon the molecules

The number of molecules of the first kind in the element $dx\,dy\,dz$.

$$\left(-\frac{\pi}{A}\right)^{\frac{3}{2}} e^{AM_1(2\psi_1 + B)}\,dx\,dy\,dz \qquad (21)$$

The effect of the force whose potential is ψ_1 is therefore to cause the molecules of the first kind to accumulate in greater numbers in those parts of the vessel towards which the force acts, and the distribution of each different kind of molecules in the vessel is determined by the forces which act on them in the same way as if no other molecules were present. This agrees with Dalton's doctrine of the distribution of mixed gases.

J. CLERK-MAXWELL

ORIGINAL RESEARCH AS A MEANS OF EDUCATION*.

THE subject of the value of original scientific investigation may be considered from many points of view. Of these, that of the national importance of original research is the one which naturally first engages attention; and it does not take long to convince us that almost every great material advance in modern civilisation is due, not to the occurrence of haphazard or fortuitous circumstances, but to the long-continued and disinterested efforts of some man of science. Nor do I need to quote many examples to show us the immediate dependence of the national well-being and progress upon scientific discoveries thus patiently and quietly made. If it had not been for Black's researches on the latent heat of steam, James Watt's great discovery, which has revolutionised the world, would not have been made. Practical applications cannot be made until the scientific facts or principles upon which those applications rest have been discovered. In our own science I might instance hundreds of cases in which discoveries made in the pure spirit of scientific inquiry have (generally in the hands of others than the original investigators) led to results of the first importance to civilisation. Chloroform was first prepared by Liebig in 1834; but it was Simpson who long afterwards applied it to the relief of suffering humanity. Faraday in 1825 discovered benzole, and from it Zinin prepared a substance called aniline, which for many years remained a chemical curiosity only interesting to the scientific man. In due course, however, a practical sphere of usefulness was to be opened out for this little known substance. Perkin discovered that this rare body was capable of yielding splendid colours. Commercial skill then at once seized upon aniline, and, instead of its being made by the ounce, it is now manufactured by thousands of tons, and the bright and beautiful colours which it yields are known all the world over, and are alike pleasing to the eye of the connoisseur of fashion and of [illegible] dusky denizen of the forest primeval. Thus, too, the purely scientific researches of our distinguished fellow-citizen Dr. Schunck, respecting the dyeing principle contained in the well-known madder root, laid the foundations for the subsequent discovery, by Graebe and Lieberman, of the artificial production of this naturally occurring principle, termed alizarine, the manufacture of which is now assuming such gigantic proportions. Again, the discovery of chlorine by Scheele, in 1774, lies at the foundation of the whole of our Lancashire trade, for without bleaching powder the cotton and paper manufactures could not exist on their present extended scale. I might almost indefinitely extend this list of discoveries, which, when first made, were apparently far removed from any useful application, but which all at once become the starting-point of a new branch of industry, and a source of benefit or gratification to mankind.

This subject of the national importance of original research is one which is gradually but surely forcing itself on public attention. A few years ago national elementary education was looked upon as a chimera; now it has become the question of the day. As soon as English people see as clearly as we do the imperious necessity for encouraging, stimulating, and upholding original research as containing the seeds of our future position as a nation, they will not be behindhand in securing the free growth of those seeds. It is therefore the bounden duty of all those whose employment or disposition has led them to feel the truth of this great principle, to leave no stone unturned to make widely known and keenly felt the importance of the national encouragement of original investigation.

It might have been a useful task for me to contrast what is done in other countries for the encouragement of free inquiry and research, and what is done, or rather left undone, in England. We should have seen that on the Continent of Europe, to a great extent, and in the United States, in some measure, those who have to wield the sceptre of government are not only aware of the national importance of original research, but, what is more, that they act up to their convictions, whilst we feel that the same cannot be said in our country. We should have seen that in Germany the facilities given in the universities, which are Government institutions, and in the other numerous and well-organised scientific educational establishments, to original research are very great; that an original investigation in some branch of human knowledge is considered the usual termination of the student's university career; and that degrees are generally given only when some new observations or experiments have been added to the mass of human knowledge. We should find that the position of professor is mainly influenced by the amount and quality of his original researches, and that this power, and not any secondary or subsidiary ones, as is sometimes the case with us, is taken as the proof of a man's fitness to fill the professorial chair.

It is my wish, however engrossing this view of the subject may be, to ask you to consider to-day another aspect of the question—viz. the educational value of original research; the value of personal communication with nature for its own sake, the influence which such employment exerts on the mind, the effect which such studies produce as fitting men for the active duties of life, and the question, therefore, as to how far original investigation should be encouraged as an instrument of intellectual progress. It may be well, however, before we commence this special question, to place clearly before our minds what is meant by scientific inquiry in general, and to see how it is related to the studies and habits of mind with which men up to the dawn of the present, or scientific age, have been familiar.

In the first place, then, the essence of the scientific spirit is that it is free and disinterested. If, therefore, any of the habits of mind, studies, or beliefs in which men have hitherto indulged have not been free nor disinterested, in so far they have not been scientific. In the second place, the spirit of true scientific inquiry knows nothing of tradition or authority. It lays down laws for itself, and refuses to be bound by any others. Scientific education begins with no preconceived idea in accordance with which everything else must be moulded. It starts in simple communion with Nature, and is content to pick up little by little the truth which she is always ready to communicate to patient listeners. Thus step by step and generation by generation, slowly but surely, the perfect edifice of science is being built up, and all those who contribute, however insignificantly, to this great work have the safe assurance that their labour has not been in vain. This process is, it is clear, at once opposed to, and, if successfully carried out, subversive of the old order of things. Between a system based on authority and one founded on freedom of thought and opinion there can never be any united action; and whilst fully acknowledging that intellectual eminence, and, of course, moral excellence, are common to all classes of men, and are not confined to those holding particular opinions, if only they be honest, it is as well that we should admit with equal candour that the followers of the old system have no claim to be called scientific, and that there is, from the nature of things, a great and impassable gulf between us and them.

It does not concern us at present to inquire which of these two systems, the free or the authoritative, is for the future to rule the world. It must now suffice for us to see clearly that the habits of mind necessary for the establishment of the one are absolutely opposed to those needed for the success of the other.

I must, however, here not be misunderstood. It would ill become me, connected as I am with a college to which it has been our constant aim to impart a university character, to undervalue or depreciate the study of subjects other than those included under the head of the physical sciences. Literary studies, whether of modern or ancient authors, giving an acquaintance with the noblest thoughts and opinions of the great men of past ages; historical studies, giving us a knowledge of the acts of men in times gone by; the study of language and philology, as giving a knowledge of how men of all times and countries express their ideas and language; of logic, as pointing out the laws of thought; and above all, that of mathematics, are all matters of the highest importance, the neglect of which would render our education poor and incomplete indeed. The same rules, however, which all acknowledge to be necessary for the teaching of physical science must be applied to the study of all these subjects. In short, the *scientific method* must be employed in all cases and carried out to its fullest extent. Whilst attempts to shackle the mind, or to stifle free inquiry, which have too frequently succeeded in past times, and which may, if we are not on our guard, succeed again, must be repulsed with all our vigour.

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* Address by Prof. Roscoe at the opening of the new buildings of the Owens College Manchester.

I would, however, here wish to protest against the supposed materialistic tendency of scientific studies. It is true that certain opinions and professions of belief have been and will be shaken by studying the book of nature; it is also equally true that the study of nature does not and cannot interfere with the highest and noblest aspirations of the mind of man. In the investigations of every branch of science we come at last to a point at which further inquiry becomes impossible, and we are obliged to acknowledge our powerlessness and insignificance. We can see and learn concerning only the minutest fraction of the great whole of nature, and it is with this minute fraction alone that we as men of science are concerned.

In inaugurating, as we are now doing, a scientific department of an institution devoted to the higher education, it may be well to glance for a moment at the preliminary stages through which, in the subject of chemical science, with which alone I am competent to deal, a student must pass to reach the portal of original inquiry. And first let me gratefully acknowledge the help which we have received in endeavouring to find a habitation for a school of chemistry aspiring to be worthy of the intellectual vigour and manufacturing power of the great district of which this city is the centre—help not only of the necessary, and therefore valuable kind of pecuniary assistance generously and willingly given, but help of a personal, and therefore still more valuable kind, without which the funds would have been useless, and our scheme for the foundation of a really great scientific institution would have fallen to the ground. The results of this help you now see in this large theatre, and in the splendidly fitted laboratories behind it. They are, I say it with confidence, the most spacious and best arranged laboratories in Great Britain, and will be found, I believe, second to none in the world for convenience and suitability to their proposed uses. It now remains for my colleagues and myself to discharge our debt; to show that the confidence which has been placed in us has not been misplaced, and to prove year by year that the goods we furnish in the shape of soundly and scientifically educated chemists bring a return worthy of the capital, both in specie and intellect, which has been expended upon their production.

Our mode of instruction in the principles of chemistry is of two distinct kinds: (1), by lectures, accompanied by experimental illustration by the lecturers, as well as by recapitulatory and tutorial classes; and (2), by experimental work practically carried out by the student himself in the laboratory. Both of these means of obtaining command over the facts and principles of our science should be carried on simultaneously; the lectures serve as giving a general view of the main features of the subject; the laboratory work brings the student into direct contact with Nature, and gives him an insight into her processes, which can only thus be obtained. In the lecture room the student forms an idea, as in a panorama, of the general appearance of the country; but it is in the laboratory, as in a walk through a given district, that he first learns what the land he is travelling through is really like. And although we know that we must spend much time and labour if we go on foot, we know that we shall be rewarded by a vivid and lasting impression, and one which may perhaps give a new colour to our lives. It is thus with the study of chemistry; the laboratory is the place where the details of the science are really mastered; and a young man must not expect to become a competent chemist without having passed several years of hard and unremitting toil in solving the sometimes tedious and difficult problems which are presented to him.

It is not necessary for me here to detail to you the particulars of the course of instruction which all students of chemistry, as a rule, go through. Suffice it to say that this course begins at the very A B C of our subject; and, if I am freely to speak my mind, I would say that in general I do not object to take students who know nothing of the science. We first seek to give him some notion of the kind of phenomena with which the science is concerned; we then begin to train him in manipulative dexterity, and, by a graduated series of examples and exercises, make him acquainted with the fixed and exact quantitative laws upon which our science is founded. From the beginning we introduce a strict system of note-taking and of carrying out simple chemical calculations, so as to insure a firm foundation for the subsequent building. The student then begins to learn the properties of the more commonly occurring amongst the sixty-three elementary bodies of which (as far as we are yet aware) the material world is built up, and properties of their compounds. He commences the study of qualitative analysis, and at last he is able to tell you the nature of the exact constituents of any substance, whether of earth, of air, or of sea, of mineral, vegetable, or animal nature, which you may ask him to examine. He has accomplished a great work, and if he has carried his examinations as far as the reactions of the rare elements (as is usually the case with all our students), he is master of the first or qualitative stage of the science. Next the question arises as to the quantity of each constituent present in the given substance, and the second or quantitative stage is reached. This is necessarily a longer and more difficult matter than his preceding task. Not only must the choice of methods of separation and estimation be successful, so as to employ good ones and eschew the bad or inaccurate ones, but skill in manipulation must be forthcoming. All depends on accuracy and care in performing delicate operations, such as weighing, collecting and washing precipitates, and a hundred other manipulations, and the results of many days' work may be in a moment lost by one false step or one careless action.

In all this preliminary work the hand is gradually trained to perform the various mechanical operations, the eye is at the same time taught to observe with care, and the mind to draw the logical inferences from the observed phenomena. Habits of independent thought and ideas of free inquiry are thus at once inculcated; no authority besides that of the senses is appealed to, and no preconceived notions have to be obeyed; the student creates for himself his own material for observation, and draws his own conclusion therefrom. If he is inaccurate either in his manipulation, his observations, or in his conclusion, nature soon finds him out. Something or other is out of order, and he is sent back with the task of finding out his mistake for himself. Not until all this has been accomplished (and very often not then) is the student fit to think about original research. Before he can successfully grapple with new difficulties he must have learned to overcome the old ones. His hand must be dexterous and accustomed to meet all the mechanical difficulties which invariably accompany such investigations; his eye must not only be open to what he expects to see, but, what is far more difficult, it must quickly seize upon the occurrence of phenomena which he does *not* expect to see; his mind, working, perhaps, with a leading thought—for without this, original work is almost impossible—must be free in its power to grasp any new combination of ideas to which the phenomena may suddenly and unexpectually give rise, and be willing at once to relinquish a favourite and cherished hypothesis if the results of experiment prove that hypothesis to be erroneous. This dexterity of hand, quickness and keenness of sight, and pliability of mind must in greater or lesser degree be possessed by all who would undertake original scientific work. I do not mention as a preliminary necessity a competent theoretical knowledge of the phenomena and laws of our science, because, though this is a matter of course, many having this knowledge will altogether fail, owing to their not possessing the other requisites.

In carrying out, then, even the simplest original investigation, some or all of these requirements are needed. In addition, other faculties are called into play by the very fact of the phenomena being in part at least new. Not only do we ourselves not know what to expect, but nobody can tell us what will happen. We are exploring a new country, and our outlook must therefore be doubly sharp; we must be prepared for every possible event, and ready to meet every change of fortune. We must, like a traveller, not be discouraged by reverses, but patiently persevere in our course, feeling convinced that the path, which for a long time may be a thorny one, must in due course lead us to a point from which we shall enjoy an extended view of the surrounding country, and be able to trace the tortuous paths by which the elevation was reached. The faculties which are called into active operation in the prosecution of experimental scientific research are, in fact, exactly those which are valuable in the every-day occurrences of life, the proper employment of which leads to success in whatever channel they may happen to be directed. A man who has learnt how successfully to meet the difficulties and overcome the obstacles which occur in every experimental investigation, is able to grapple with difficulties and obstacles of a similar character with which he comes in contact in after-life.

(*To be continued.*)

CONDUCTING POWER FOR HEAT OF CERTAIN ROCKS*

A collection of more than twenty specimens of rocks of the best marked descriptions were chosen for the purpose, and were cut to a uniform shape and size by Messrs. Walker, Emley, and Beall, of Newcastle-on-Tyne, and a part of them were subjected to experiment. The plates are circular, 5 in. in diameter, and half-an-inch thick, and they are as smoothly and accurately ground to this uniform size as was possible in the case of some of the refractory substances as granite, whinstone, &c., that were employed. On the other hand, many more friable and softer rocks, as chalk, coal, marl, &c., are not included in the list of sample sections now collected.

The purpose of the present paper is simply to establish from the experiments the general *bad* conducting powers of the harder rocks, and to corroborate in the case of a few examples that were numerically reduced the conclusions of a similar kind that were obtained by Peclet.

The rock-plate to be tested is placed on a flat-topped tin boiler of its own diameter to raise its temperature on the underside to the boiling-point of water, while on its upper side a conical flat-bottomed tin flask of spring-cold water is placed, and absorbs the heat transmitted through the rock section from its heated side. A thermometer inserted through a cork in this flask marks the rise of temperature and the quantity of heat transmitted through the rock.

A small quantity of heat is also intercepted and absorbed by it which requires a part of the higher temperature on the heated side to introduce it into the rock, but this quantity is so small compared to the quantity which passes through it and enters the water, that it may easily be allowed for by a suitable correction.

The flask above the rock contained about ¼ lb. of water, and under the action of the steam heat below, it rose in temperature about 1° in 35 seconds for *slate*, and 1° F. in 38 or 40 seconds for different kinds of hard and close-grained rocks, as granite, serpentine, marble, and sandstone; while the time occupied for a similar rise in temperature was greatest in the case of a specimen of black shale from the coal-measures round Newcastle, when the thermometer rose 1° in 48 or 50 seconds, or *slower than* in the case of slate in the proportion of about 5 : 8.

In this series of trials it was easily supposed that the real temperature of the surfaces of the rock-plates was considerably different from those of the metallic surfaces in contact with them; and a thermo-electric pair of wires attached to cork-faces was now applied to test the real difference of temperatures of the two faces of the rocks. Two platinum wires were twisted on to the two ends of a piece of iron wire and were connected with the poles of a Thomson's reflective galvanometer. The iron wire itself was bent so as to bring its two twisted ends into contact with the opposite faces of the rock. On testing the thermo-electric arrangement by means of a double tin lid placed between its cork-faces, filled with water of different degrees of temperature on its two sides (which were measured by thermometers inserted in the lids), it was found that a difference of between 3° and 4° F. produced a deflection of 1 division of the galvanometer.

On now taking a plate of marble out of the heating vessel and placing it between the thermopyles, it was found that no sensible heat difference was recorded by it; the rock was reversed, top for bottom between its poles, and the effect was still insensible, although the heat of the finger pressing alone on one of the wire junctions moved the galvanometer 3° or 4°. In order to increase the temperature difference the rock-plate was then brought into contact with the metal surfaces by means of mercury; and the thermometric flask itself being filled with about 10 lbs. of mercury instead of ¼ lb. of water, it was found that the thermometer rose 1° in 10 seconds, corresponding to a transmission of 330 heat units per hour through a standard plate 1 in. thick, and 1 square foot in surface. When taken out of its cell and transferred to the galvanometer, the temperature difference was now found to be about 7°; giving the rate of conduction about 47 heat units per hour, instead of between 22 and 28 heat units as assigned by Peclet.

The process of lifting the rock out of its cell having undoubtedly produced a loss of the heat difference before the measurement was made; a new mode was now employed, and the wire junctions were pressed against the rock faces *in situ*, being at the same time protected from the heat of the boiler and thermometer plates facing opposite to them by thick felt wads upon which they were fastened to those plates. In this case a very different variation between the two rock-faces was now found the difference in the case of marble being 50° or thereabouts, while the passage of heat into the water thermometer flask was now about 264 heat units per hour, corresponding to a conducting power of about 5¼ heat units per hour. The same process was applied to two kinds of the black shale already described, and their conducting power was found to be much less than that of the fine-grained marble specimen, being at the rate of only 2 or 2½ heat units per hour. These quantities are not more than ⅛th or [illegible]th part of the values obtained by Peclet for the same kinds of rocks. Although time did not permit these experiments to be repeated with a different arrangement of the apparatus, when the sources of error peculiar to each of them would have been easily removed, as their origin in each case is easily explained, yet they confirm provisionally the values of the thermal conductivities found by Peclet; since in two experiments which certainly gave the values alternately in excess and defect, the quantities obtained varied from 5 or 7 to 42 or 4[illegible] heat units per hour for a kind of marble to which Peclet assigns 22 or 28 heat units per hour as its conducting power; and in the case of some other rocks of which Peclet describes the conductivity as about half that of the close-grained marble just mentioned, the values found by experiment also indicate a smaller thermal conductivity of these rocks in almost exactly the proportion which Peclet has assigned.

The form in which it will be desirable to repeat these experiments is one which will show the amount and kind of influence exercised by junctions between the surfaces of solid, liquid, and gaseous bodies in retarding the transmission of heat across them; as well as to conclude the actual thermal conductivities of the materials employed, and for this purpose a suitable modification of the apparatus and of the mode of conducting the experiments has been contrived, which it may be expected will fully effect the objects which it is thus intended to obtain.

THE DIVERTICULUM OF THE SMALL INTESTINE CONSIDERED AS A RUDIMENTARY STRUCTURE*

THE author took this structure as an illustration in reply to those who are not yet satisfied that structures exist which are useless to the animal body containing them. Referring first to the case of the appendix vermiformis of the great intestine, a survey of the anatomy of the cæcum in various animals, and of the stages of its development in man, leads to the inference that this worm-like appendage is a rudimentary and virtually a useless structure. It has, however, been generally supposed that, being present, it must have some function; and as it was manifest that a thing of this kind at the otherwise closed end of the great intestine is a source of danger by admitting foreign bodies which it could not expel, it has been argued that contrivances designed to avert this danger might be recognised. That it opens at the back instead of at the bottom of the cæcum; that its opening is oblique; that it has a kind of valve; that it is directed more or less upwards; and so on. On the contrary, the worm-like appendix is a vestige, the rudimentary representative of the true cæcum, and all these supposed contrivances by which the danger is lessened are simply the result of the forward and downward development of the great intestine away from the resisting wall of the abdominal cavity against which the appendix and back of the intestine lie. Although from this cause the appendix vermiformis is not nearly so dangerous a structure as it might have been, it is, notwithstanding, occasionally the cause of death. The author knew of several cases of this, and every experienced pathologist must have met with it. Foreign matters get impacted, causing ulceration, and perforation takes place, followed after a few hours by death.

The conclusion, however, that there are parts within the animal body which are useless, and worse than useless because dangerous, is so distasteful to the adherents of the extreme theological school that they will rather fall back on the bare possibility of some unknown function even for such a rudiment. The diverticulum of the small intestine may be employed here to complete the argument. Although in a classification of rudimentary structures they would be placed in different groups, the one being normal though often varying, the other only occasional, they are on the same footing for the purposes of this argument. It is known to be a vestige of the structure joining the intestine by which, at an early stage of the evolution of the animal frame, nourishment is introduced. All trace of it usually disappears, but occasionally part of it remains as a pouch opening from the small intestine. It has the usual coats of intestine, the inner coat presenting the same food-absorbing villi. It is therefore acting, but no one will argue that it is designed for use in those comparatively few persons who possess it. Unfortunately it is sometimes the cause of death. The author had met with cases of this, and it is well known to surgeons. It may be unable to expel its contents; or by adhering to a neighbouring part a noose is formed, a most dangerous condition, a sort of bowel-trap, through which a knuckle of intestine slips, and strangulation, followed by death, is the result. Here then we have an elaborate structure which is useless, or worse because dangerous. Were a railway contractor to leave open a siding which he had used in the construction of the line, the train might dash into it and a fatal accident result. This is exactly what is done when this diverticulum of the small intestine is left unclosed, and the fatal accident occasionally occurs. Were further illustration necessary we might refer to the fact of disease sometimes attacking that functionless structure the rudimentary breast in the male.

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* Paper read by A. S. Herschel, F.R.A.S., before the British Association, Bradford.

* Abstract of a paper read by Prof. Struthers, F.R.S.E., of Aberdeen, before the British Association, Bradford.

The consideration of such structures as the diverticulum may be said not to take us farther than to clear the ground, showing us that we have been on the wrong path. But a survey of rudimentary structures generally carries us farther. On the hypothesis of the independent origin of species they are unintelligible, while the hypothesis of evolution furnishes a clue to the whole. The facts of embryology, of palæontology, of rudimentary as well as developed structures are harmonised, and the whole present themselves as the result of the operation of a great law, the equivalent in the organic world of the law of gravitation in the inorganic. Although we do not as yet see so well how this biological law operates, the anatomist sees enough to make him feel that he is shut up to some form or other of the theory of evolution, and that the notion which we imbibed in our early years, and have long cherished, that so-called species arose independently of each other, must be a mistake.

The slow progress which this view has made in this country compared with Germany, the author attributed partly to the teleological bias which anatomy early received among us, but mainly to the fact that anatomy has been taught in the medical schools of this country for the most part as a mass of detail in its professional application, without reference to the ideas which it suggests when more widely and profoundly studied as a science.

SCIENTIFIC SERIALS

Ocean Highways, October.—The principal article in this month's number is one by Lieut. Salaverry, of the Peruvian Navy, on the "Navigation of the Upper Amazon and its Peruvian Tributaries," in which he gives some very interesting particulars of the measures that have been adopted by the Peruvian Government to open up and encourage the flow of commerce along the great fluvial highways which connect the rich provinces of the Andes with the Atlantic. The amount of work done by the Peruvian Government during the last few years in the exploration of the region with which the article is concerned is wonderful, and we are sure quite unknown even to many of those who take an interest in geographical discovery. Captain Davis contributes a second article on the *Challenger*, which is followed by one on the Pacific Railways of the South, *i.e.* the Southern United States. Two very interesting narratives are "A Visit to the Kuh-I Khwajah in Sistan," the place mentioned being a remarkable hill to the west of Naseribad, the chief city of Sistan; and "A Visit to Kuloja," by Mr. Ashton Wentworth Dilke, the plain of Kuloja being "a continuation of the Seven Rivers country running up between the Ala Tau and Thian-Shan Mountains."—Mr. E. G. Ravenstein contributes a paper on "Elmina, and the Dutch Gold Coast;" which is followed by an article on the *Polaris*, the usual reviews, proceedings of societies, &c. There are Maps of the former Dutch Possessions on the Gold Coast, of the Amazonas in Peru, of the Pacific Railways of the South, and a Chart of the *Challenger's* course to the Cape de Verde Islands.

Bulletin de la Société Impériale des Naturalistes de Moscou, No. 3, 1872.—In a paper on tantalum, in this number, M. Herman describes five different combinations of the metal with oxygen, two only having been hitherto known.—There are several zoological and botanical lists,—M. Becker gives an account of beetles and flies met with on a journey to the Astrachan region; Mr. M'Lachlan gives drawings of some new species of Phryganides, and a Chrysopa, found in Finland and the Caucasus; M. Hochhuth enumerates the beetles of Kien and Volhynien, &c., while M. Lindemann furnishes a report on the formation of his herbarium.—M. Lubimoff's paper on a new theory of the field of vision and magnification of optical instruments, has been elsewhere noticed in our columns.

No. 4 (1872) commences with an interesting article, with illustrations, by M. Mayewski, on evolution of the barbules of *Begonia manicata*, showing the various stages from that of simple hairs consisting only of epidermic cells.—Some strictures on M. Lubimoff's views as to the field of vision are offered by M. Bredichin, who thinks the theory neither new nor exact.—M. Hochhuth continues his list of beetles (as also in the following number), and M. Kryloff describes some geological formations in the Government of Kostroma.—Dr. Dreschler communicates an account of a collection of mathematical and physical apparatus in Dresden: and the number concludes with a table of meteorological observation in Moscow, in 1872.

SOCIETIES AND ACADEMIES

LONDON

Royal Horticultural Society, Sept. 17.—General Meeting.—Mr. Henry Little in the chair.—The Rev. M. J. Berkeley called attention to some pears, part of which were cracked and small, while the rest were perfect. They had been taken from opposite sides of the same tree, and the difference was probably caused by injury from wind when in a young state.—Mr. Bull exhibited for the first time *Odontoglossum Roezlii*, a near ally of *O. vexillarium*, and which Prof. Reichenbach suggests may be a hybrid between that species and *O. Phalænopsis*.

Oct. 1.—General Meeting.—Mr. Henry Little in the chair.—The Rev. M. J. Berkeley alluded to the numerous interesting and rare species of fungi which were exhibited. *Paxillus atro-tomentosus*, sent by the Rev. W. W. Newbould from Woburn; *Russula aurata*, by Miss Hubbard, from Horsham; *Hydnum squamosum*, new to Britain, from Somerset, by Mr. Aubrey Clark; *Cortinarius orellanus*, also new to Britain, from Epping Forest, by W. G. Smith, &c. Mr. Berkeley also referred to Schwendener's theory as to the nature of lichens. Bornet had recently published an admirable paper in support of the same views. He himself, however, was not convinced of their correctness. On the contrary he believed he had seen the gonidia of *Parmelia* originating from hyphæ within the cells of some drift-wood from the Arctic regions. He also read a letter from Dr. Thwaites, of Ceylon, who thought that the symmetrical growth of the lichens was an argument against one portion being parasitic on the other.

PHILADELPHIA

Academy of Natural Sciences, June 10.—Dr. Ruschenberger, president, in the chair.—Mr. Gentry made the following remarks:—At the last meeting of the Academy, Mr. Meehan made some observations upon the peculiar structure of the flowers of *Pedicularis canadensis*, observing that he had vainly watched them during two seasons with the view of determining the manner in which they were fertilised. He further said that he had noticed that they received the attention of a species of humble-bee, for the sake of their honey, which, in order to accomplish its purpose, always bored a hole into the side of the tube. On Wednesday morning last, I visited a spot where the plants were growing luxuriantly, affording an interesting field for observation. It was not long before I observed a *Bombus terrestris* to alight upon the outer side of the tube of a flower, at a distance of three feet from me. At this distance it did seem as if the bee in order to obtain the honey which it secretes, produced a slit into the tube, as Mr. Meehan observed. But the movements of the bee being so quick, and the distance too great to judge accurately, I approached the insect by degrees, until I was within three inches of it, when the whole process became apparent. The bee, however, was so intent upon its labours, as not to take any notice of me. The flower is composed of an erect tube, with a natural cleft running along its lateral walls from above, through one-third its entire length, presenting outwardly apparently a mere crease, from the manner in which the compressed margins of the upper lip fit into the rolled-in edges of the lateral lobes of the under lip. The upper lip is compressed, arched, and beaked, presenting an aperture at the apex, through which passes a curved pistil, the lower lip is reflexed, consisting of three lobes, one median and two lateral, assuming a platform arrangement. Enclosed within the upper lip are four stamens, didynamous, with their anthers turning backwards, facing each other vertically. When ripe these anthers split upon the inner side, thus giving a fancied resemblance to an oval snuff-box thrown backwards upon its hinges. Each cell is filled with white pollen grains. Now when the bee alights upon the tube, by means of its trunk, it opens the natural cleft above alluded to, and having thus gained partial entrance, it would defeat its intention did not the length of the flower's tube when contrasted with that of the bee's trunk, necessitate the admission of the entire head also. In this operation the lips of the flower are pressed apart, the margins of the upper lip are separated to receive the head, and the pollen grains, already ripe, by the considerable motion to which they are subjected, become dislodged from their cells, and fall down in a dense shower upon the bee's back and head. Having obtained the coveted sweet, it flies to another flower upon a different stalk, as I observed in a score of cases during two days; but before renewing the preceding operations, stations itself awhile upon the lower lip, its head coming in contact with the stigma of the pistil. Then, by means of the hairs that line the inner side of the tarsus of each interior leg, and the constant rubbing together of the parts comprising its trophi or its instrumenta cibaria the attached pollen grains are sent flying in every direction, sure to adhere to the stigma. Whilst observing the above process, I also noticed that after the lips had been pressed apart, and were permitted to regain their position, the upper lip, being somewhat elastic, sprang back to its place with considerable force, sending through the aperture, through which passes the pistil, a complete cloud of pollen, enveloping the stigma upon every side. This operation can be performed artificially, by taking hold of the under lip with the left thumb and fore-finger, and pulling the upper lip backward, by the right, and then releasing the hold of the latter: the upper lip springs to its place, spirting the pollen through the aperture upon the left hand. From the above it is to be seen, that the plant has two chances of being fertilised—one by its own pollen, and the other by that of another. Although the flower seeds abundantly, yet I am disposed to think that it is mainly through the pollen of another that the seeds become perfect. I incline to this opinion because, in an examination of many pods, I noticed that a few seeds were found in a rudimentary condition, apparently manifesting a tendency to abort, while the majority were in a vigorous condition; the former, doubtless, being the effects of self-fertilisation in part, which, as is well-known, is a degenerating process. I desire also to call attention to an interesting discovery which I was enabled to make recently, whilst engaged in an examination of a double flower of *Ranunculus fascicularis*. In the genus Ranunculus, the corolla of a normal flower is made up of five petals, each of which on the inner side of its basal part is usually provided with a scale. This scale from its position is denominated the *nectariferous scale*. In the specimen under consideration three of these scales had assumed the character of petals, agreeing with the flower's true petals in every particular except size, being but three-fourths the dimension of the latter. It very frequently happens that we find, in examining flowers, parts which we can refer to no organ with which we have become acquainted. They appear to be distinct from any of the whorls which make up a perfect flower, although located among them and attached perhaps to them. All such parts are designated as appendages. Under this category are placed the scales that are characteristic of some species of Crowfoot. Prof. Lindley thinks that these small appendages are barren stamens united to the bases of the petals. This opinion I think is a just one. From the facts here indicated it is reasonable to conclude, that the double flowers of the Ranunculus do not always originate by true staminal metamorphosis, but sometimes by scale transformation; also that nectariferous scales when they exist are barren stamens, which favourable conditions may develop into true petals. Whilst examining several specimens of *Potentilla canadensis* lately, I was struck with the variableness displayed in the number of segments which constituted their outer or calycine whorls. This series in *Potentilla*, as is well known, consists of five sepals, with as many intermediate bractlets. In the specimens to which I refer, I counted from seven to ten bractlets. This numerical variation I am confident results from the splitting, so to speak, of some or all of the primary bractlets, as specimens were observed which exhibited all the transitional forms, from a slight indentation at the apex to partial and complete division.

PARIS

Academy of Sciences, Oct. 13.—M. de Quatrefages, president, in the chair.—The president announced the death of M. Antoine Passy.—The following papers were read:—On crystalline dissociation, by MM. Favre and Valson. This portion of the author's researches deals with the estimation of the work done in saline solutions. Tables of the value of this work were given.—Researches on the ancient fauna of the Island of Rodriguez, by M. Alph. Milne-Edwards.—Verification of Huyghens's law by means of the prismatic method, by M. Abria.—Monograph on the fishes of the family of the *Symbranchidæ*, by M. C. Dareste.—On a mechanical purifier for illuminating gas which will also serve to mix vapour with the gas, by M. D. Colladon.—Researches on the action of the so-called antiseptics on carbuncular virus, by M. C. Davaine. The author found the bodies in question were, as a rule, effective in destroying the virus.—Studies on the *Phylloxera*, continuation by M. Max Cornu.—On the oak *Phylloxera*, by M. Balbani.—Note on a new method of tempering steel, by M. H. Caron. The method consists in quenching the heated steel in heated water, the temperature varying with the size of the article. The author stated that this method augmented the elasticity considerably without altering the softness of the metal.—On the use of potassic disulphate as a means of detecting galena, by M. Jannettaz.—Meteorological observations made in a balloon, by M. G. Tissandier.—New remarks on the epidemic goitre of the St. Etienne barracks, by M. Bergeret.

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ERRATUM.—Vol. viii. p. 519, 2nd col. last line, for "tonic" read "toxic"

THURSDAY, OCTOBER 30, 1873

OUR NATIONAL MUSEUMS

WE may congratulate ourselves that the Museum question is now being taken up with vigour. Not only must the Royal Commission on Science include it among their inquiries, but the Society of Arts is directing public attention to it.

This is the more opportune, as the intention of the Government to transfer to irresponsible trustees the only museums under the direction of a Minister of State has recently been declared, and needs only to be declared to be condemned on all hands.

We now let the following documents speak for themselves. Next week we shall return to the subject:—

I.

Memorial to the Right Honourable W. E. Gladstone, M.P.

"1. We, the undersigned members of the Council and Members of the Society for the Encouragement of Arts, Manufactures, and Commerce, request the attention of Her Majesty's Government to the remarkable proof of the public desire for instruction and pure enjoyment afforded by the examination of works of Art and Science, which has been shown by the opening of the Bethnal Green Museum.

"2. This Museum, established in one of the poorest and busiest districts in London, where men, women, and children are most laboriously employed, has been frequented during three months by more than 700,000 visitors,* a number which probably exceeds that of the visitors to all the other metropolitan museums and galleries during the same period.

"3. The undersigned submit that this museum could never have come into useful existence, and have been instrumental in conferring great benefits on the people, without the aid of Parliament; and they desire to press this fact upon the consideration of Her Majesty's Government, with the hope that they will submit to Parliament the policy so essentially national of voting increased means to facilitate the establishment of museums, libraries, and galleries of Science and Art in large centres of population, wherever such localities are willing to bear their share in the cost."

Appended to this are the signatures of 250 Peers, Members of Parliament, and other well-known and distinguished men.

II.

CORRESPONDENCE RELATING TO THE ABOVE MEMORIAL.

"*The Secretary of the Society of Arts to the Right Hon. W. E. Gladstone, M.P.*

"July 3, 1873.

"SIR,—A memorial relative to the beneficial action of the Bethnal Green Museum, has been prepared by the Society of Arts for presentation to you.

"It has been signed by one hundred and fifty members of the Council and of the Society, of whom twenty-two are peers, and sixty-three are members of the House of Commons. In addition to the above, thirty-seven peers and sixty-three members of the House of Commons, not members of the Society, have expressed their concurrence in the object of the memorial.

"I am directed to request that you will have the kindness to receive a deputation to present the memorial, and to name a day for doing so, giving, if possible, at least a week's notice."

"*Mr. Gurdon to the Secretary of the Society of Arts.*

"July 5, 1873.

"SIR,—Mr. Gladstone desires me to acknowledge the receipt of your letter of the 3rd inst., requesting him to receive a deputation to present the memorial from the Society of Arts, on the subject of the museum at Bethnal Green. I am directed to express Mr. Gladstone's sincere regret that the pressure of his duties, as First Lord of the Treasury, renders it absolutely necessary that he should confine his attention to those matters which fall directly within his province; and he therefore trusts that those on whose behalf you have written will kindly excuse him if he asks them to address themselves to the Privy Council Office."

"*The Secretary of the Society of Arts to Mr. Gurdon.*

"SIR,—I have brought before the Council your letter of the 5th July, in reply to mine of the 3rd July, asking Mr. Gladstone to receive a deputation to present a memorial from this Society on the subject of the Bethnal Green Museum. The Council observe that you express Mr. Gladstone's regret that the pressure of his duties as First Lord of the Treasury renders it absolutely necessary that he should confine his attention to those matters which fall directly under his province, and his trust that those on whose behalf the reception of a deputation was sought will kindly excuse him if he asks them to address themselves to the Privy Council Office.

I am directed, in reply, to point out that the memorial, having relation to a subject of vast importance to the education, general cultivation, and social welfare of the people, did appear to the Council to bring the subject strictly within the consideration of the Prime Minister, rather than of a department of the Government. Moreover, it did appear to the Council that the deep interest which the subject excites is manifested by the unusual character of the signatures, being those of 60 peers and 130 members of the House of Commons attached to the memorial, and justified the Council in asking for the special attention of Mr. Gladstone himself.

"Under these circumstances, the Council submit their conviction that the subject involves considerations of principle and policy worthy the attention of the Prime Minister of this country, and too wide in its political and fiscal considerations to be dealt with effectually by any single department of the Government.

"They, therefore, respectfully decline to adopt Mr. Gladstone's suggestion that they should address themselves to the Privy Council Office."

"*Mr. Gurdon to the Secretary of the Society of Arts.*

"SIR,—I am directed by Mr. Gladstone to acknowledge the receipt of your letter of the 18th inst., and to request that you will be kind enough to acquaint the Council of the Society of Arts that the intention of the reply to your communication of July 3 was to point out that, in regard to a subject of the nature of that which you brought before him (viz. the beneficial action of the Bethnal Green Museum), which falls properly within the province of a department of the State appointed to deal with it, the First Lord of the Treasury could not take the initiative out of the hands of that Department.

"This Mr. Gladstone would be doing were he to receive the proposed deputation; and he would be acting contrary to the rules of administration which are necessary for the conduct of public business.

"If, however, the Society of Arts think fit to favour him with a written communication, Mr. Gladstone will himself correspond with the proper department concerning it."

"*The Secretary of the Society of Arts to the Right Hon. W. E. Gladstone, M.P.*

"SIR,—The Council of the Society of Arts have directed me to reply to Mr. Gurdon's letter of July 22, in which he states that, 'if the Society of Arts think fit to place before you a written communication, you would yourself correspond with the proper department concerning it.'

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* "The numbers up to the end of September were upwards of one million and a half of people; to 31st December, 1872, only six months and a half, they amounted to 901,464, whilst the number of the general visitors to the British Museum for the whole year 1872 were only 424,068.—October 1873.

"The deputation which desired to have the honour of waiting on you, and explaining in detail the objects of the memorial, would have stated that, in their view, the experiment of the Bethnal Green Museum is suggestive of the following points :—

"1. That a general popular desire exists for such museums, and that it would be good national policy for the Government to encourage the establishment of them.

"2. That like primary elementary schools, it would be impossible that such museums could, without State aid and inspection, become part of a national system, aiding technical instruction and secondary education.

"3. That this question, unfettered by any denominational difficulties, is quite ripe for solution; that the necessary expenditure for aiding museums of science and art would be advantageous from every point of view, even remunerative as respects commerce; and, further, would be auxiliary in promoting morality and social good order.

"4. That such museums are absolutely necessary to the industrial progress of the country, which is behind other countries already in the possession of them.

"5. That the time has come when it is necessary that all public museums and galleries of works of science and art receiving Parliamentary aid, should be brought under an intelligible system of administration, controlled by a responsible Minister of State, so as to render them auxiliary to the development of local museums and galleries.

"The Council submit that these are subjects not only of general policy, but involve some new principles of administration, large financial considerations, the reform of old institutions, &c., which it is the province of the general Government, and not of any single department, to deal with. The Council especially desired that the answer they might receive should come direct from yourself as Prime Minister. They could not hide from themselves the knowledge they possessed of the several departmental difficulties which attended the opening of the Bethnal Green Museum, and that they had been made cognizant, through Parliamentary returns and the revised estimates for 1871-2, of the opposition which the Treasury. as lately administered, had persistently offered to carry into effect the decisions made by Her Majesty's Government in 1866, for conducting the Bethnal Green Museum.

"The Council respectfully request you to have the kindness to bring this memorial before Her Majesty's Government. They hope it will meet with favourable consideration, and lead to decisive action; and they will feel obliged by receiving an answer upon it at as early a period as convenient."

"*Mr. Gurdon to the Secretary of the Society of Arts.*

"SIR—Mr. Gladstone desires me to acknowledge the receipt of your letter of October 6th, the contents of which he will not fail to make known to his colleagues."

III.

Resolutions of the Council of the Society of Arts passed at their last Meeting :—

"1. That the undermentioned persons be invited to serve on a Standing Committee for the purpose of bringing under parliamentary responsibility the National Museums and Galleries, so as to extend their benefits to Local Museums, and to make them bear on public Education. The following are the several objects in view for effecting this purpose :—

"2. All Museums and Galleries supported or subsidised by Parliament to be made conducive to the advancement of Education and Technical Instruction to the fullest extent, and be made to extend their advantages to the promotion of original investigations and works in Science and Art.

"3. To extend the benefits of National Museums and Galleries to Local Museums of Science and Art which may desire to be in connection, and to assist them with loans of objects.

"4. To induce Parliament to grant sufficient funds to enable such objects to be systematically collected, especially in view of making such loans.

"5. For carrying out these objects most efficiently, to cause all National Museums and Galleries to be placed under the authority of a Minister of the Crown, being a member of the Cabinet, with direct responsibility to Parliament; thereby rendering unnecessary, for the purpose of executive administration, all unpaid and irresponsible trustees, except those who are trustees under bequests or deeds, who might continue to have the full powers of their trusts, but should not be charged with the expenditure of Parliamentary votes.

"6. To enter into correspondence with all existing Local Museums and the numerous Schools of Science and Art, including Music, now formed throughout the United Kingdom, and to publish suggestions for the establishment of Local Museums.

"7. Also, to cause the Public Libraries and Museums Act (18 and 19 Vic. c. lxx.) to be enlarged, in order to give local authorities increased powers of acting.

"Earl of Carnarvon.
Earl Russell.
Lord Elcho, M.P.
Lord George Hamilton, M.P.
Lord Houghton.
Lord Lyttelton.
Sir T. Acland, Bart. M.P.
Sir Antonio Brady.
Sir John Lubbock, Bart, M.P.
Right Hon. Sir Stafford Northcote, Bart., C.B., M.P.
Sir Wm. Thomson, F.R.S.
Sir S. Waterlow, Bart., Lord Mayor of London.
Sir Joseph Whitworth, Bart.
Right Hon. Sir John Pakington, Bart., M.P.
Right Hon. W. J. Henley, M.P.
Right Hon. Cowper Temple, M.P.
The Hon. Mr. Justice Grove.
Thomas Ashton, (Manchester).
E. A. Bowring, M.P.
Dr. Carpenter, F.R.S.
Henry Cole, C.B.
Montague Corry,
W. De La Rue, F.R.S.
E. B. Eastwick, M.P.
Gabriel Goldney, M.P.
Principal Greenwood (of Owens Coll., Manchester).
John Henderson, M.P.
Dr. Hooker, F.R.S.
C. Wren Hoskyns, M.P.
James Howard, M.P.
Prof. Huxley, F.R.S.
U. J. Kay-Shuttleworth, M.P.
George Melly, M.P.
S. Morley, M.P.
Dr. Mouat.
A. J. Mundella, M.P.
Prof. Roscoe, F.R.S. (of Owens College, Manchester).
Lyon Playfair, C.B., M.P.
Hodgson Pratt.
Prof. Ramsay, F.R.S.
C. Seely, jun. M.P.
Col. Strange, F.R.S.
E. Thomas, F.R.S. (Athenæum Club).
George Trevelyan, M.P.
Thomas Twining.
Prof. Tyndall, F.R.S.
G. W. Ward (Nottingham).
Prof. Williamson, F.R.S.
Also the Heads of the City Companies for the time being.
Also the Chairmen of Local Committees of Schools of Science and Art, and of Local Museums Committees.
Also the members of the Legislature who signed the Bethnal Green Memorial.

(By order) "P. LE NEVE FOSTER,
Secretary."

SPENCER'S DESCRIPTIVE SOCIOLOGY

Descriptive Sociology; or, Groups of Sociological Facts. Classified and arranged by Herbert Spencer. No. I. —English; compiled and abstracted by James Collier. (London: Williams & Norgate.)

NOT long since, an announcement appeared in NATURE of Mr. Herbert Spencer's plan of publishing, not a complete and finished treatise on Sociology, but a collection of classified materials for the use of students and investigators. The origin of this important work is explained in the following extract from the preface to Part I., which has now appeared.

"In preparation for the *Principles of Sociology*, requiring as bases of induction large accumulations of data, fitly arranged for comparison, I, some five years ago, commenced by-proxy the collection and organisation of facts presented by societies of different types, past and present: being fortunate enough to secure the services of gentlemen competent to carry on the process in the way I wished. Though this classified compilation of materials was entered upon solely to facilitate my own work; yet, after having brought the mode of classification to a satisfactory form, and after having had some of the tables filled up, I decided to have the undertaking executed with a view to publication; the facts collected and arranged for easy reference and convenient study of their relations, being so presented, apart from hypotheses, as to aid all students of Social Science in testing such conclusions as they have drawn, and in drawing others"

An objection to this scheme, which struck most who noticed its announcement, was that materials thus arranged would form a patch-work of dead scraps, rather than an organic whole. The specimen which was first circulated, relating to one of the barbaric grades of culture, confirmed this unfavourable expectation. Now, however, that a section of the actual work has been published, it is evident that the scheme can be made to carry an interest of its own, and even to serve an educational purpose. This first section is a methodical summary of the development of England, intellectual and moral, from the beginning of its history in Cæsar's time, to about A.D. 1850. At the first glance, it suggests a question which may disconcert not a few of the lecturers and tutors engaged in training students in history at our Universities. This question is, whether the ethnological record of national life ought any longer to be treated as subordinate to the political record of the succession of rulers and the struggles for supremacy of ruling families, or whether the condition of society at its successive periods is for the future to be considered as the main subject, only marked out chronologically by reigns, battles, and treaties. This question has, it is true, been already raised. It is, in fact, the issue between historical chronicle and the philosophy of history as rival subjects of study. But Mr. Spencer's work brings it more clearly and practically into view than any previous one, as will be seen from the following outline of his scheme. It consists of two parts.

The first part is a series of tables, arranged in thirty to thirty-five columns, each with a heading of some department of social life or history, which again are combined into groups. Thus the group of columns relating to the structure of society takes in political, ecclesiastical, and ceremonial departments, under which again we find separately given the laws of marriage and inheritance, the regulation of tribes and castes, the military and ecclesiastical organisation, and the ceremonies and customs of daily life. Next, the group of columns devoted to the functions of society, regulative and operative, contains particulars of the morals, religion, and knowledge of each age, the state of language, and the details of industry, commerce, habitations, food, clothing, and artistic products. Three special columns at the beginning, middle, and end of this long colonnade, contain the skeleton of ordinary history: namely, the principal dates, names of rulers, and political events. Thus, by glancing across any one of the huge double pages, we see the whole condition of England at any selected period. Thus, in the century after the Norman Conquest, the influence of the invaders is observed in the growth of architecture, painting, music, poetry, the introduction of new food and more luxurious living, the importation of canonical law and of mathematics from the East, and so on through all the manifold elements which made up the life of noble and villain in our land. If the page be turned to the 16th century, the picture of English life is not less distinct. The scholastic philosophy is dying out, men's minds are newly set to work by the classical revival, by voyages into new regions, the growth of mercantile adventure and political speculation; chivalry ceases, archery declines; judicial torture is introduced, the "Italian" crime of poisoning becomes frequent; the ancient belief in witchcraft and pervading demons holds its ground, as do the miracle-plays and local festivals; but a highway act is passed, new roads are being made, the new houses have chimneys, their furniture and fare become more luxurious; the power of the old feudal families is destroyed, the Star-Chamber is new-modelled; church-fasts are still observed under pain of imprisonment, and high offices of state are still in the hands of churchmen, but among the signs of momentous change come the dissolution of monasteries, and the distinct appearance of a sect of Protestants. Thus the tabulated record goes on till it ends near the present day, among such items as Trades'-Unions, Divorce Courts, the Manchester School, County Courts, Free Thought, Railways, Rifled Cannon, Præ-Raphaelitism, Chartism, Papal Aggression, and the crowding events of modern manufacture and science.

It is by following the several columns downwards, that the principle of Evolution, the real key to Mr. Spencer's scheme, is brought out into the broadest light. It seems most strange, however, that he should not have placed in its proper niche the evidence of pre-historic archæology. Mr. Spencer can hardly doubt that the stone implements found in England prove the existence of one, or probably two, stone-age populations before the Kelts, who, under the name of Ancient Britons, begin his series. If he acknowledges this, why should a first link so important in his chain of evolution have been dropped? Otherwise the chain is carefully stretched out so as to display it from end to end. In many matters simple and direct progress is the rule. From the Ancient Briton's bow with its bronze-tipped arrows, to the cross-bow, the match-lock-gun, and thence through successive stages to the rifled breech-loader; from the rude arithmetic before the introduction of the "Arabic" numerals, through the long series of importations and discoveries which led to the infinitesimal calculus in its highest modern development from the early English astronomy, where there was still a solid firmament studded with stars, and revolving on the poles about the central earth, to the period when the perturbations of planets are calculated on the theory of gravitation, and the constitution of the fixed stars examined by the spectroscope—these are among the multitude of cases illustrating the development of culture in its straightforward course. Harder problems come before us, where we see some institution arise, flourish, and decline within a limited period, as though resulting from a temporary combination of social forces, or answering only a temporary purpose in civilisation.

To take an instance from Mr. Spencer's Table, English history has seen the judicial duel brought in at the Conquest, flourishing for centuries, declining for centuries more, till its last formal relic was abolished in 1820. Again, in the Old English period, marriage appears as a purely civil contract, on the basis of purchase of the wife; then with Christianity comes in the religious sanction, which by 1076 had become so absolute that secular marriages were prohibited: with a strong turn of the tide of public opinion, the English Marriage Act of 1653 treated marriage as a civil contract, to be solemnised before a justice of the peace; till after a series of actions and re-actions, in our own day the civil and ecclesiastical solemnisation stand on an equal footing before the law. Closely similar has been the course of English society on the larger question of a National Church, which, soon after the introduction of Christianity, claimed an all but absolute conformity throughout the nation, practically maintained the claim for ages, and then was forced back to toleration, which has at last left it with a supremacy little more than nominal. This is not the place to discuss these subjects for themselves, but to show how the table before us, by its mere statement of classified events in chronological order, must force even the unwilling student to recognise processes of evolution in every department of social life. The writer of the present notice once asked an eminent English historian, a scholar to whom the records of mediæval politics are as familiar as our daily newspaper is to us, whether he believed in the existence of what is called the philosophy of history. The historian avowed his profound distrust of, and almost disbelief in, any such philosophy. Now it may seem a simple matter to have tabulated the main phenomena of English social and political history in parallel columns, as Mr. Collier has here done under Mr. Spencer's direction, but his tables are a sufficient answer to all disbelievers in the possibility of a science of history. Where the chronicle of individual lives often perplexes and mystifies the scholar, the generalisation of social principles from the chronicler's materials shows an order of human affairs where cause and effect take their inevitable course, as in Physics or Biology.

It may be objected, however, that summing up complex events in short headings, and arranging these in columns, is a rough and ready method often leading to erroneous inferences, and even liable to gross error. It is evidently in order to guard against this that Mr. Spencer follows the first part of his scheme by a second. Here, under their proper headings, the passages from standard authorities which vouch for the brief statements in the tables are given in full, and with references. This part of the work, much the largest in extent, is thus an elaborate historical commonplace-book, containing some thousands of selected quotations. Mr. Collier is on the whole to be congratulated on the completeness of his reading, and the discrimination with which he has chosen his passages. So much information, encumbered with so little rubbish, has never before been brought to bear on the development of English institutions. There is hardly a living student but will gain something by looking through the compilation which relates to his own special subject, whether this be law or morals, education or theology, the division of labour or the rise of modern scientific ideas. Of course it is very far from perfect. There are some actual blunders; a weak authority is often taken where a strong one was to be had; small matters are often put in, and large ones left out; the want of notes leaves no opportunity of correcting an author's half-true statement. Thus under the heading of Accessory Institutions, there is a good account of the Royal Institution and the Pharmaceutical Society, and a mention of the Russell Institution and the Swedenborg Society, but not a word of the Royal Society. An extract from the Pictorial History of England ascribes the system of Sunday Schools to Mr. Robert Raikes, of the *Gloucester Journal*, about 1780, whereas their real inventor, Jonas Hanway, flourished at an earlier date. Again, under the heading of Religious Ideas and Superstitions, various slips are to be noticed. It was natural enough that, years ago, Brand should, in his Antiquities, have considered the country rite of throwing toast to the apple-trees to secure a fruitful year as being a "relic of the heathen sacrifice to Pomona;" but a modern reader quoting him, should never in Brand's old-fashioned way have dragged in a Roman deity to account for a genuine English superstition. Just below this is the following sentence in brackets, and without an author's name:—"The resistance of tides in the Wash caused by their meeting with the ebb-waters is called the Ægar—one of the gods of the Scandinavian mythology." This statement is misleading, and not the less so for having a real etymology hidden behind it. Our English word *eagre*, signifying the "bore" of an estuary, is Anglo-Saxon *eagor*, the sea, and its use merely asserts the plain fact that the sea runs up the channel. It is true that there is a corresponding old Norse word *ægir*, the sea, and that this in Scandinavian mythology becomes the personal name of *Œgir*, the Sea-god. But it does not follow that our eastern counties' word had ever any such mythological notion attached to it. These happen to be the first weak points which struck the writer in glancing over a page or two in quest of errors. It is needless to continue this critical process on a professed book of extracts; enough has been done to show that the proper use of such a work as the present is not so much to furnish the scholar with complete second-hand ideas, as to indicate how the ideas lie and where they may be obtained first-hand.

Mr. Spencer, out of the evidence amassed by the readers collecting facts under his direction, might have made an admirable treatise of the usual kind on the History of English Civilisation. No doubt, however, for years to come lectures will be delivered and articles written full of suggestive facts in the history of culture, which the initiated will recognise as borrowed from the unwieldy pages of this present atlas-like compilation. In the meantime, we may hope that Mr. Spencer's scheme may be carried out through the whole range of savage and civilised life, and that his tables of development of culture (printed on one side of the paper as if in anticipation of such use), may be set up like maps on the walls of class-rooms. They are certainly to be compared with maps for the range and precision and correlation of parts with which they show their contents at a glance.

E. B. TYLOR

OUR BOOK SHELF

Aus der Urzeit. Bilder aus der Schopfungsgeschichte, von Prof. Dr. Karl A. Zittel, in Munchen. Mit 78 Halzschnitten. (München: Rudolph Oldenbourg, 1871-2.)

THIS is one of a series of popular works on Science entitled "Die Naturkrafte," that are being published at intervals by Herr Oldenbourg, of Munich. Prof. Zittel, in his preface to the present work, speaks of the vast influence which popular scientific literature is calculated to have upon the entire development of a people, and therefore insists on the great importance of diffusing, in an intelligible manner, among the people thoroughly correct notions of every science, instead of mincing down scientific truths until they lose all that is characteristic or informing. It is, perhaps, of far more importance that scientific books meant for the people should be as absolutely correct and as far advanced as it is possible to be, than those intended for scientific men themselves. The latter can discover and reject the false or imperfect; the former in their ignorance accept what is written as the truth, and the injury thus done is often serious in its consequences and may take a generation or longer to remedy. Popular scientific works, like school text-books of science, ought to be written only by those who are thoroughly masters of their subjects. The book before us seems to us to be in this respect satisfactory. In a series of chapters, each corresponding mainly with one of the great geological periods, the author endeavours to present a series of pictures of the gradual development of our earth, mainly with reference to the life which it supports. He seems to know his subject well in all its aspects, and presents in an interesting and intelligible way the latest results of geological research, with the conclusions derived therefrom by the most advanced thinkers. The illustrations are very good, and the work as a whole is a good specimen of a popular scientific treatise.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Remarkable Phenomena

IT may be within the memory of some of your readers that between the 15th and 20th August, 1868, a succession of waves reached Sydney, and were recorded by the self-registering tide-gauge. The average interval between the waves was about 25 minutes, and the greatest oscillation 34 inches, measuring from the crest of one wave to the hollow of the next. It was thought at the time that they were earthquake phenomena.

A similar visitation has just reached us, but it was not so marked in its character. The self-registering tide-gauge shows that the disturbance began during the afternoon of the 15th, and attained its maximum between 1 A.M. and 4 A.M. of the 17th, the greatest oscillation, amounting to 5 inches, occurred between 3.15 A.M. and 3 33 A M of the 17th, the average interval of the waves at this time was 25 minutes, but the average of 20 between 8 P.M. and 5.30 A.M. was 28 minutes. The waves cannot be traced beyond the 18th.

On the afternoon of the 16th we had a thunderstorm, during which the barometer was very unsteady, and the barograph sheets show some peculiar curves; strange to say, the average interval of the 5 most conspicuous of the barometer curves or waves between 5.40 P.M. and 7.30 P.M. is 25 minutes; the largest oscillation was 0·045 in. of mercury, equal to about 6 inches of water. Just before daylight on the morning of the 17th several fine meteors were seen to N.E., but the observer who reported them to me did not make notes of particulars. At Newcastle, which is a port 60 miles north of Sydney, I have another self-registering tide-gauge, which recorded a disturbance similar to the Sydney one; it began on the afternoon of the 15th and was greatest between 8 P.M. of 16th and 7 A.M. of 17th; the greatest oscillation, 9 inches, occurred between 12.15 A.M. and 12.30 A.M. of 17th, and the average interval of all the waves from 8 P.M. to 5.3 A.M. of 17th, amongst which are several that only occupied 5 minutes, and look like double oscillations, is 20 minutes.

Struck by the circumstances that both sets of waves, though separated by an interval of 5 years, occurred in August, I determined to examine all the tide-gauge sheets since 1866, when the instrument was set up, and was surprised to find a repetition of it every year, the amounts were too small individually to attract notice, but are nevertheless unmistakable, the periods are as follows:—

1866	August	9th to 10th, and again 15th to 21st.
1867	,,	5th ,, 13th, very marked from 9 A.M to midnight of 12th.
1868	,,	15th ,, 20th, remarkable (see beginning of this letter).
1869	,,	11th ,, 17th.
1870	,,	12th ,, 22nd, marked from 5 P.M. of 17th to 4 P.M. of 18th.
1871	,,	9th ,, 10th and 20th to 21st.
1872	,,	10th ,, 13th.
1873	,,	15th ,, 18th, as recorded in this letter.

It is not easy to believe that earthquake phenomena will recur with such regularity, and we must seek another cause depending it would seem on the earth's annual motion, and to a certain extent affecting air and ocean alike.

It would be premature to express a decided opinion without further investigation, which I have not had time to make yet, but it seems very probable that the August meteor stream through or near which the earth passes about 10th August may be the cause. It will be observed that even in the few observations given above there are indications of a five-year period; for the double disturbance of 1866 is reproduced in 1871, and the great disturbance of 1868 is followed by a similar one in 1873.

Sydney Observatory, Aug. 23

H. C. RUSSELL

Periodicity of Rainfall

I DO not altogether agree with Governor Rawson when he says, in his interesting letter in NATURE, vol. viii p. 245, that "the experience of Barbados is opposed to the theory broached by Mr. Meldrum and Mr. J. N. Lockyer." On the contrary, I rather think that Mr. Rawson's figures support the theory. He has taken 1846 and 1871 as middle maxima years (in my first paper I also took 1848), whereas 1849 and 1872 are probably more correct. Making this slight alteration, we get, according to Mr. Rawson's statistics:—

	Years.	Rain. In.	Sums. In.
Min.	1843	45·31	163·67
	1844	74·45	
	1845	43 91	
Max.	1848	63·77	184·42
	1849	52·77	
	1850	67·88	
Min.	1855	77·31	186·70
	1856	48·49	
	1857	60·90	

	Years.	Rain In.	Sums In.
Max.	1859	56·22	187·95
	1860	57·91	
	1861	73·82	
Min.	1866	59·68	174·21
	1867	69·93	
	1868	44·60	
Max.	1871	41·46	154·85
	1872	48·39	
	1873	65·00	

Grouping the results we obtain:—

Rain in Max. Years.	Rain in Min. Years.
184·42	163·67
187·95	186·70
154·85	174·21
527·22	524·58

showing an excess of 2·64 in. on the maximum side.

The quinquennial periods, as far as they admit of comparison, give also an excess in favour of the maxima years.

The heavy falls in 1844 and 1855, and the comparatively small fall in 1872, are apparently opposed to the theory; but it should be borne in mind that rainfall is greatly affected by local causes, and that to reveal the effects of a weaker but more general cause we must, as far as possible, eliminate chance, by comparing the total falls in maxima and minima periods. Tried by this preliminary test, the experience of Barbados can scarcely be said to be opposed to the theory.

My main object, however, is to draw attention to some discordances between Mr. Rawson's figures and those given by Mr. Symons in NATURE (vol. vii. p. 143); for until this disagreement be explained, there will be considerable uncertainty respecting the rainfall of Barbados. The following table will show where the two statements are at variance:—

	Years.	Rain (Mr. Symons)		Rain. (Mr. Rawson)	
Min.	1843	45·3	163·7	45·31	163·79
	1844	74·5		74·45	
	1845	43·9		43·91	
Max.	1847	42·5	158·3	48·10	164·64
	1848	62·8		63·77	
	1849	53·0		52·77	
Min.	1855	73·5	170·7	77·31	186·70
	1856	46·4		48·49	
	1857	50·8		60·90	
Max.	1859	55·1	186·6	56·22	187·95
	1860	60·4		57·91	
	1861	71·1		73·82	

The greatest differences are in 1847, 1855, and 1857, and amount (for these three years alone) to 19·4 in.

It is worthy of remark that both statements show an excess on the side of the maxima years; Mr. Rawson's of 2·2 in., and Mr. Symons's of 10·5 in. But how did such great differences arise.

A remark made by Mr. Rawson may explain the matter. He says "the average of the island for twenty-five years, from 1847 to 1871, is 57·74 inches, based upon the mean of three stations in 1843, and increasing to 141 in 1871." Now it would be useful to know how the mean yearly rainfalls were determined. Is the fall given for 1844 (74·45 inches) a mean of the falls at three stations, and the fall for 1872 (48·39 inches) a mean of the falls at 141 stations? If so, and if the other yearly means were similarly obtained, Mr. Symons may not have taken the same number of stations as Mr. Rawson. Yearly means thus determined would not of course be comparable, for even in a small island the rainfall varies greatly according to locality. The rainfall in maxima and minima sunspot years cannot be fairly compared except by taking the same number of gauges and the same stations; and it is desirable that the falls in the intervening years should be given.

"Assuming that sunspots affect all parts of the globe *equally*, and that periodicity prevails in all *alike*," Mr. Rawson, with the above experience of Barbados before him, is "led to the conclusion that it was 'chance alone' that led to the coincidences noticed by Mr. Symons." Now the theory makes neither of these assumptions. It assumes that there is a sunspot periodicity; that this periodicity implies a secular variation of solar heat and radiation; that, therefore, there is a corresponding periodicity of temperature, wind, and rain on our earth; but that, from various counteracting causes, the observations at some stations will not show a periodicity, while those at a large majority of stations, and a mean of all the observations, will do so. In short, with respect to rain, the theory assumes that the annual fall over the globe is subject to a variation, corresponding with the sunspot variation, but that from disturbing influences, local exceptions must be expected. Granting, therefore, that the rainfall of Barbados is opposed to the theory, I do not think it follows that the favourable experience of the British Isles must be owing to chance alone; for that experience is what theory leads us to expect, and it is much more extensive both as to time and space than the experience of Barbados. If England and Barbados were the whole globe, the theory would be well-nigh proved, as far as observation goes; for, according to Mr. Symons's Table 1, there was not, from 1815 to 1864, a single exception to the rule that more rain falls in the maxima years; and if we take the aggregate falls for England and Barbados from 1843 to 1873, it will be found that there was a large excess on the maximum side.

I have now examined 93 rainfall tables from various parts of the globe. They are all I have as yet been able to procure, and they have been published *in extenso*, so that the evidence they afford may be scrutinised. That evidence is such that if no rain at all had fallen at Barbados in the nine principal maxima years since 1843, and the rainfall in the nine minima years were to be put in the other scale of the balance, there would still be a large surplus in favour of the theory. Up to the present time the more numerous the observations, the stronger the evidence. Still I shall be prepared to abandon the theory whenever a preponderance of undoubted facts may be brought against it. But I see no prospect of this, for the rainfalls of England, Scotland, the Continent of Europe, India, Africa, America, and Australia, as far as they have yet been examined, sustain the theory.

C. MELDRUM

Mauritius, Sept. 15

Dr. Sanderson's Experiments and Archebiosis

DR. SANDERSON has strangely misunderstood the wording of my letter which appeared in NATURE on the 9th inst. Any one may see that I did not challenge him to "deal" with my main proposition "that Bacteria are capable of arising in fluids independently of living reproductive or germinal particles." That position was merely alluded to by me in order to show the relevancy of the question which I asked Dr. Sanderson, and the question itself was—"Whether he still believes that Bacteria are killed by a temperature of 100° C. in fluids; and if not upon what grounds he has changed his opinion?"

Whilst tacitly declining to answer this question, Dr. Sanderson now says, "I hope that Dr. Bastian will allow me to decline to enter on the general question." But it is precisely because Dr. Sanderson has distinctly expressed himself upon the general question both at the late meeting of the British Association and in your columns (NATURE, vol. viii. p. 181), that I feel he may, both from a moral and from a scientific point of view, be called upon to reply to the question above quoted.

The need that Dr. Sanderson should express the grounds of his opinion concerning the death point of Bacteria in heated fluids is further shown by Mr. Ray Lankester's communication in last week's NATURE, in which he says, "Dr. Sanderson does not believe that there is a definite relation between the precise temperature to which the infusion is exposed and the destruction of Bacterian contamination." Now if this is really Dr. Sanderson's present opinion, it may not inappropriately be asked whether it is an opinion based upon definite evidence or whether it is a mere surmise? I say the question is not inappropriate because, as Dr. Sanderson will recollect, I have heard from his own lips, since his return from Bradford, that he has made no definite observations upon the subject, and that he is quite unprepared to question the truth of the experimental evidence which I have recently brought forward (Proceed. of Royal Society, Nos. 143 and 145) showing that Bacteria are killed in fluids which have been raised for five minutes to a temperature of 60° C. (140° F.)*

Dr. Sanderson previously supposed that Bacteria were incapable of appearing and rapidly multiplying in certain fluids raised to 100° C. and subsequently protected from contamination. He has been convinced that his supposition on this subject was erroneous. And since this period, whilst I have been careful to undertake fresh researches concerning the death point of Bacteria, he has been content to rest in the stage of mere supposition on this most important point, and is now, as it appears, quite unprepared to question the truth of my assertion that Bacteria are killed at 60° C. It is right that the public should know this, and I only regret that Dr. Sanderson himself cannot be induced to inform them as to the real extent of his knowledge upon this part of the subject.

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* I should have hesitated about referring to what has passed in conversations between Dr. Sanderson and myself, if he had not set the example both in your columns (NATURE, vol. viii. p. 181) and in a discussion at one of the meetings of the Royal Society.

H. CHARLTON BASTIAN

University College, Oct. 20

Foreign Orders

THE acceptance and refusal of foreign orders by British subjects has hitherto been universally misunderstood. The existence of the Queen's Regulations, which you have reprinted in your columns (vol. viii. p. 481), prohibiting the receipt of these orders without special permission, must, after the discussion which took place in the House of Commons during last session, surprise many of your readers, who will naturally ask why regulations so stringent and so habitually disregarded, have been either kept entirely private in the Foreign Office, or, if published, have never been followed up. As it is, I will venture to say that not one out of some hundreds who have received foreign orders are aware of the prohibition or have any obvious means of becoming aware of it. Announcements of the presentation to British subjects (and it is assumed acceptance of by them) of such orders habitually appear in the most conspicuous type of the most widely circulated papers, but never a hint on the part of the Foreign Office that the recipients are violating Her Majesty's rules, as drawn up by itself and signed by the Secretary of State for Foreign Affairs.

Such being the case, it is somewhat singular that the Foreign Office should issue regulations approved by Her Majesty, forbidding British subjects to accept or to wear foreign orders and their decorations, except in the very rare cases in which Her Majesty's permission is obtainable, and yet take no steps through its agents at foreign courts to instruct the habitual givers that Her Majesty not only disapproves of their action, but requires of her subjects to tell them so in the most ungracious of all ways, namely by refusing to accept their favours, and returning the tokens thereof.

Surely if the prohibition to accept is wise and good (and I am the last person to doubt Her Majesty's wisdom) the obvious course for the Foreign Office to pursue is to inform all foreign Sovereigns of the fact, and instruct British subjects to transmit any orders that they may receive or have received to the Foreign Office to be returned to the sovereign who sent them, if the services of the recipient are not of such a nature as to enable him to obtain permission to accept them.

Into the merits of the prohibition I am not disposed to enter at much length. That foreign orders are comparatively valueless in themselves is generally admitted; and it is well understood that not a few are to be had for the asking by men of real or supposed eminence, and others by solicitation from men of no eminence at all, or of doubtful eminence. It would surprise your readers to know how many of these orders there are in the possession of their countrymen, whose habitual disregard of such honours leads them in most cases to toss them into a drawer and say nothing about it to any one but their wives, who think they would suit their necks better than their husbands' long-tailed coats.

Some few (very few) no doubt have a definite scientific or literary value; but so long as the British public are entirely ignorant of this value, they will be held in no higher estimation than the others, nor do I see any way by which the value of a foreign order could be made known and recognised, or by which the title of the recipient to wear it could be appraised

I believe that it is to the rarity of British orders that any desire to obtain foreign ones is mainly due. Had we more, or none, their value would diminish or expire; as, however, I am not prepared to propose either the restriction or multiplication of British orders, a third alternative might be suggested to the Foreign Office, and that is the command to wear them if accepted; which would result in a display in our *soirées* and assemblies of which men of eminence would be heartily ashamed, and lead to a petition for relief, that would be followed by an abandonment of the practice of giving by the powers that be.

D.C.L.

Mr. Forbes on Mr. Mallet's Theory of Volcanic Eruption.

I DO not intend to depart from my purpose, as stated in my last (NATURE, vol. viii. p. 485), to have done with further controversy. I must, however, beg your permission to correct a statement as to a matter of fact which constitutes the prominent feature of Mr. D. Forbes' letter on the above, and which is published in the last number of NATURE.

Mr. Forbes says, and begs your readers to remember that his remarks [namely, in his original review of my translation of "Palmieri"] were altogether directed to the assertions contained in my introductory sketch, and not *comments upon my theory of volcanic energy*—of which Mr. Forbes now says we, *viz.*, he and your readers, as yet know little or nothing. That is to say, nothing beyond what is given in the abstract in the Proceedings of the Royal Society and in my Introduction to Palmieri.

Mr. Forbes' review (NATURE, vol vii. p. 259) which called forth this correspondence, was no doubt confined to my translation of, and introduction to, "Palmieri's Vesuvius," &c. But in that same introduction was contained a sketch of my theory of volcanic energy—upon which Mr Forbes deemed himself warranted to make his sweeping condemnation—that it was not probable that this hypothesis will receive the adhesion of either chemist, mineralogist, or geologist.

If this were not a comment upon my theory of volcanic energy I know not what a comment means.

My complaint has been that it was a comment condemnatory—based on erroneous as well as inapplicable premises—and made at a time when, as Mr. Forbes himself in his last admits, he knew very little about that theory, as fully expounded in my paper in the Phil. Trans.

ROBERT MALLET

Oct. 28

Settle-Cave Report

I HAVE just read with considerable astonishment Mr. Tiddeman's letter (NATURE, October 23) relating to an abstract which I never saw till to-day, and for which, therefore, I am not responsible. The whole question of the antiquity of cave-deposits as well as that of those in the Victoria Cave, in particular is treated in my work on "Cave-Hunting," shortly to be published, and therefore I see no reason for entering into any argument based on the distribution of the Pleistocene Mammalia, or to depart from my rule of not entering into a controversial correspondence.

W. BOYD DAWKINS

Owens College, Manchester, Oct. 24

The Oxford Science Fellowships

I WRITE to confirm Prof. Clifton's letter (in the last number of NATURE) respecting Mr. Perry and Oxford Science Fellowships. Nothing, it seems to me, can be more conclusive than the way in which Mr. Perry's letter has been answered. Any remark further of mine on this point would be superfluous.

I will only say that, in the practical part of the examination, no subject could have been chosen better fitted for giving perfectly fair play to all concerned. If it were possible to imagine that any advantage was given, it was, by the choice of the subject, given to those who were unacquainted with the University laboratory.

In conclusion—far from being looked on as an unwelcome intruder, I met with from all, whether candidates or examiners, the most generous courtesy and kindness.

Cambridge, Oct. 24 THE CAMBRIDGE B.A.

PROFESSOR CLIFTON cannot have considered what a great mistake I have been the victim of, or he would not in his hastily written attempt to defend the general science arrangements at Oxford, have forced me to the following explanation. He knows that I stated my case fairly, and he might surely have given credit for this whilst letting us have the benefit of his later information.

1. I have not at hand a copy of my letter to the Warden. I am quite sure that I told him I was a graduate of the Queen's University in Ireland. The Warden simply directed me to the short notice in the *Times* (afterwards given in your columns), said that the election would not be limited to graduates of Oxford, and would altogether depend on the results of the examination held at Merton on Oct. 7. I thought this letter perfectly satisfactory

as to my eligibility, as did several Oxford graduates to whom it was shown. I shall presently refer to Prof. Clifton's "warning."

The examination was to begin on Oct. 7, at 9 A.M. On presenting myself, a gentleman whose name I do not know, told me that the Physics papers would not be given out before Oct. 10, that if I felt inclined to work the paper given to candidates for the *Mathematical* Fellowship I might do so, and credit would be given for Mathematics in the event of two men being equal in the Physics examination. I shall not comment on this promising arrangement, or on the fact that the candidates for the Physics fellowship had not till then heard of the Mathematical paper. Our informant told me that there were grave doubts as to the eligibility of outsiders. He certainly gave me to understand that these doubts extended to *all* who were not Oxford graduates. I understood that some Cambridge men had presented themselves also; that the question of our eligibility was about to be settled with the Registrar of the University, and that if I called on the Warden between four and five in the afternoon (the time mentioned in the original notice) he would be provided with the results of the deliberations.

At 4.30 I found the Warden about to go away somewhere. I had an audience of about two minutes; was asked what College I belonged to (meaning in Oxford).—Not an Oxford man, I answered.—Then he was afraid I was ineligible. I then informed him that I was the graduate of the Queen's University, to whom he had written in June. I suppose he had very little time for apologies, but he let me know, before leaving, that he had misinterpreted the results of some late commission when he wrote in June, and that I need have no hope.

I have stated the grounds for my former general statement. If Prof. Clifton is certain that graduates of Dublin and Cambridge are eligible, we must rely on his information being most correct, but I am troubled to know who is answerable for my being left in ignorance until now, and if anybody knows whether elections are never made of men who would really be ineligible by the laws of the University.

2. He insinuates a deception on my part, in not mentioning his "warning." I take it that Prof. Clifton has partly forgotten the matter of which he speaks. I wrote to him for leave to inspect the Physical Laboratory at Oxford, not certain that he was one of the examiners, but aware that he had charge of that institution and that the examination *must* be held there (see 3). I did not speak of my eligibility.

There is no doubt about the fact that great difficulties are thrown in the way of outsiders, but I should have been wrong if I had laid any blame on Prof. Clifton for taking the only course open to him. The case is simply this: according to the present Physics arrangements at Oxford, outsiders preparing for the October Fellowship examination at Merton could not without giving the greatest imaginable trouble to Prof. Clifton get any opportunity of inspecting the apparatus.

After stating that he was unable to afford me the desired opportunity, he asked if I had ascertained about my eligibility, informming me that the warden or sub-warden was the proper person to apply to. I immediately wrote *that I had already made such an inquiry*, stating the result.

I now infer that he, after receiving my letter and aware that I had made the proper inquiries, allowed both the Warden and myself to remain in ignorance of the grievous mistake. On receiving no answer I felt perfectly certain that the information received from the Warden was correct.

When I last wrote to NATURE I felt grateful to Prof. Clifton for his inquiry, incomplete and worse than useless "warning" as it had been. Surely no one will think that I had any right to introduce his name.

3. He says it was by no means certain that the Practical Physics examination would be held in the Physical Laboratory. Will he assert that in any one of the nineteen colleges of which he speaks, or in the nineteen collectively there is apparatus for conducting such an examination?

He wonders why it should be necessary to inspect the particular apparatus to be employed in the examination. I do not know if Prof. Clifton was really one of the examiners for the fellowship, but surely he cannot have thought about the matter without being aware of the immense importance of a previous acquaintance with the apparatus such as Oxford men are sure to have. I heard by accident in July that there was no delicate apparatus, nor were proper arrangements made for exact experiments in Static Electricity. Can Prof. Clifton not understand that to an outsider such information might be of the greatest importance. "What arrangement of telescope stand is there for measuring wave-lengths?" "Is there a Soleil's instrument for measuring the angle between the axes in biaxial crystals?" "Will the arrangements for observing deflections of a needle enable us to employ the logarithmic decrement?" These questions and a hundred others as important were constantly distracting me during the four months of preparation.

My letter to Prof. Clifton was, I believe, modest, and showed my respect for him as a man who had done a great work in his attempt to create a Physics School at Oxford. My request was not "unreasonable." I did not know that his presence was necessary during an inspection of the Physical Cabinet of the University. I maintain too, that he has no right to assert that I must feel very uncertain about my own practical knowledge.

London, Oct. 28 JOHN PERRY

Simple Diffraction Experiment

THE apparatus for this experiment consists of a slit and a grating. A slit may be made by ruling a line on a piece of smoked glass. The grating is made by slightly greasing the thumb and forefinger (there is naturally sufficient on the hot and moist hand), and by drawing a piece of clean glass through them so as to obtain alternate parallel light spaces and greasy lines on both sides of the glass; out of several trials a grating may be made which when used in the following manner will give very pretty results.

The grating being placed close to the eye, the slit (with its direction parallel to that of the lines on the grating) is held up before some bright light, as of a candle, and looked at, as if the grating did not exist. Very beautiful and numerous spectra may then be seen ranged on each side of the slit.

The vitreous surface of window glass does not seem to give such good gratings as a worked and polished surface, as for instance that of a weak spectacle lens.

Oxford H. L.

Publication of Learned Societies' Transactions

IN NATURE, vol. viii. p. 506 Mr. Rohrs wishes that our learned societies would publish their papers separately. I have urged this before in NATURE, but unsuccessfully. With Transactions such as those of the Royal Society, the present system is almost an absurdity, for papers on most incongruous subjects are bound up together, and the cost is too great. When once a paper is printed, the Council seem to think that there is nothing more to be done, and do not in any way try to make the work known. All papers should be sold separately as cheaply as possible, and on publication, should be advertised in the scientific journals.

If this were done, we should not have men like Prof. Sylvester writing as follows:—"I owe my thanks to M. Radau and the editor of the Annals of the École Normale Superieure for having been at the pains to disentomb the little known conclusions contained therein from their honourable place of sepulture in the Philosophical Transactions." W. B. GIBBS

EXAMINATIONS OF THE SCIENCE AND ART DEPARTMENT IN BIOLOGY

THE syllabus of the Biological subjects in which examinations are held by the Science and Art Department, has undergone considerable modifications in the edition of the Directory which has been recently issued. Animal Physiology, Elementary Botany (including Flowering Plants only), are subjects which at present appear to be best adapted for the purposes of school instruction. They stand, therefore, in no necessarily logical relation to the other two which are grouped together under the head of General Biology. These involve the use of the compound microscope, and some amount of microscopic manipulation. They are therefore better fitted for rather more advanced, or at any rate, older students than the first stages of the subjects first mentioned.

The two subjects included under General Biology have a common first or Elementary stage. After passing this, the candidate may proceed at choice, either with the zoological or the botanical side.

The following extract from the syllabus will show how this arrangement is intended to work, and will afford the best idea of the direction which the examination is likely to give to elementary biological study. It does all that a written examination can do to encourage practical work, and discourage the prevalent habit of cramming from text-books :—

SUBJECTS XVI. AND XVII.—GENERAL BIOLOGY

First Stage or Elementary Course

Questions will be confined to the following subjects with which the candidate will be expected to show practical acquaintance.

1. The form and size; the results of optical, chemical, and mechanical analysis; the mode of growth and multiplication; the conditions of life; and the results, direct and collateral, of the living activity of *Torula, Protococcus, Amœba, Bacterium*, and of the colourless corpuscles of the blood of man.

2. The structure and mode of growth of *Penicillium*; its mode of multiplication; the development of *hyphæ* and *mycelium* from *conidia*. the conditions and results of the living activity of this mould.

3 The structure and mode of growth of *Chara*; the differentiation of axis and appendages, of nodes and internodes; the structure and arrangement of the nucleated cells of which the body of this plant is composed The process of cell-division and its laws; protoplasmic movements; Chlorophyll; asexual propagation; sexual propagation. Development of the pro-embryo and of the embryo.

4. The structure and mode of growth of a Fern. The differentiation of cells into tissues. Epidermis, parenchyma, fibres, ducts, spiral vessels. The Frond as a respiratory and alimentary organ; air-passages; stomata Asexual multiplication. Sporangia and spores. Development of spores; structure of the Prothallium. Structure and functions of Archegonia, Antheridia and Antherozoids. Development of the embryo.

5. The anatomy and physiology of a flowering-plant, with especial reference to the morphology of the stem and root. Leaves and their modifications. The structure of pollen and ovule. The process of impregnation and the development of the embryo. The resemblances and differences between flowering-plants and ferns.

6. The anatomy and physiology of the frog. The general disposition of the parts of the body, and the plan of structure characteristic of the frog as a vertebrated animal. The structural characters of the tissues of which the body is composed and their ultimate resolution into nucleated cells.

The physiological properties of the tissues.

The form and structure of the chief organs and the modes in which their functions are performed.

The development of the embryo and the metamorphoses of the larva.

7. The anatomy and physiology of the freshwater Polype

8. The anatomy and physiology of the Lobster or Cray-fish.

9. The anatomy and physiology of the fresh-water Mussel.

10 The anatomy and physiology of the Sea-anemone.

Second Stage or Advanced Course of Subject XVI.

(Division of Animal Morphology and Physiology)

Questions may be set in all the topics enumerated under the first head, and in addition on :—

The leading facts relating to the anatomy and physiology of the skeleton, of the brain, and of the cerebral nerves; of the organs of the higher senses; of the alimentary, circulatory, respiratory, renal, and reproductive apparatus, in the Lamprey, in an osseous fish (Pike or Cod), bird (Pigeon, Fowl, or Duck), in a quadrupedal mammal (Sheep, Rabbit, Dog, or Cat,) and in Man.

2. The morphology of the vertebrate skull and limbs, as exemplified by the *Vertebrata* already mentioned, and by the Dogfish, Horse, Bat, and Porpoise

3. The general outlines and process of the development of the chick within the egg.

4. The characters of the orders of the *Vertebrata*.

5. The broad facts relating to the geographical and geological distribution of the *Vertebrata*.

6. The anatomy and physiology of insects, as illustrated by Blackbeetle, a Bee, a Butterfly, and an Aphis.

7. The anatomy and physiology of an Earthworm and of a Leech.

8. The anatomy and physiology of a Fluke and of a Tapeworm, and the history of their development.

9. The anatomy and physiology of the *Rotifera* and of the *Polyzoa*.

10. The anatomy and physiology of a Sea-urchin (*Echinus*) and the history of its development.

11. The anatomy and physiology of a Snail and of a Whelk, and of a Cuttlefish, Squid, or *Octopus*.

12. The morphology of the *Hydrozoa*.

13. The anatomy and physiology of the *Infusoria*.

14. The anatomy and physiology of sponges, *Foraminifera* and *Radiolaria*.

Honours.

In this examination questions will be set at the discretion of the Examiner, who will have regard to the state of Zoological teaching in the country and the means of acquiring information.

Second Stage of Advanced Course of Subject XVII.

(Division of Vegetable Morphology and Physiology.

Questions may be set in all the topics enumerated under the first head, and, in addition, on—

1. The principal modifications in the minute anatomy of the axis in flowering plants.

2. The nature of the parts used for support in climbing plants.

3. The various modes of agamogenesis in flowering plants.

4 The leading facts in the development of the parts of a flower, including that of the pollen, ovule embryo sac, endosperm (albumen), and embryo

5. The morphology and relations to one another of the parts of the flower and fruit throughout the classes Dicotyledons and Monocotyledons, more especially as exemplified in the following genera.—

Ranunculus, Nymphæa, Capsella, Viola, Stellaria, Malva, Geranium, Ilex.
Euonymus, Vicia, Rosa, Saxifraga, Lythrum, Epilobium, Anthriscus.
Lonicera, Senecio, Campanula, Erica, Solanum, Plantago, Lamium.
Polygonum, Urtica, Viscum, Fagus.
Orchis, Iris, Potamogeton, Allium, Arum, Lemna, Typha.
Carex, Triticum.

6. The various adaptations by which cross-fertilisation is effected in Flowering plants.

7. The modes by which seeds are diffused

8. The broad facts of the geographical distribution of Flowering plants.

9. The distinctive characters and origin of the Arctic-alpine flora, and the floras of oceanic islands.

10. The morphology and physiology of the vegetative and reproductive organs in Pinus, Taxus, and Juniperus.

11. The geographical and geological distribution of the genera of Gymnosperms.

12. The morphology and physiology of the vascular cryptogams, more especially with reference to the following types :—

Selaginella, Pilularia, Lycopodium, Equisetum, Polypodium, Lastrea, Osmunda.

13. The morphology and minute anatomy of the Carboniferous Lycopodiaceæ.

14. The morphology and physiology of Mosses and Liverworts as exemplified by Polytrichum (or Funaria) and Marchantia.

15. The morphology and physiology of Algæ as exemplified by—

Fucus, Ceramium, Saprolegnia, Spirogyra, Closterium, Ulva Volvox, Protococcus, Palmella.

16. The modes of reproduction in Fungi as illustrated by—

Agaricus, Peziza, Penicillium, Peronospora, Mucor, Uredo, Saccharomyces (yeast).

17. The processes of plant nutrition, comparing also their modifications in Fungi, Neottia, and different parasitical plants.

18. The ash constituents of plants and their distribution in [illegible] tissues.

19. The influence of heat and light upon plants.

Honours

Questions at the discretion of the examiner, who will have regard to the state of botanical learning in the country, and the means of acquiring information.

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE*

VII.

WEIGHING AND MEASURING INSTRUMENTS, AND THEIR USE

THE instrument universally used for weighing is the balance, with its various modifications. It serves to determine the weight of bodies by comparison with a body of known weight, such as a standard weight. The simplest form of balance is a beam made to vibrate upon a centre or axis of motion, with pans hanging from the extremities of the two arms of the balance. These two pans hold the bodies compared, and their equality or difference of weight can thus be determined.

Balances are of two kinds:—1. Ordinary balances with equal arms, which have the beam suspended by the middle. If an equal-armed balance is accurately adjusted, so that the beam is exactly horizontal when the pans are empty, the beam will also be horizontal, and the balance will be in equilibrium when equal weights are placed in the pans. 2. Balances with unequal arms, in which the beam vibrates upon the centre of motion placed more or less near one of the extremities. In both of these kinds of balance the beams are levers of the first order, the fulcrum upon which the beam vibrates being placed between the power and the weight, that is to say, between the extremities of the beam which support the bodies compared. On the principle of the lever, the power of any weight to move a balance is proportionately greater according as the part of the beam which supports that weight is more distant from the fulcrum or centre of motion of the balance. Hence it follows that the power of the weight to move a balance is in a ratio compounded of the weight itself and of its distance from the centre of motion of the balance. A multiplying or proportionate balance may consequently be constructed for determining the weight of a body placed in the pan suspended from the shorter arm of the bearer, and required to be equal to any multiple of a given unit weight placed in the pan suspended from the longer arm of the beam, termed the weight pan. For this purpose, if the beam be divided into, say three equal parts, and the centre of motion be placed at the first division, one pound placed in the weight pan will form an equipoise with two pounds placed in the other pan, and so on. This principle is greatly extended in larger weighing machines by lengthening the longer arm, through the use of compound levers, so that one pound can be made to form an equipoise with 180 pounds or more.

The ancient Roman balance is perhaps the earliest form of a well-constructed multiplying balance, and corresponds with our modern steelyard. It has been remarked by Sir Gardiner Wilkinson that no instance has been found of the existence of the steelyard before the Roman era. But the principle of its construction was in use amongst the ancient Egyptians, who ascertained the weight of articles suspended from different parts of a scale beam by means of a heavy determinate weight placed in one scale. The Roman balance consists of a determinate weight attached to the longer arm of the beam, and made to traverse along a number of divisions marked upon it. The multiplied power of the traversing weight when resting on the several sub-divisions, as they increase in distance from the centre of motion, is indicated by corresponding figures upon the graduated beam.

The following figure (taken by permission from the "Imperial Journal of Art," vol. i. p. 85) represents an ancient Roman balance of an elegant form, found at Pompeii, and in use A.D. 77. It is described as having the graduated divisions on the longer arm of the beam marked with Roman numerals from X. to XXXX. (probably Roman pounds), and with a V. on the half of each decimal series, the smaller subdivisions being also marked. The inscription on the shorter arm of the beam (shown in a separate and enlarged figure) denotes its having been proved at the Capitol in the 8th of Vespasian Emperor Augustus, and in the 6th Consulate of Titus Emperor Augustus his son. This steelyard is consequently a duly verified standard weighing machine.

For the justness of an equal-armed balance, it is requisite (1) that the points of suspension of the pans from the beam be exactly in the same line as the centre of motion; (2) that these points be precisely equidistant from the centre of motion; (3) that the arms be as long as conveniently may be, in relation to their thickness and the weight they are intended to carry, in other words, consistently with the stability of the balance; (4) that there be as little friction as possible at the centre of motion and the points of suspension; (5) that the centre of gravity of the beam be placed a little below the centre of motion.

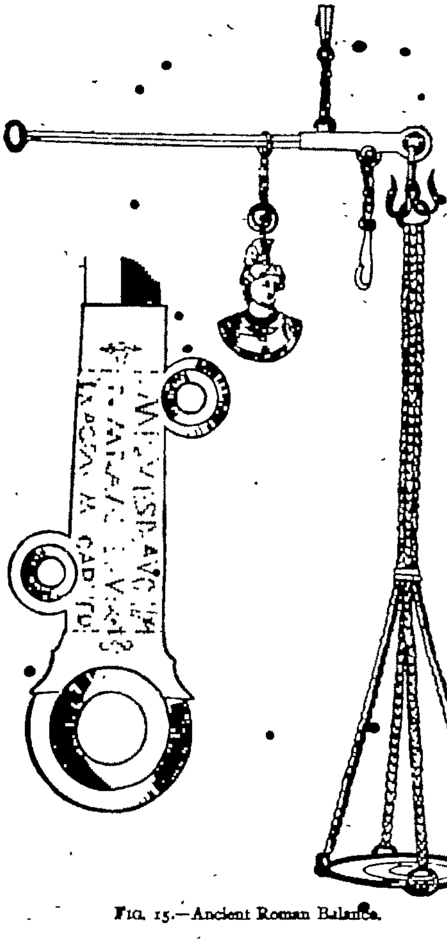

FIG. 15.—Ancient Roman Balance.

The fulcrum upon which the beam of a balance rests is formed with a steel knife edge, and the two pans at its extremities are hung upon similar knife edges. In ordinary trade balances, these knife edges, are placed in contact with steel bearings having a spherical curve. But in the practical construction of balances of a high degree of sensibility, such as are required for scientific purposes or for the comparison of standards in which very minute differences of weight are to be determined, there are many circumstances to which attention is requisite, that may properly be neglected in balances used for commercial purposes. In such balances of precision great care is required in the adjustment of the knife edges. They are first made quite sharp, and are then slightly rounded with a fine hone or a piece of buff leather. On the regular form of this rounded edge, the excellence of the action of the balance very much depends. The central knife edge rests upon an agate or polished steel plate, whilst the two pans are suspended from agate or steel planes bearing upon the knife edges at the ends of the beam. In order to preserve the nice adjustment of the knife edges, they are never allowed to rest upon their bearings, except when weighings are made. At all other times, the beam and pans are separately supported upon a brass frame attached to the column of the balance, but moveable in a vertical direction upon it. When required to be put in action the support is gradually lowered by means of a lever handle, and the knife edges are brought upon their bearings.

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* Continued from p. 491.

The principal cause of discordances in the results of successive weighings with a balance of precision arises from the risk of the knife-edges not being brought again to exactly the same position on the plane bearings, after the balance has been stopped and again set in action. The most perfect balance is that which varies least in the points of contact between the knife-edges and their bearings during successive weighings. For the attainment of this very important requirement, the supporting frame is furnished at each of its extremities with two pins terminating in cones and made to fit exactly into corresponding conical holes in the plane bearings, at each of the extremities of the beam. The pins and holes are in a line normal to the axis of the beam. The points of these four cones are all in the same horizontal plane. As the movement of the supporting frame in a well-constructed balance of precision is always in the same vertical line, being guided by a vertical rod fitted to a cylindrically drilled hole in the column of the balance, the knife-edges and their bearings are always brought into contact in the same relative positions. Balances of precision are always enclosed in plate glass cases, with a view both to their preservation, and to keep the balances as far as possible from being affected in their action by draughts of air, alternations of temperature, &c.

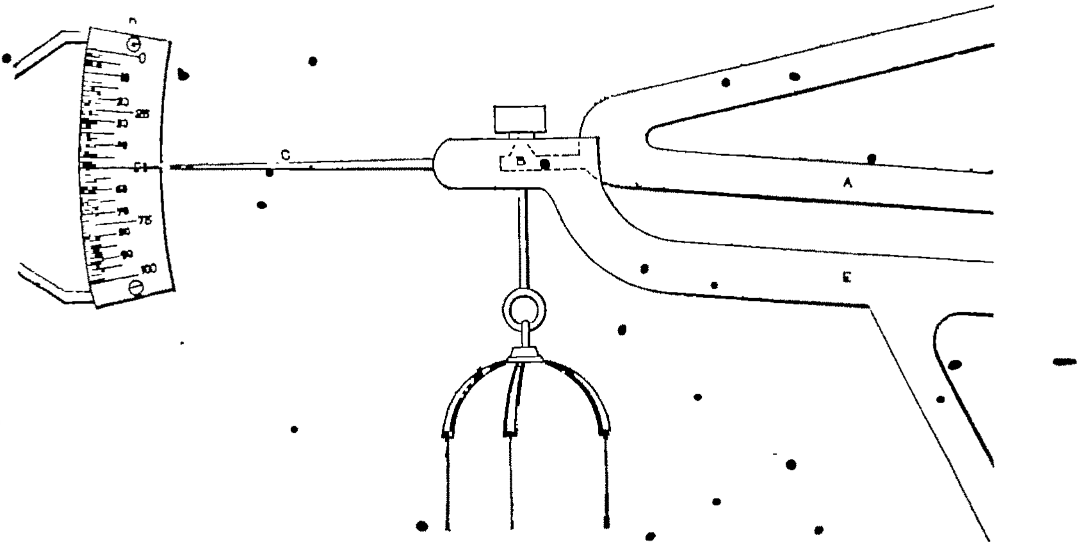

FIG. 16.—Index Scale, &c., of No. 3 Balance of Standard Department.

As to the theory of the relative positions of the centre of motion and the centre of gravity of a balance, it is to be remarked, (*a*) If the fulcrum be placed in the centre of gravity of the beam, and the three edges be all in the same right line, the beam of the balance will have no tendency to one position more than another, but will rest in any position in which it may be placed, whether the pans be suspended to it, or not, and whether the pans be empty or equally loaded. (*b*) If the centre of gravity of the beam, when level, be immediately above the fulcrum, it will upset with the smallest action; that is to say, the end which is lowest will descend; and it will descend with the greater velocity, according as the centre of gravity is higher, and the points of suspension less loaded. (*c*) But if the centre of gravity of the beam be immediately below the fulcrum, the beam will not rest in any position but when level; and if disturbed from that level position, it will vibrate, and at last come to rest in a horizontal position. Its vibrations will be quicker, and its tendency to the horizontal position stronger, the lower the centre of gravity, and the less the weight upon the points of suspension.

Again, as to the relative position of the central knife edge, which constitutes the fulcrum of the beam with the line joining the two outer knife edges, which form the points of suspension, it is further to be remarked, (1) If the fulcrum be below the line joining the points of suspension, and these be loaded, the beam will upset, unless prevented by the weight of the beam tending to produce a horizontal position, as shown in (*c*). In such case, small weights will form an equipoise. In case of (*a*), a certain exact weight will rest in any position of the beam; and all greater weights will cause the beam to upset, as in (*b*). (2), If the fulcrum be above the line joining the points of suspension, the beam will come to its horizontal position, unless prevented by its own weight, as in (*b*). (3) If the centre of gravity be nearly in the fulcrum, all the vibrations of the loaded beam will be made in lines nearly equal, unless the weights be very small, when they will be slower. The higher the fulcrum the quicker will be the vibrations of balances, and the stronger the horizontal tendency.

It is thus evident that the nearer the centre of gravity of the beam is to the centre of motion, the more delicate will be the balance, and the slower the vibrations. The tendency to a horizontal position is therefore increased by lowering the centre of gravity, in which case it will also require a greater additional weight to cause it to turn or incline to any given angle, and it is therefore less sensible with a greater load. The fixing of the centre of motion in a balance is consequently of peculiar importance, for on this depends the ease with which it will be affected by a smaller weight, and the readiness with which the beam will return to a horizontal position. And it will be seen that the best position of all is that in which the centre of motion is a little above the centre of gravity. Even in this, it should be proportioned to the distance of the weights from the fulcrum, and the amount of the load, which can only be attained in different beams by practice and experience. In order to regulate the centre of gravity in balances of precision, they are made to carry a small weight either over or under the centre of motion, which is moveable by means of a screw.

From what has been said it would appear that if the arms of a balance be unequal, weights which form an equipoise will be unequal in the same proportion. But although for many purposes the equality of the arms of a balance is advantageous, yet a balance with unequal arms will weigh just as accurately as one with equal arms, provided the standard weight itself be first counterpoised, then taken out of the pan, and the weight to be compared be substituted and adjusted against the counterpoise. Or when proportional quantities only are required, they may be weighed against standard weights, taking care always to put these weights in the same pan. But in this case it is indispensable that the relative lengths of the two arms of the beam continue invariable. For this purpose, either the three knife-edges should be truly parallel, or the points of suspension and support be always in the same part of the knife-edge.

If the beam of an equal armed balance be adjusted so as to have no tendency to any one position, as in (*a*), and the pans be equally loaded, then if a small weight be added to one of the pans, the balance will turn, and the point of suspension move with an accelerated motion, similar to that of falling bodies, but very nearly as much slower in proportion as the added weight is less than the whole weight borne by the fulcrum. The stronger the tendency to a horizontal position in a balance, or the quicker its vibrations—see (*c*) and (2)—the greater additional weight will be required to cause it to turn or increase to any given angle. If a balance were to turn with $\frac{1}{10000}$ part of the weight, it would move at the quickest, 10,000 times slower than a falling body; that is to say, the pan containing the weight, instead of falling through 16 ft. in a second of time, would fall only through $\frac{1}{50}$ part of an inch; consequently all accurate weighing must be slow.

Long beams have been generally recommended because the quantity of motion in a given body varies as its distance from the fulcrum; and therefore the greater the distance, the most distinguishable will be the motion arising from any small difference between the weights compared. On the other hand, there are certain advantages in the quicker angular motion, greater strength, and less weight of a short beam.

The pans of a balance should be suspended in such a manner that in all positions the corresponding cords or rods may be parallel to one another; else the weights, though equal, will not be in equilibrium.

In ordinary commercial balances, the preponderance of either pan is indicated by a slender rod attached to the beam immediately over its centre of motion in a line perpendicular to the axis of the beam, and moveable freely between the two forks of the handle. It is called the tongue of the balance, and the degree of preponderance of either pan is shown by the greater or less deviation of the tongue from its normal vertical position. In balances of precision, the index is a longer needle-rod, fixed either in a line perpendicular to the axis of the beam, and below its centre of motion, or in a line in continuation of its axis. In both cases the pointer moves along a graduated index. But an index placed perpendicular to the beam affects its equilibrium when turning from its horizontal position; the momentum of the index being measured by its weight multiplied with the distance of its centre of gravity from a line perpendicular to the horizon. The error thence arising may, however, be corrected by continuing the index-rod or counterpoising it, on the opposite side of the beam.

The finest balances of the Standards Departments have the index pointer in the line of the axis of the beam, as shown in Fig. 16, which represents the left-hand side of the balance, the right-hand side being similarly furnished with a pointer and index scale.

This is the medium size of six of the finest balances of the Standards Department, constructed by Mr. Oertling. For all weighings of standards requiring special accuracy, the highest and lowest points reached by the needle in each oscillation of the balance are read on the index scale through a telescope fixed at about 5 ft. distance, by which means each reading can be satisfactorily taken by estimation to one-tenth of a division of the scale.

Another balance of the Standards' Department is one constructed by Barrow, and used by Prof. Miller for all his weighings during the construction of the new Standard pound. The knife-edges work upon quartz planes. Index scales marked on a thin and nearly transparent slip of ivory are fixed immediately above each end of the beam and oscillate with it. They are of the following form and size. These scales are illuminated by a candle placed at a little distance either in front of or behind the balance case, a lens being interposed; and they are viewed through compound microscopes, having a single horizontal wire fixed in the focus of the eye-piece. The microscopes are fixed to the front of the balance-case, and as the observer must necessarily be close to the microscopes during weighings, a second glass screen is interposed between him and the front of the balance-case, having openings opposite the eye-pieces of the microscopes.

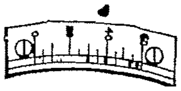

FIG. 17—Index Scale of Barrow's Balance

The weight intended to be carried by each of these balances, and the mean value of one division of the index scale, or the weight represented by it, when the balance is fully loaded, may be seen in the following table:—

Balance	Length of Beam.	To carry in each pan		Mean value of 1 div. of Index Scale.
	In.	Avoir.	Troy	Grains.
No 1	36	56 to 14 lbs,	or 500 to 200 oz.	0·15
No 2	24	7 to 2 lbs,	or 100 to 20 oz.	0·02
No 3	16	1 lb to 2 oz	or 10 to 2 oz	0·0015
No. 4	10	1 oz. and under,	1 oz and under	0·0008
No 5	10	30 gr. and under		0·0002
No 6	20	1 kilo. and under		0·0015, or 0·1 mgr.
Barrow's	...	1 kilo and under.		0·005, or 0·3 ,,

There is another much larger balance which was originally constructed for weighing the contents of water of the Imperial Standard bushel, the total weight in each pan being nearly 300 lbs. The beam of this balance is of mahogany, 67½ in. in length. With a full load, the mean value of 1 div. of the index-scale is 0·4 grain. This balance, like the other, is enclosed in a large plate-glass case.

In all these balances, the value of a division varies from time to time according to the weight in the pans, the condition of the balance, the state of the atmosphere, &c., and in all very accurate weighings it is desirable to determine the value for each comparison, by an additional weighing, after a very small weight, accurately verified and equal to a few divisions only of the balance, has been added to one of the pans, so that its effect on the reading of the index scale may be noted. The above stated values indicate nearly those found when the balance is in good working condition, and fairly weighted.

All these balances, when in equilibrium, will turn with a very small additional weight, equal to the value of two or three divisions, placed in one of the pans. They are exceedingly sensitive, for the sensibility of a balance is to be measured by the least amount of additional weight placed in either pan that is sufficient to turn the index-pointer from its normal position, when the balance is in equilibrium, and by the greatest amount of deviation from the normal position which is produced by a very small difference in the weights.

H. W. CHISHOLM

(*To be continued.*)

*CINCHONA CULTURE**

FEW subjects have been so frequently before pharmaceutical readers during the past ten or fifteen years as the efforts of the governments of Holland and Great Britain to introduce the various species of Cinchona into their respective colonies. It would be hardly possible to overrate the importance of the enterprise, and it is one that interests alike the pharmaceutist, the botanist, and the votary of economic science. The records of progress which have been made public are so scattered and unconnected, the opinions and reports so conflicting, that it has been difficult for the general reader to retain the thread of the story or to arrive at any very clear estimate of the present position and prospects of the undertaking. The earliest steps in this great experiment in acclimatisation date back to a period before that which we have had under review, but so far as results are concerned, the subject is one which pertains essentially to the past few years, and I propose to place before you, in as few words as may be, and unencumbered by the controversial matter with which its literature abounds, an outline of the beginning of the enterprise and of its present practical aspect.

The initiative in Cinchona cultivation was taken, as you well know, by the Dutch Government, whose efforts were directed to its introduction into the Island of Java. The first Cinchona trees which were sent out to that colony were a few specimens of *C. Calisaya* † raised from seeds collected by M. Weddell in Bolivia, and forwarded by a firm of nurserymen in Paris in exchange for rare Javan plants. In the same year, 1852, the Dutch Government were induced to send M. Hasskarl, a gentleman previously attached to the Botanic Gardens at Buitenzorg, on a mission to South America, for the purpose of collecting plants and seeds. During the two years following M. Hasskarl pursued his labours, and succeeded in forwarding consignments from some parts of Peru, the Cinchona districts of Bolivia being for the most part closed against him; and his efforts were supplemented as to the New Granada species by the assistance of Dr. Karsten. The resulting collections were sent in part direct to Java, and the remainder to Amsterdam for re-shipment. I need not dwell on the mishaps and disappointments inevitable in so new and difficult an enterprise—it is sufficient to note that within three or four years, that is by the middle of 1856, upwards of 250 plants, almost exclusively of two species, *C. Pahudiana* and *C. Calisaya*, were flourishing in the Java plantation as the outcome of the expedition. In the same year, with wise forethought, an accomplished chemist, Dr. De Vrij, was sent out to conduct chemical observations on the growing barks.

We may pass over the long series of troubles that attended the early efforts of those in charge of the trees, the ravages of insects, the destruction of young plants by rats, the devastation committed by wild cattle and rhinoceroses, and, above all, the difficulties dependent on climate, which eventually necessitated the transplantation of nearly the whole of the trees from the locality first chosen, on the north side of the mountain range, to one with a southern aspect. We will pass on, I say, to the year 1863, and we shall find that the total number of Cinchona trees in Java was then 1,151,810. Of these about 99 per cent. were of the species know as *C. Pahudiana*, the remainder comprising about 12,000 of *C. Calisaya* and trifling numbers of four other species. This proportion was unfortunate, for the bark of *C. Pahudiana* was found to be deficient in alkaloids, and therefore supposed to be valueless, and by decrees dated 1862 and 1864 its further culture was ordered to be forthwith stopped.

We may now turn to the steps taken by the British Government in the same direction.

Dr. Ainsley, in his work on "Materia Medica," was perhaps the first to suggest the idea of the acclimatisation of the Cinchona in India, and, as early as 1839, Dr. Forbes Royle especially indicated the Neilgherry and Silhet mountains as eligible for the experiment. Appeals were subsequently made to the East India Company by Mr. Grant and Dr. Falconar, with the object of inducing them to take up the matter, and in 1852 instructions were sent to the British consular agents in South America to endeavour to procure seeds of the various species, but without much real effect. Dr. Royle, as Reporter on the Products of India, continued to urge the subject on the attention of Government up to the time of his death, and eventually, in 1859, at the instance of his successor in office, Dr. Forbes Watson, the services of Mr. Clements R. Markham were called into request by the home authorities.

Mr. Markham proposed a fourfold expedition to South America, and his scheme was at last sanctioned by the Secretary of State for India, and ordered to be carried out. The first portion of the expedition was directed to Bolivia and Caravaya, the region of *Cinchona Calisayo* and *C. micrantha* (var. *Boliviana*). Secondly, Huanuco and Huamalies were to be searched for *C. nitida* and *C. glandulifera*. Thirdly, Cuenca and Loxa in the Republic of Ecuador for *C. Chahuarguera*, *C. Uritusinga*, and *C. Condaminea*; and lastly, New Granada as the habitat of *C. pitayo* and *C. lancifolia*. Mr. Pritchett and Mr. Spruce were appointed coadjutors to Mr. Markham, and the expeditions set out in 1859, the latter gentleman proceeding to the northern part of Bolivia, the district of the yellow barks; Mr. Spruce to the mountain region of Chimborazo, in quest of red cinchonas; Mr. Pritchett taking the grey bark forests of Huanuco, in the north of Peru. The perils encountered by these travellers, the hardships they endured, the disappointments they suffered, form a chapter in the history of travel. But illness and privation, bad roads, and even native jealousies left unaffected the general success of the expedition, and though, unfortunately, the plants collected at great risk by Mr. Markham, including many of the best species of Bolivia, perished in the Red Sea in their transit to India, leaving no survivors, it is to the work accomplished by these three enthusiastic labourers that we owe the basis of our present Cinchona plantations. In 1860, the Ootamacund station was established, and the following year the number of young Cinchona trees was reported to be 1,128. Under the excellent care of Mr. McIvor these had been increased in 1863, the date to which I have brought my account of the Java plantations, to 248,166.

It is no part of my purpose to enter into minutiæ of history, nor to do more than associate with the first steps in Cinchona culture the names of Messrs. Hasskarl and Markham, Spruce, and Pritchett as travellers, those of Dr. De Vrij and Mr. John Eliot Howard as advisers in technical details, and more recently, Messrs. McIvor and

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* From the Address delivered at the Pharmaceutical Conference, Bradford, by Henry B. Brady, F.L.S., F.C.S., President.

† My friend, J. E. Howard, F.L.S., to whose kind revision subsequent paragraphs owe any scientific value they possess, tells me that, accurately speaking, these were *C. Calisaya*, and var. *Josephiana*.

Broughton, who have been conspicuous, so far as India is concerned, in the rapid development of the enterprise.

The efforts of our own Government have not been confined to India, but localities have been sought in other parts of the world where natural conditions seemed to favour the chance of success in the introduction of quinine-yielding trees, and at the time I speak of (1863) there were under the care of Mr. Thwaites in Ceylon upwards of 20,000 young Cinchona plants. Jamaica also had made a successful beginning, and the authorities of several European countries were considering how far their respective colonies might be utilised to the same end, though but little decided action beyond what I have stated had been taken.

The ten years that have intervened need not detain us, but having noticed the origin, we will turn at once to the practical aspect of the subject at the present time.

The latest official return places the number of Cinchona trees in cultivation in the Island of Java at two millions.

I can find no published account of the exact extent of the British plantations at the present time. My latest information I owe to the kindness of C. R. Markham, F.R.S., of the India Office. It is contained in the Parliamentary Blue-book of August 1870, and refers only to the Madras and Bengal Presidencies. This gives the total number of Cinchona plants growing on the Neilgherries in January of that year at 2,595,176, of which nearly one-half (1,143,844) were permanently planted out.[*] The number at Darjeeling in the Bengal Presidency in March 1870 is stated at 2,262,210, of which a million and a half were in permanent plantations.

Of the extent of the plantations in Ceylon and Jamaica I know nothing, but reports from time to time state that they are prospering. It is needless to refer to the experiments in cultivation in the south of Europe, the Caucasus, Brazil, the Philippines, or Australia, as these are not yet sufficient in extent to have any practical significance.

The relative value of the bark produced by the various species and varieties of Cinchona is a question that has received close attention, and perhaps cannot be considered settled until something more like uniformity in the subdivision and nomenclature of the genus prevails. Plants regarded as merely varieties of the same species yield widely differing proportions of alkaloids, and the subject is further complicated by considerations as to the possible effects of cultivation and of different climatal conditions. . . . .

The barks now being produced in the Dutch and British colonies are referrible to five species, viz.:—

C. Calisaya, of which, as I have said, only a small proportion realises expectation in its yield of quinine;

C. Hasskarliana (called a hybrid), which appears to be of little value in respect of alkaloids;

C. Pahudiana, deficient in the same particulars, but producing a bark which finds a ready market for pharmaceutical purposes;

C. officinalis, which, in British India,[*] appears to be the most generally satisfactory; and

C. succirubra, which, notwithstanding certain exceptional samples, has not turned out altogether well. . . . .

I can say little about the West Indian plantations as to extent, but the quality of the bark they produce is encouraging. Mr. Howard reports that the chemical examination of barks from Jamaica is "highly satisfactory as regards the prospects of Cinchona culture in that island."

Various questions are still pending:—the influence of manures on the chemical constituents of the trees, the various methods of removing the bark from the tree, and the encouragement of renewal by the processes of stripping and mossing, and many others of like importance, the solution of which must be left to time, and need not occupy our consideration here.

DONATI

SCIENCE, and more particularly astronomy, has recently sustained a serious loss in the death of Prof. G. B. Donati, Director of the Royal Observatory of Arcetri, near Florence, and Professor of Astronomy in the Royal Institution of that city.

On his return from Vienna, where he had represented Italy at the International Meteorological Congress, he was seized by a severe attack of Asiatic cholera, to which in a very short time he fell a victim, dying at his villa near the Observatory, on the morning of the 20th of September last, being only forty-seven years of age. He was born at Pisa in 1826. In 1852 he began his astronomical career at the Observatory of Florence, and by his talents, his attainments, and his indefatigable industry, rapidly gained the esteem and admiration of the learned, attaining a well-merited fame, not so much by the discovery of new comets—among which the most remarkable was that of 1858, to which he bequeathed his name—as by the important observations which he made and published. Of these we need only mention his observations on the study of the spectra of the stars, by which work he successfully inaugurated in 1860 one of the most important branches of physical astronomy, namely, the spectroscopy of celestial bodies.

In 1864 he succeeded Prof. G. B. Amici as Director of the Observatory, after which much of his time and energy were devoted to the establishment of an observatory for Florence and for Italy, which should be completely adapted to the present exigencies of Science, both as regards astronomy and terrestrial physics.

He was in no way discouraged by the serious difficulties of this undertaking, but, inspired by a true love of Science, he overcame them all, insomuch that in a short time, under his active and keen-sighted superintendence, the new observatory was erected on the hill of Arcetri; an observatory which, by the excellence of its position, as well as by the convenience and solidity of its construction, has guaranteed for astronomy and terrestrial physics the most important advantages in every branch of observation.

The observatory was already in working condition, and an important series of observations had been commenced when Science was robbed, by a premature death, of one of her most valued worshippers, who was thus cruelly cut off just as he had entered upon a brilliant career, in which, had he lived, he would certainly have greatly augmented his fame, and shed glory on the Observatory of Arcetri.

Prof. Donati had already commenced a series of notes from the new observatory by the recent publication of

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[*] Since this was written I have received a copy of a return which is believed to represent the actual number of Cinchona trees in the Government plantations in the Neilgherries at the present time. It shows an increase of 12,330 "planted out," and is as follows:—

Crown barks (*C. officinalis*)	508,878
Red barks	579,938
Yellow barks	33,850
Grey barks	28,759
Other species	4,749
	1,156,174

In addition to these it must be recollected that the Government had up to 1870 distributed upwards of 178,000 trees from the Neilgherry nurseries, as well as nearly three hundred ounces of the seeds of various species, to private individuals disposed to plant on their estates. After all, when the experimental stage of such an undertaking is over, private enterprise would seem to be its safest basis. A Parliamentary paper on the progress of India in 1872, just issued, gives the total number of plants in the Neilgherry plantations as 2,639,285, but this probably includes the very young trees still in nurseries. I have no particulars beyond what appear in a paragraph in the *Times*.

[*] This limitation is at present necessary. Dr De Vrij's late paper on Jamaica barks ("Pharm. Journal," August 16, 1873) shows the produce of *C. officinalis* in that island to be very deficient in quinine, inferior indeed to *C. Pahudiana*, whilst a still later communication confirms Mr. Howard's opinion as to the richness of Indian-grown specimens.

some most careful observations of his own on the luminous phenomena of the great Polar aurora of the 4th to the 5th of February, 1872; and we had hoped that other important observations by the illustrious Italian astronomer would, to the great advantage of Science, have been published in the future Notes issued from that scientific establishment.

NOTES

WE regret to have to record the death of two notable men this week. The one is Sir Henry Holland, Bart., M.D., F.R.S., &c., who died on Tuesday, the 28th inst., at the age of 85 years. Sir Henry had caught cold on returning from Paris, which, in spite of his wonderfully robust constitution, proved too much for the veteran traveller. The other is Mr. Albany Hancock, the distinguished anatomist, who died on the 24th inst. He was a medallist of the Royal Society, though not a Fellow. We hope shortly to give memoirs of both men.

SIR ROBERT MACLURE, C.B., so well known in connection with Arctic discovery, died on the 17th inst., at the age of 66.

SIR SAMUEL BAKER was announced to appear before the Geographical Society on Monday first, and give an account of the geography of the country he has lately visited; but we regret very much to hear that illness will prevent him from fulfilling this and other engagements. He has been suffering from inflammation of the lungs.

PROF. FLOWER, we regret to hear, has been compelled to spend the winter in Egypt on account of the state of his health.

DR. J. EMERSON REYNOLDS has been elected Professor of Chemistry to the Royal College of Surgeons in Ireland. The College of Surgeons is to be congratulated on this appointment. Dr. Reynolds will, we believe, still hold his appointment of Keeper of the Minerals and Professor of Analytical Chemistry to the Royal Dublin Society.

MR. JOHN STUART MILL has left his herbarium of European plants to Kew.

WE are informed that the authorities of the Jardin des Plantes, of Paris, have acquired the valuable collection of books on Natural History belonging to the late M. J. Verreaux, and also his private collection of Sugar birds (*Nectarinidæ*), which includes many unique specimens.

IN connection with St. John's College, Cambridge, there will be offered for competition an Exhibition of 50*l.* per annum for proficiency in Natural Science, the Exhibition to be tenable for three years in case the exhibitioner have passed within two years the previous examination as required for candidates for honours: otherwise the exhibition to cease at the end of two years. The candidates for the Natural Science Exhibition will have a special examination (commencing on Friday, December 12, at 9 A.M.) in (1) Chemistry, including practical work in the Laboratory. (2) Physics, viz., Electricity, Heat, Light. (3) Physiology. They will also have the opportunity of being examined in one or more of the following subjects, (4) Geology, (5) Anatomy, (6) Botany, provided they give notice of the subjects in which they wish to be examined four weeks prior to the examination. No candidate will be examined in more than three of these six subjects, whereof one at least must be chosen from the former group. It is the wish of the Master and Seniors that excellence in some single department should be specially regarded by the candidates. They may also, if they think fit, offer themselves for examination in any of the Classical or Mathematical subjects. Candidates must send their names to one of the tutors fourteen days before the commencement of the Examination. The tutors are Rev. S. Parkinson, D.D.; Rev. T. G. Bonney, B.D., and J. E. Sandys, Esq., M.A.

THE Royal Horticultural Society of Tuscany has announced an International Horticultural Exhibition to be held at Florence from May 17 to 25, 1874, and has also issued the programme of an International Botanical Congress to be held on three days during the Exhibition. A very large number of prizes, including 100 gold medals, are offered for collections of plants or single plants, which are included in 248 different classes; and among other objects for which prizes may be obtained are bouquets, botanical drawings, models, garden tools and ornaments, garden structures, manures, herbaria, specimens of timbers, &c. The Congress will be opened by the president, Prof. Parlatore; excursions to the neighbourhood of Florence and the principal gardens will be inaugurated, &c.; and among the subjects proposed for discussion, *inter alia*, are the following:—On the duration of dormant vitality in plants, and on the means of restoring it; on the causes of the movements in leaves; on the acclimatisation of perennial plants; on the analogy between the reproductive organs of flowering and (so-called) flowerless plants; on the general occurrence, or otherwise, of cross-fertilisation, and on the durability of the vitality of pollen; on the nature and functions of the gonidia of lichens: on the nature and origin of Bacteria; on the possibility of establishing rules for a rational distinction between the groups called species, race, variety, &c.; on the value to be set on the determination of fossil plants, &c.; on the character and origin of Alpine floras, and especially on the causes which have limited their extension. The Horticultural Society of Tuscany seem determined to do everything they can to attract visitors, who must send their names to the president or secretary at the Musée Royale de Physique et d'Histoire Naturelle at Florence; and altogether botanists and horticulturists seem likely to have a good time of it.

AN effectual remedy for the devastations committed on the vines by the *Phylloxera vastatrix* is said to have been discovered by MM. Monestier, Lautand, and D'Ortoman, of Montpellier. It consists in placing in the ground, close to the root of the infected plant, an uncorked tube containing about 2 oz. of bisulphide of carbon. The vapour from the bisulphide in a short time permeates the whole of the ground about the root; the vapour is not, like the liquid itself, injurious to the plant, but is immediately fatal to the insect. Care must be taken not to spill any of the liquid on the roots of the vine.

THE following subjects for prizes to be awarded in 1874 have been proposed by the Batavian Society of Experimental Philosophy:—1. To discover if there exists in the molecular state of bodies, modifications other than those caused by temperature, which are such as to give for the same body, different spectra. The Society wishes that this inquiry should bear chiefly on the magnetic condition of bodies. 2. To find out by new experiments if the vapour of water exercises on radiant heat an absorbant effect much more powerful than dry atmospheric air as Mr. Tyndall maintains; or if there exists no difference in this respect between dry and moist air, as M. Magnus maintains. The Society desires that the new experiments which it asks for be conclusive and enable it to decide between the two opinions. 3. To determine what influence the pressure which is put upon an electrolyte has on electrolysis, and how far in this case is the principle of conservation of energy confirmed. It is wished that this inquiry bear on three liquids at least, to be chosen by the competitor. 4. To determine the resistance of the liquid amalgams of zinc and gold to the galvanic current. Six at least of each of these amalgams, in various proportions, ought to be examined. 5. A prize is proposed for new experiments which will enable a certain decision to be come to on the opinion advanced by M. Gaugain as probable, viz. that voltaic electricity is propagated by matter, while induced electricity is propagated by ether.

THE German expedition for the exploration of the Libyan desert is expected to start from Europe about the end of November, and from Egypt early in December, and it is thought that the first reports may accordingly be looked forward to about Christmas. The leader of the expedition is Dr. Gerhard Rohlfs.

FATHER SECCHI, we are glad to see, has received permission from the Italian Government and Cardinal Antonelli to remain at the Royal College of Astronomy.

AMONG the societies concerning which we have received information since the publication of our list last week, is the Working Men's College Field Club, of which Prof. Flower is president. It meets in the Museum of the College in Great Ormond Street, has been in existence only five months, but appears from a reports before us to be in good working trim. It has meetings at which papers are read, courses of lectures by well-known scientific men, and several field-days each month. These field-days seem generally to be Saturday and Sunday, and we only wish that working-men generally put their Saturdays and Sundays to such an excellent recreative use.

WE congratulate the Sunday Lecture Society on the excellent beginning, to be made next Sunday, of their winter course of lectures. Dr. Carpenter, we see, is to give a series of two lectures on the brain; and we think the society ought to consider whether it would not be advisable to have more connected series of lectures than they have hitherto had.

IN a final letter to yesterday's *Daily Telegraph*, Mr. George Smith concludes the account of his Assyrian Expedition. Altogether both Mr. Smith and the *Telegraph* are to be congratulated on the results of the enterprise.

THE following "Science Lectures for the People," are announced to be delivered at the Memorial Hall, Manchester; the Hulme Town Hall being now required for other purposes:—Wednesday, Oct. 29, "Polarised Light," illustrated by experiments in the electric light, by Wm. Spottiswoode, F.R.S., Treasurer of the Royal Society. Nov. 5, "How Flowers are Fertilised," by A. W. Bennett, M.A., Lecturer on Botany, St. Thomas's Hospital, London. Nov. 12, "On Parasites and their Strange Uses," profusely illustrated, by T. Spencer Cobbold, M.D., F.R.S. Nov. 26, "Animal Mechanics," illustrated by experiments with the electric light and the oxy-hydrogen lantern, by S. M. Bradley, F.R.C.S. Dec. 3, "The Senses," by Prof. Croom Robertson. Dec. 10, "On Muscle and Nerve," illustrated by experiments with the electric light and the oxy-hydrogen lantern, by Prof. Gamgee, F.R.S. Dec. 17, "The Time that has elapsed since the Era of the Cave Men of Devonshire," by Wm. Pengelly, F.R.S.

THE French Association, as is known, is to meet at Lille in 1874. Among the many towns which desire to be favoured with its presence in 1875 is Nantes, the Municipal Council of which has already devoted 10,000 francs to defray the preliminary expenses of the session, should it take place there.

ACCORDING to *La Nature* the volcano of Mauna Loa, in Hawaii, is at present in full eruption.

A MICROSCOPIC SOCIETY has recently been founded at Melbourne.

LAST Thursday the whaler *Erik* arrived in Dundee, having on board Rev. W. D. Bryan, who was astronomer to the *Polaris* Expedition; B. Manch, seaman; and J. W. Booth, fireman. All the men were in excellent health. On Friday the *Ravenscraig* arrived at Dundee, having on board one of the boats ingeniously constructed by Mr. Chester, in which the castaways effected their escape from their winter quarters. It is about the size of a whaling-boat, and somewhat similarly shaped.

THE *Journal of the Society of Arts* gives, from the annual report published by the Minister of Public Education, the following particulars respecting education in Italy during the scholastic year 1872-73:—The number of students registered at the Royal Universities was 5,614, and in addition to this number 1,333 persons were allowed to attend the course of lectures, making in all 6,497. At the Universities of Camerino, Ferrara, Perrugia, Urbino, 284 students and 22 non-students, in all 806, attended the course of lectures. At the Royal Institute of high studies at Florence the number of students was 214. The Literary and Scientific Academy of Milan numbered 26. At the Royal School of Application for Engineers the number of students was 173, and at that at Naples 185. The Technical Institute of Milan was attended by 209 students, and the Normal School of Pisa by 41. 295 students were registered at the schools of Veterinary Science of Milan, Turin, and Naples. The royal lyceums are 79 in number, with 4,228 pupils; the royal gymnasiums 104, with 8,462 pupils. In the royal colleges, which are 26 in number, there were 2,208 pupils. The following schools received subsidies from Government:—32 in Piedmont, 67,290 francs; 19 in Lombardy, 49,810 fr.; 10 in Venetian provinces, 16,550 fr.; 24 in Emilia, 52,800 fr.; 14 in Tuscany, 31,200 fr.; 17 in Marches, Umbria, and Roman provinces, 20,800 fr.; 54 in Neapolitan provinces, 90,350 fr.; 5 in Sicily, 6,200 fr. The number of elementary schools throughout the kingdom was 41,713 (being 3,413 more than were opened during the previous year). Of this number 21,353 were for boys, and 16,280 for girls. 33,556 were public and 8,157 private schools. The number of pupils attending those schools during the scholastic year 1872–73 was 1,723,007, showing an increase of 145,853 on the number of the previous year; of this 960,517 were boys, and 762,490 girls. The total number of pupils attending the public schools was 1,545,820, and those of the private schools 177,187. The total number of teachers in these schools was 43,420, being an increase of 3,102 on the number of the previous year. Of these 23,212 were teachers in the boys' schools, and 20,211 in the girls' schools; the public schools being conducted by 34,309 teachers, and the private by 9,111.

WE have received the Catalogue of the publications of Gauthier-Villars, of Paris, for April, May, and June of this year. It contains the publications of most of the scientific societies of France, beside a number of original works in mathematics, physics, engineering, &c., which recommend it to the attention of scientific men. A few more foreign catalogues have also come to hand, which we would recommend to those who wish to know what is being published on the Continent; no doubt the publishers would be glad to send these catalogues to any one asking for them:—Catalog des Antiquar. Bücherlagers von Fidelis Butsch Sohn (Augsburg, 1874, *sic.*); A catalogue of works in Anatomy and Physiology, and Medicine generally, which belonged to the late Dr. Fahle, of Altona (T. O. Weigel, Leipzig); the same bookseller has sent a Catalogue of standard works in all departments of Science.

WE are glad to see that the *Quarterly Journal of Education*, which is shortly to become a monthly, has opened its columns to a correspondence upon questions relating to science-teaching.

WE have received a separate reprint from the "Proceedings of the Geologists' Association" of Mr. D. C. Davies' valuable paper on "The Overlapping of the Several Geological Formations of the North Wales Border."

THE United States Signal Service has recently constructed a telegraph line to the summit of Pike's Peak, in Colorado, which is said to be the highest point reached by any line in the United States, or perhaps in the world. The height is said to exceed 11,000 ft. Regular reports as to the weather are to be sent to Washington three times daily.

THE additions to the Zoological Society's Gardens during the past week include an American Cross Fox (*Canis fulvus*), a Golden Eagle (*Aquila Chrysaetus*), and a Virginian Eagle Owl (*Bubo virginianus*), from North America, presented by Capt. D. Herd; a Mexican Deer (*Cervus mexicanus*), from Porto Rico, presented by Mr. W. Isaacson; two Sand Badgers (*Meles ankuma*), from Japan, presented by Lieut. Hon. A. C. Littleton; a Black-eared Marmoset, (*Hapale penicillata*), from Brazil, presented by Mr. C. Hawkshaw; a Spotted Hyæna (*Hyæna crocuta*), and two Bronze-winged Pigeons (*Phaps chalcoptera*), born in the Gardens; two Rheas (*Rhea americana*), from S. America, deposited; two Chilian Tinamous (*Rhynchotus perdicarius*), three Banded Tinamous (*Crypturus noctivagus*), and two Obsolete Tinamous (*C. obsoletus*), from S. America, received in exchange.

ORIGINAL RESEARCH AS A MEANS OF EDUCATION[*]

II.

IT is the greatest possible mistake to suppose—as, unfortunately, many yet do—that a scientific education unfits a man for the pursuits of ordinary professional or commercial life. I believe that no one can be unfitted for business life or occupations by the study of phenomena, all of which are based upon law, the knowledge of which can only be obtained by the exercise of exact habits of thought, and patient and laborious effort. I dare say many who have had a scientific education make bad men of business, but so do many who have not had such an education; it is not the scientific education which has spoilt them. Even more directly does the value of scientific education bear upon professional and manufacturing life. The medical man's success depends mainly upon the exercise of faculties which are pre-eminently called forth, and strengthened in original scientific investigations. The manufacturer who aspires to something more than following the rule-of-thumb work of his predecessors, requires exactly these habits of mind which are developed by original research. If the brewer, the calico-printer, the dyer, the alkali-maker, the metallurgist wish to make any advance of their own in their respective trades, they cannot do so without the exercise of powers which can only be gained by the prosecution of original inquiry. Doubtless many—nay, even most—of the great discoveries and improvements in the arts and manufactures may have been made by men who have been self-taught. But these men have acquired for themselves, by slow and difficult steps, the same habits of exact observation, patient and laborious devotion, and manipulative or constructive skill which the modern student of science may, at any rate to a very considerable extent, gain in his college course. So valuable is this kind of education found to be, that in Germany, where it is most practised, the chemical manufacturers now refuse to take young men into their works unless they have not merely had a scientific education, but also have prosecuted original investigation.

If, then, education in its widest sense has for its objects, as I presume will be generally allowed, the training of the mind and faculties in such a way as most fully to qualify the possessor to discharge with benefit to mankind his duties in after-life, surely plans for the encouragement of original scientific research should form no inconsiderable portion of the work of every institution professing to deal with the higher education of the country. And yet when we come to look at the provision made for encouraging original research, either at our older or at most of the more modern seats of learning, we are astonished to find that this essential provision is almost altogether ignored. At Oxford and Cambridge thousands of pounds are each year lavished upon the encouragement of classical and mathematical attainments, whilst the claims of original research can scarcely be said to be recognised. Hence these highly endowed universities, whilst they are justly celebrated for their critical faculties, have ceased to represent, in any one direction, the productive power of the country.

Original research, the true life-breath of civilisation, does not in England, as is the case in Germany, look to the universities as the nurseries where its young shoots shall be tended and cherished, for there, at present, its value is scarcely recognised. Indeed, Sir William Thompson has expressed his opinion that the system of examinations at the universities has a tendency to repress original inquiry, and exerts a very injurious effect in obstructing the progress of science. The time is, however, not far distant when this want of appreciation of the value of original research will be a thing of the past, and when the universities will vie with each other in encouraging this mainspring of progress, and in honouring more those whose lives are devoted to this high calling. Owing to the want of means of promoting original investigation in our great seats of learning, the scientific activity of the country has found vent through other channels. No want of encouragement can repress really great minds or powerful wills. Manchester can boast the names of many men who, in spite of want of university aid, have done much for science. Who, for instance, in the whole scientific annals of Oxford, can be placed on a footing of equality with Dalton or Joule? These men are, however, great in spite of our systematic negligence of the subjects, the mastery over which has made their names immortal.

If, in the face of so much that is discouraging in this want of recognition of science, England has still no reason to fear the comparison of her great men of science with those of other countries, we may feel sure that our position among the nations will be raised when the Government, our universities, and the country at large become alive to their duties as regards the encouragement of original scientific research, and when the number of able men who devote themselves to this pursuit shall thereby be largely increased. Much assistance in this direction may confidently be expected from the Royal Commission on Scientific Instruction and the Advancement of Science, of which his Grace our President is chairman, and which has lately published its third report on the progress of scientific education and research in the two old universities. In this report, the importance, from a national point of view, as well as an educational instrument of original research is fully recognised, whilst the means of enabling the universities to take their due share in the management of this branch of human activity is suggested. The evidence given before this Commission by Sir Benjamin Brodie, Prof. Frankland, Dr. Carpenter, and other competent authorities, is of the most decided and unanimous character, and the opinion thus strongly expressed must ere long produce its effect.

The importance of fostering scientific research in connection with higher education is, however, now well understood to the authorities of this college. Very considerable facilities for carrying out original work are given both to the teachers and to the pupils, whilst in the appointment of the professors special weight is always laid on their power of conducting scientific research. In my department, which has now been organised for many years, I make bold to say that we have not been behind any chemical laboratory in this kingdom in the original work we have produced. The physical laboratory, which has only recently been inaugurated, has already, under the care of its talented Director, whose original researches are valued wherever Science is appreciated, done valuable work, and the new department of practical physiology which has just been established will doubtless soon bear fruit of a similar character. In the biological sciences our teaching resources have hitherto been limited; but although this has necessarily prevented the prosecution of research by the students, the professors of this department have long been distinguished for original investigation in their special branches.

To assist in developing in the practical community the appreciation of scientific research, and owing to the liberality of Manchester men and to the wise advice of Prof. Frankland, who then occupied the chair which I have now the honour to hold, a scholarship for original chemical research—our Dalton Chemical Scholarship—was founded in 1853 as a testimonial, and a fitter one could not have been proposed, to our great townsman. The establishment in England of a scholarship for excellence in original research was, twenty years ago, a circumstance without a parallel, but in spite of the novelty of the experiment, time has fully proved the wisdom of the course which its originators adopted. We can already point to a fairly long list of men who have taken our Dalton Scholarship, who now hold high and responsible positions in scientific, manufacturing, and official life; and these men will all acknowledge the benefit conferred upon them by the training they received when competing for the scholarship, and whilst occupied for the first time in their lives in carrying out an investigation on some original subject.

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[*] Concluded from p. 539.

On the model of our Dalton Chemical Scholarship, an important physiological scholarship has lately been founded in this College by Mr. Robert Platt; the conditions of tenure involve the prosecution of an original investigation in physiology; and it is to be hoped and expected that this scholarship will do as much to stimulate the study of physiology amongst us as the Dalton has certainly done in the case of chemistry. The establishment of similar scholarships in the branches of physics and biology is much to be desired, and benefactions made for these special purposes will assuredly prove of the greatest value.

It is unnecessary for me to point out the direct applications which the knowledge and experience gained in the laboratory receive in the arts and manufactures dependent upon chemical science. These everyone can see for himself. The ordinary routine work of the alkali maker, the dyer, the brewer, the calico printer, calls immediately for chemical knowledge, and manufacturers who do not yet see the value of the training afforded by original experimental investigation, are ready enough to appreciate chemical knowledge if it can show them that their drugs are adulterated or their water impure.

Concerning the exact mode by which encouragement should be given in this country to original research, opinions may differ. One proposal has lately been made by the distinguished president of the British Association (Prof. A. W. Williamson), in his able address at Bradford, which it behoves all interested in the progress of the country carefully to consider. Without attempting to discuss the details of this or other schemes, it may be well to point out those general features of the subject upon which these proposals are based.

In the first place, then, we shall agree that the measures which have to be taken must be systematic, must apply to the country at large, and must include all classes. What we need is the development of the latent intellectual resources of the country as regards science, the means of sifting out from the great mass of the people those golden grains of genius which now too often are lost amongst the sands of mediocrity. This can only be fully accomplished by a system extending from the lowest primary schools up to the highest educational establishments in the land, and therefore almost necessitates the action of Government. But whilst believing that a national system is needed in order that the potential scientific energy of the country shall become active, I for one should most strongly object to the establishment of a complete system of State education. One of our greatest safeguards and sources of national strength has been and is the freedom from Government control which our educational, municipal, and local institutions have always enjoyed; and the evils of a uniform State system as existing in France (which is such that the Minister for Education remarked with pride, that at a given moment the classes in all the Lycees in France were engaged in reading the same chapter in Cæsar's Commentaries) need only be felt to be deplored.

Secondly, it is clear that in order to be able to select from amongst the people those whose mental and physical powers fit them for ultimately advancing science themselves, the rudiments of a scientific training must be much more widely diffused than is at present the case. This can only be slowly accomplished; the methods of teaching science are only beginning to be understood, and, unfortunately, in school teaching the introduction of a scientific subject has too often been looked upon more as an amusement than as a study requiring as much or more attention and exactitude than the older subjects, one which when properly taught acts to quite as great an extent as a mental discipline. Science teachers have yet to be trained, and a system of introducing elementary science as disciplinary teaching into primary and secondary schools has yet to be made general. At the same time new institutions have to be founded in which the higher branches of the various sciences are taught and original research encouraged, and into which youths of conspicuous merit must be drafted, whilst existing colleges and universities have to be modified to suit the requirements of the time. These institutions must contain laboratories, not only for teaching purposes, but suited for scientific research, and the professors must take in a certain number of advanced students to work on original investigation. This is indeed, as Sir Benjamin Brodie points out in his evidence before the commission, an educational function of the most important character; because here scientific education is carried out to its end, and if this is not done, you stop short of the most important part of all m scientific education, for the perfection of science as a means of education is seen only in scientific inquiry. The pupils thus trained eventually pursue science as their main business in life, and become in their time teachers and professors of their subject. Thus by degrees the profession of the investigating teacher will become recognised as one in which the ablest of our youths may obtain reward and recognition, as well as satisfaction and delight, and thus the scientific power of the country will be vastly increased.

Concerning the ennobling nature of original scientific inquiry it is needless for me to say much, for although I should be the last to contend that men of science are free from the foibles and weakness common to all mankind, I think it stands to reason that the habits of mind which an investigator must cherish, are such as must raise him above the petty struggles of ordinary existence, and must, for a time at least, lift him into an atmosphere free from the cloud and smoke which too often darken the usual current of men's lives. In order to give you an idea in what original research consists, and to point out to you the interests attaching to an inquiry, the practical applications of which seem as far distant as those of a newly-discovered planetoid, I will for a few moments draw your attention to a case of the kind with which I happen to be familiar. Amongst the sixty-three different elements of which the earth, so far as we know, is made up, there are many which have been found only in the most minute quantity. Indeed, in the list of elements suspended on the wall, you will notice that a large number out of the sixty-three are marked as rare. A few only of these substances are employed in the arts and manufactures, or are known to play any part in the economy of nature; the rest are rarities of interest at present only to the scientific chemist. It would, however, be presumptuous on our part were we to assume that the existence of these bodies is a matter of no moment, for we are constantly learning that substances hitherto supposed to be useless are of the most vital importance. Hence it is obviously our duty to get to know all we can about the properties of each, even the rarest, of these elementary bodies, and especially about their relation to, and mode of action on, the other elements. It is clear, too, that as long as our knowledge of the properties of any one of these elementary bodies is inaccurate, or if mistaken views regarding any one have arisen, our science must suffer in completeness. For just as an error made in the basement of a house throws the upper storeys wrong, so a mistake concerning the size and shape of the foundation blocks of our science may render the whole chemical superstructure faulty.

In 1830 the great Berzelius fully examined a new elementary body termed vanadium, the existence of which had been previously discovered by his countryman Sefstrom. Having most carefully ascertained the remarkable properties of this new substance and its compounds with the other elements, Berzelius gave to vanadium and its compounds a certain chemical position and place amongst the other elements. Thus to the compound of vanadium and oxygen containing the largest proportion of the latter element, and called vanadic acid, he assigned the formula V_2O_3, meaning thereby, in the atomic language of our great townsman Dalton, that two indivisible particles or atoms of the metal are combined with three indivisible particles or atoms of oxygen; and these views, enforced by experiments of the most unimpeachable character, were for years universally adopted by chemists.

In 1858 a fact was observed by the German chemist, Rammelsberg, with regard to the crystalline form of the best known mineral containing vanadium which exhibited Berzelius's conclusions in a new light. It had long been known that substances which have an analogous chemical composition are found to crystallise in an identical form. Thus the different alums containing alumina, oxide of iron, oxide of chromium, oxide of manganese, all crystallise in octahedra, and the oxides contained in these alums have all an analogous composition; that is, the relations between the number of atoms of metal and of oxygen in each case is identical. Now, Rammelsberg found that the crystalline form of a mineral contained vanadic acid, and lead was identical with another mineral containing phosphoric acid and lead. Hence we should expect to find that the oxide of vanadium, termed vanadic acid, and the oxide of phosphorus, called phosphoric acid, possess an analogous chemical constitution. Such, however, was found not to be the case. Phosphoric acid is well known, and, without doubt, consists of two atoms of phosphorus, united with five atoms of oxygen, whereas Berzelius only found three atoms of oxygen to two of the rare metal in vanadic acid. How is this discrepancy to be explained? We have here to do either with an exception to the otherwise general law of isomorphism, so that we may have identity of crystalline form, without any analogy in chemical composition, or Berzelius's experiments and conclusions respecting the constitution of this vanadic acid are incorrect. By experiments on the properties of vanadium and its compounds, made with much larger quantities than it fell to the lot of the Swedish chemist to work with, it was shown that something had been overlooked by him. It was proved that the substance which he supposed to be a metal was not a metal at all, but an oxide, and that vanadic acid really contains more oxygen than he believed it to contain. And what is remarkable is that this quantity of oxygen, which had been overlooked, is exactly the quantity which is needed in order to make the constitution of vanadic acid identical with that of phosphoric acid. We have to take out of each atom of Berzelius's metal one atom of oxygen in order to get the true vanadium, so that the real atomic weight of this element is less than that given to it by Berzelius by the atomic weight of oxygen, 67·3 − 16 = 51·3. Thus the chemical constitutions of phosphoric and of vanadic acids are represented by the formulæ P_2O_5, V_2O_5. The law of isomorphism remains unassailed, and the goddess (Vanadis is a cognomen of the Scandinavian goddess Freia) who was found wandering as a waif and a stray amongst her companion elements, has been restored to her natural friends, and now forms a recognised member of a family group.

To sum up, my aim in the foregoing remarks has been to show that if freedom of inquiry, independence of thought, disinterested and steadfast labour, habits of exact and truthful observation, and of clear perception, are things to be desired as tending to the higher intellectual development of mankind, then original research ought to be encouraged as one of the most valuable means of education. And that on this ground alone, and independent of the enormous material benefits which such studies confer on the nation, it is the bounden duty not only of the Government, but of every educational establishment, and of every citizen of this country who has the progress of humanity at heart, to promote and stimulate the growth of original research amongst us.

HELVETIC SOCIETY OF NATURAL SCIENCES

THE fifty-sixth annual meeting of this society was held on the 18th, 19th, and 20th of August last, at Schaffhouse, under the presidency of Dr. Stierlin, and is described as having been a highly animated one. We shall note a few of the more important papers presented; for particulars of which we are indebted to the *Archives des Sciences*.

In the section of Physics and Chemistry, M. Soret described a method for studying ultra-violet spectra. It consists in placing a thin fluorescent lamina (sulphate of quinine, *e.g.* between two glass plates) before the eyepiece of a spectroscope, where the image is formed, and observing, with sufficient inclination of the eyepiece the image of the ultra-violet spectrum then developed on the lamina. Prof. Kopp read a paper on bromline and its derivatives. The Deacon process of manufacturing chlorine was the subject of a paper by M. Hurter, which gave rise to lively discussion. Dr. Heim, who has been observing the sounds of cascades, find they all give the note C sharp, or F.

In Geology, Dr. Schalch had a paper on the volcanic rocks of Höhgau. These are in two groups, that of basalts, and that of phonolites. They form isolated cones surrounded with thick deposits of volcanic tufa, the nature and arrangement of which indicate that the eruptions happened at successive intervals about the end of the tertiary epoch. M. Favre showed a section of the Vaudois Alps made at Pleiades, near Vevey aux Ormonts; in which he distinguishes three zones, consisting of superior Jurassic and Neocomian, and different portions of Eocene, strata. Dr. Heim exhibited a new method of geological representation of a country; it consists in a series of sections, on the same scale, coloured and fixed vertically at equal distances on a geological map. He also made some observations on the zone of contact of crystalline rocks and sedimentary strata in Eastern Switzerland and the Bernese Alps. M. Lang announced the early publication, by the Alpine Club of a glacier-register, in which information will be given as to dimensions, form progress, &c., of glaciers. At the first general *séance* Prof. Heim gave a valuable *résumé* of the various theories of glacial motion. At the second, Prof. Desor presented a memoir on *morainic* landscapes, by which he denotes those indicating a former extension of glaciers. The most striking types are at the southern base of the Alps. There is discernible a zone consisting of a succession of verdant hillocks, sometimes aligned, sometimes separate; these are found to be composed of the *débris* of old formations bruised and triturated, and clearly indicating glacial action. A good example occurs at the base of Monte Campo di Fiori.

At the general opening *séance* Prof. Forel gave an account of his researches on the deep-water fauna in Lake Leman, of which he enumerates some thirty species. He had also studied the fauna of the lakes of Neuchatel, Zurich, and Constance. His conclusions are briefly these:—There are in the lakes three distinct faunæ: (*a*) a littoral, extending to 15 or 20 metres depth; (*b*) a deep fauna, from 20 to 300 metres; and (*c*) a pelagic fauna. All the forms of the deep fauna have analogous or similar forms in the littoral fauna; but the converse does not hold, At the same level the deep fauna are the same. A few species found between 30 and 100 metres are not found at 300 metres. but all the types at 300 metres are found between 30 and 100 metres. There are local and seasonal differences. The deep fauna are best studied between 30 and 60 metres. In comparing different lakes the general characters of deep fauna are the same, but special characters vary.

In the section of zoology and botany. M. Bugnion described some sensitive organs found in the epidermis of Proteus and Axolotl. They are considerably developed in the former (1400 were counted in one specimen), and are disposed in linear groups of three or four along certain nerves of the head, and the lateral nerve to the end of the tail. They resemble the cyathiform organs discovered by M. Leydig in 1850, in the epidermis of fishes. Dr. Cartier gave a paper on the sensitive hairs of crocodiles.

In the medical department Prof. Karsten, of Vienna, made a communication on *necrobiosis* in which he pointed out that Bacteria, Vibriones, and micro-coccus, &c., are not to be regarded as organic species, properly so called; the phenomena of animal reproduction have never been observed in them. They are pathological products, which grow in the interior of vegetable or animal cells, but which do not penetrate these when once developed, as parasites

In the department of Pure Mathematics the principal paper was by Prof. Schwarz on a new example of a continual function which does not admit of derivatives. This paper will be found *in extenso* in the *Archives*.

This is the third time in its history that the Helvetic Society has met at Schaffhouse, the former occasions having been in 1825 and 1847. The next annual session is to be held at Coire.

SCIENTIFIC SERIALS

Sitzungsberichte der Königl. Böhmischen Gesellschaft der Wissenschaften in Prag. Jan. 1871 to June 1872. (3 numbers).—Among the more valuable matter in these numbers may be noted some contributions to palæontological botany; more especially a paper by M. Feistmantel describing the various fruit-forms met with in Bohemian coal formations. (As published separately, the paper contains several excellent plates). The same author communicates also full accounts of the flora in coal-measures at the foot of the Riesengebirge, and at Merklin.—M. Dvorak describes some curious experiments on individual differences between the two eyes, and between different parts of the retina of the same eye. He shows that two non-simultaneous impressions, each affecting one eye, appear simultaneous, when the time-interval is of a certain length; this interval he measures with suitable apparatus.—In chemistry we have a note by Prof. Stolba, giving a new method of preparing borofluoride of potassium, and an account of the properties of this substance.—Dr. Weyr investigates mathematically the distance-action of electrical solenoids on material plane surfaces; and a note by M. Domalip furnishes experimental proof of certain laws deduced by M. Dub as to the dependence of magnetic moment on the dimensions of a magnetic bar.—There are also papers on the fauna of lakes in the Bohmerwald, on basaltic formations, and on several points in mineralogy and pure mathematics.

Bulletin de l'Académie Royale de Belgique, No 8, 1873.—In this number is described a recording *meteorograph*, devised by M. Van Rysselberghe, and which seems to have some merit, the advantage being that the readings of several different instruments can be recorded by means of a single steel graver, making traces on a varnished copper sheet. The sheet is fixed on a vertical cylinder, which rotates at equal intervals (*e.g.* every ten minutes); an electric circuit, of which the instrument to be observed forms part, is closed by the movement of the cylinder; this liberates the graver, which then gives a tracing proportional, in length, to the indication of the instrument. At each revolution the graver descends a little; thus a series of equidistant lines are obtained, the extremities of which form the curve of observations. The copper sheet is afterwards dipped in an acid and thus made ready for engraving.—M. Terby communicates some drawings made by M. Schroeter, in the end of last century, which show the configuration of the spots of Mars at that time. He finds, in these, fresh proof of the permanence of the spots.—A letter from Prof. Genocchi, of Turin, on several mathematical questions, calls forth a long report from M. de Tilly with reference to the alleged impossibility of demonstrating the postulates of Euclid by plane geometry, or by any geometrical reasoning.—We further find notes on the congelation of alcoholic liquids, (Melsens), on the motion of projectiles, on hypo-sulphurous acid, on some storms at Aartselaer in July, and other topics.

Bulletin de la Société Impériale des Naturalistes de Moscou, No. 1, 1873.—In this number there is a valuable paper of spectroscopic solar observations in 1872, by M. Bredichin. Four plates are appended, showing the spectroscopic profile of the sun from July 22 to September 10. The author's results confirm, in the main, those of Secchi.—M. Berg gives some particulars as to the successful acclimatisation of a Japan silkworm, the *Antheræa Yama Mayu*, in the Baltic provinces. Cultivators were looking in this direction partly because of the difficulty of acclimatising mulberry in the north; the new animal feeds on oak leaves. One striking fact is, that some of the eggs were exposed, at times, for three days successively, to a temperature of 12° R., without apparent injury. The temperature at which the worms were kept after leaving the egg till spinning time, varied between 12° and 16° R. The entire extra-oval life of the Yama Mayu in Riga is about 16½ weeks; or 9 in the caterpillar, 6 in the chrysalis, and 1½ in the moth stages respectively. Experiments, extending over three years, have fully shown that the scheme in question is a practicable one. We have further to note a long and interesting account, by M. Wolkenstein, of certain ancient cemeteries named "Jalnikis," found on many of the hill-sides in Novgorod. The tombs are made of unhewn stones arranged in form of a rectangular cyst, which contains the skeleton. In his study of the question whether these cemeteries belonged to ancient Novgorodians, or some other people, the author is led to assign a Slavic origin.—Among the remaining papers are a note by M. Stepanoff on the development of Calyptræa, and a reply by M. Lubimoff to M. Bredichin.

Reale Istituto Lombardo di Scienze Lettere Rendiconti Fascicolo, XV, 1873.—In addition to a large quantity of historical and philosophical matter, which includes a fourth paper on Kant's philosophy, by C. Cantoni, this number contains observations of Comet II, 1873, by S. Tempel; a long paper on the polymorphism of *Pleospora Herbarum*, by Drs. Gibelli and Griffini; and also some anatomical and medical notices.

THE *Annali di Chimica applicata alla medicina* for September contains the usual number of notices on pharmaceutical preparations, &c.

American Journal of Science and Arts, October.—This number contains a description of some valuable improvements in the silt analysis of soils and clays, by Mr. Hillgard. From minute observations on the working of the elutriating apparatuses of Nobel, Schulze, Fresenius, and others, he concludes that all determinations hitherto made with conical vessels are vitiated by irregular currents, and a kind of miniature avalanche formed by the particles. He employs a cylindrical elutriating tube, having a rotary churn attached to its base, but screened by wire from the liquid column. This has given good results.—Prof. Dana has a (continued) paper on the quartzite, limestone, and associated rock of the vicinity of Great Barrington, Berkshire Co., Mass.—Mr. May describes some experiments on the determination of lead as peroxide, and Mr. Remsen communicates a note on isomeric sulpho-salicylic acids.—Mr. Bentham's anniversary address to the Linnean Society is given; also a French Academy notice of Dr. Verneuil, who did valuable service to North American geology.—We further note accounts of various survey operations in Colorado, Sierra Nevada, Utah, &c.

Atti della Realle Accademia dei Lincei, Roma, Dec. 187 This publication contains, among other papers, an interest description, accompanied with plates, of certain human bou found in a remarkable state of preservation in a cemetery at Ferentillo. The authors, MM. Maggiorani and Moriggia, made analyses of the soil, which abounded in salts of lime having, of course, avidity for water. The ground was porous, and readily permitted passage of vapour from one stratum to anoth Scarcity of humus and good ventilation were other favour causes. There was a popular tradition that the soil was broug from Palestine, but this is thought incorrect. The mummie were throughout invaded with sporulæ and various other para sites which doubtless contributed to the mummification.—A lo paper by M. Volpicelli offers a complete and general sol through the geometry of situation, of the problem relating t course of a horse over a checkered surface.—Prof. Cantoni article on the various modes of electrical testing (*esplorazion* on the influence of hypothesis in electrostatics; in which he ma some strictures on certain passages in Tyndall's little work o Electricity, referring to the existence of two fluids.—We furthe notice a paper by Prof. Cadet on the functions of the white nerve substance, and one by Prof. Respighi on the shower of falling stars observed November 28, 1872.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Oct. 20.—M. de Quatrefages, president, in the chair.—The following papers were read:—Theory of the movement of a point attracted towards a fixed cent by M. J. Bertrand.—On Dr. Reye's explanation of the sol spots, by M. Faye. Dr. Reye considers that the heat of facula causes an up-rush and expansion of the superincum atmosphere, causing a sort of vortex through which the m of lower strata rise, expand, cool down, and condense. M. after explaining the theory in question, argued that a very s fact overthrows it at once. Dr. Reye's theory would make the vortex or spot *on* the sun, while the measurements of Carrington have shown that it is really *in* the sun.—Anatomical researches on the tardigrade *Edentata*, by M. P. Gervais.—M. Alph. de Candolle presented the last volume of the "Prodromus Systematis Naturalis Regni Vegetabilis."—The secretary reported on a number of papers on the *Phylloxera*.—Researches on an easy method of measuring the capacity of ships, by M. d'Avout.—Additional note to the monograph on the fish of the family of the *Symbranchidæ*, by M. C. Dareste.—On the production of galls on vines attacked by the *Phylloxera*, by M. Max Cornu.—On the reproduction of the oak *Phylloxera*, by M. Balbiani.—On the production of certain crystalline borates in the dry way, by M. A. Ditte. The paper in question described several borates of barium and magnesium, and also several double salts of the same class.—Note on the chlorovanadates, by M. P. Hautefeuille.—On the production of methylamines in the manufacture of pyroligneous products.

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